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Comments on "Are personality differences between twins predicted by astrology", S. Ertel and G. Dean

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Summary—Apart from a poor understanding of the spirit and technique of the experiment, the following points should be noted. (1) The authors claim that in 50% of the cases, the "traits (are) added or amended by SFB after the event". This is a false statement. (2) Clash with previous findings: this is not a valid argument. Scientific advances are frequently unexpected. (3) Effect size: the value obtained by the authors is based on an arithmetical operation without explanation or value. (4) The "stereotypes" bias is irrelevant because zygocity was only known after the psychological phrases were sent to the families. (5) The "response set" bias does not seem worthy of retention; a new test has been presented which confirms the technique used. (6) The English replication is negative, but it was carried out under apparently inadequate conditions. A French replication will be undertaken at a later stage. *Note*: I am the author of 150 articles published in scientific journals. Copyright © 1996 Elsevier Science Ltd.

A certain number of comments need to be made on the text by S. Ertel and G. Dean. These can be classified under the following six headings:

DATA MANIPULATION

The authors claim that in 50% of the cases, "traits (are) added or amended by SFB after the event". This is a false statement used by them to destroy the scientific value of the work.*

REASONS FOR DOUBT

It is unusual to begin an article by putting forward "reasons for doubt" which should be discussed in the conclusion. The "clash with previous findings" are not reasons for raising doubts but interesting factors for those with an open mind.

CICHETTI'S EFFECT SIZE

Cichetti (reference of the authors) contests the use of the notion of statistical significance. He points out, after others and in keeping with conventional theoretical results, that a similar deviation λ may be non-significant to a given threshold α for a value of N (number of observations). However, if N increases, he deduces the necessity to avoid an artefact and therefore to distinguish between statistical significance and what he refers to as "real significance" (substantive, practical or clinical significance).

It is strange that this author does not specify what he means by "real significance", even though, on the contrary, the definition of statistical significance is known: when a difference λ is statistically significant, it is because it is not acceptable to attribute it to chance (intervention of random factors). Under these circumstances, one cannot consider the problem treated to be clearly expressed, even if Cichetti asserts that when N is sufficiently large, only trivial differences (this expression is not defined either) can be statistically significant. Despite the vagueness of this problem, Cichetti proposes to apply himself, in the course of parapsychological experiments, to what he calls "size of the effect" (delta), but this too he omits to define, contenting himself with writing while studying the numerical values obtained during an experiment carried out by Schmidt in the course of which success had been obtained in PO = 26.10% of the tests while chance only attained PC = 25%: "the size of our effect (delta) can be conceptualized as 1.1% above mean chance expectation". It will be noted that the designation "mean chance expectation" is perhaps not very precise since it refers to a percentage (and therefore a frequency).

In this context, Cichetti suggests using what he acknowledges to be a metamorphosis of a classical statistic, that is to say, kappa = (PO − PC)/(1 − PC) whose value, according to him, constitutes a measurement of "chance-corrected size effect" (definition not provided) which could be interpreted, when it is statistically significant, in terms of "real significance" on a scale proposed by him in which this "real significance" is mediocre (non-defined) for kappa < 0.40.

* It is not easy to understand why, in all the text of the authors, "void" responses are counted "as half a hit", which is not the case in my paper.

The authors (S. Ertel and G. Dean) obtain here kappa = 0.37, then the " disattenuated effect size" that they find equal to 2. They conclude that SFB's results are absurd since the "effect size cannot exceed unity". It will be noted that the authors do not give the definition of "disattenuated effect size" (even though they quote Cichetti concerning kappa) and, what is most strange, they content themselves with indicating that they calculate "in the usual way" by writing an arithmetical operation in full *but without providing the algebraic expression. Moreover*, apart from kappa = 0.37, *they use estimated numerical values for this calculation, without these estimates being in any way clarified or justified. Quite the contrary, they even express themselves in a manner which could lead one to assume that these estimates have a subjective character, at least in part.* It therefore seems necessary to bear in mind the evidence, namely, that it is always possible, in the course of a calculation, to obtain an absurd result (according to the term used by the authors) by using well chosen numerical values.

STEREOTYPES

(a) The entire study is based on the reduction of the psychological traits used in my work to 54 different variations. However, this evaluation distorts any analysis as my work used not 54 but 184 distinctive phrases (computerized calculation). Consequently, the results of this study cannot be considered accurate, which already leads one to suspect that the conclusions are not well-founded.

It should also be stressed that the protocol using classification of cards by students does not necessarily produce information on popular "stereotypes" but can quite conceivably lead to real psychological knowledge.

(b) The analysis undertaken in this manner prompts the authors to maintain that I wrote the psychological phrases under the influence of "stereotypes" relating to the order of birth, sex or zygocity of twins.

While the order of birth was indeed the basis for analyzing the two birth charts of twins, zygocity, on the contrary, could under no circumstances have been a determining factor. In fact, with the exception of my two nieces and two families of friends, I was always unaware of zygocity when writing out the phrases.

In the book written in French (Astrologie: la preuve par deux, published by Laffont, 1992), it is clearly demonstrated that I received information on zygocity only in the second form containing the replies of the families to my psychological phrases. This book was sent to S. Ertel as soon as it was published. On the contrary, in the article in *Personality and Individual Differences*, there is a difference: "in the first instance, each family received a preliminary form ... were required to say whether the twins were mono- or dizygotic". Why? Quite simply because of an error made by the translator which I had not corrected: I had no idea that this could have been of any importance. The proof that I was unaware of zygocity when I wrote the phrases was submitted to the Editor-in-Chief by sending copies of all the forms addressed to the families. Contrary to what the authors have written, I replied to S. Ertel by sending two forms by way of example, which I thought would be sufficient, given my qualities as a scientist and author of 150 scientific articles.

As for gender, I wish to point out that I analyzed the charts without looking at the first names: gender is not taken into account in astrology, neither the "easiest-to-say" quality of the two first names which may not have been a source of bias by the families.

(c) The article by the authors includes quotations from works on the numerous family influences of twins. There appears to be very little relationship between complex family interactions, built up at length, and the simple formulation of psychological phrases under the effect of the so-called "stereotypes". Furthermore, the conformity of the "stereotypes" between myself and each family seems too unlikely to have produced the scores obtained in the experiment.

RESPONSE SET AND EXTRACTION OF THE PSYCHOLOGICAL PHRASES FROM BIRTH CHARTS

As I wrote in *Personality and Individual Differences*: "In order to strengthen the validity of the response, the two psychological statements are systematically presented in the following manner: the psychological phrase relating to the first twin is placed on the right, that of the second on the left" (p. 1138). In my opinion, "the exact response will thus correspond to an additional, conscious, mental effort, as requires inversion in chronological order of time of birth of both twins at the beginning of the form". Indeed, I believe that the motion of writing from left to right in our languages induces an unconscious tendency on the part of the undecisive families to use the left box for the first twin, and the right box for the second.

The authors criticize this approach, since a phenomenon of reactance, according to them, can lead to the reverse tendency: right before left.

To test my thesis more thoroughly, I carried out an experiment with 109 students of both sexes, selected at random from the campus of the University of Paris-Sud-Orsay. It involved handing them a sheet with two short and insignificant phrases A and B, written one under the other, and asking the subjects to write these two phrases in the boxes on the left and on the right of the same sheet.

Sixty students received sheets with phrase A placed above B, the 49 others with phrase B placed above A (phrase A = it's a nice day, phrase B = the train is leaving).

The replies in the left box were as follows:

First case: A above B
—50 A
—10 B

Second case: B above A
—33 B
—16 A

thus giving a total of 83 replies with the first phrase transposed in the left box, that is 76.14% of the total, and only 26 with the second phrase in the same box. This implies that more than three out of four persons follow the tendency I used in my protocol, and that the reactance phenomenon is a minor one which can in no way account for the scores obtained in my experiment (copies of this experiment were sent to the Editor-in-Chief of *Personality and Individual Differences*). It would evidently be a conclusive exercise to reproduce this test in a language written from right to left, as in Arabic for example.

Nevertheless, a randomization of the phrases attributed to each twin is conceivable but not indispensable, and may even be harmful.

The authors assert that in many cases, I am unaware of the planet close to the angles and that in others, one or several traits are not extracted from the chart; this is impossible. The difference arises out of the fact that the authors have not analyzed the charts by hand but tried to do it by computerization. It is clear that in the 238 × 2 cases, the extractions are not all simple. It can happen, for example, that several sky elements together are situated in the "angles": a choice then becomes necessary which cannot be handled easily by a computer. In other cases, as I pointed out (p. 1138), "sometimes both methods 1 and 2 are used simultaneously if this is necessary to obtain more information". Of course, all this requires a good practical knowledge of astrology such as I have acquired after 20 years' study, which does not apply to one of the two authors. (Personal oral communication by S. Ertel in London in November 1993: I have not studied astrology).

Admittedly, a computerization of the extraction of the psychological phrases is theoretically feasible, but it would certainly be a tricky task and would entail working with a much larger population.

ENGLISH REPLICATION

The authors fail to say that for a large part of this replication, M. O'Neill and I decided to proceed with a rating of the extracted phrases by the former in the English replication already carried out, compared with the phrases that I myself extracted from the same charts.

I was in complete agreement with only 10% of the cases. For this population, I noted that 40% of the phrases, which I considered to be incorrect, corresponded to "reverse" replies, while 46% of the exact replies corresponded to correct or nearly correct phrases. There is therefore a genuine problem of methodology, obviously aggravated by a problem of translation: I tried to help M. O'Neill by analyzing some pairs of twins which was then translated into English, but the result was not better. The translation is certainly at fault for the key words published in *Personality and Individual Differences* were translated quite differently by the various English translators I had contacted. There are some other varying factors compared to the experiment carried out in France: I have always received replies from the families within a short period (an average of one week), which is not the case in England. Moreover, the English twins used frequently come from astrological references which is not necessarily an advantage: families are no longer "naive", as they should be for this experiment.

The next French replication which I will attempt in the future will no doubt provide further details, but this will not remove the need for good replications in foreign countries.

CONCLUSIONS: COMPREHENSION OF THE PROCEDURE AND GENERAL ATTITUDE OF THE AUTHORS

The authors assert that the astrological logic is violated by my choice of twins as subjects and the difference between two personalities covaries with the difference between the corresponding astrological chart. These are preliminary assertions that run counter to the—poorly understood—spirit of my work. On the contrary, the choice of twins represented a new means of testing the astrological hypothesis (at least partially) as long as one was not confined to simplistic considerations. For example, the logic of the authors leads to the notion the "DZ twins differ more than MZ twins, the difference between their astrological charts should tend to be larger" which is obviously erroneous, since the intervals between births have nothing to do with zygocity.

The genetic potential is not denied by astrology (the 10 monographs of the French Book, Five MZ and Five DZ, illustrate this problem). If the astrological hypothesis is valid, as I tend to believe, it only adds as a differentiating filtering factor to both the genotype and its expression in the family and socio-cultural environment.