

Means of all the four element sub-groups (Water, Air, Fire and Earth) were compared and are shown in Table 3. The means reveal that the Water sign group had the lowest mean scores as compared to the other three (Fire, Air and Earth) sign groups. The Air signs group scored highest on Neuroticism. However, the differences were, again, not significant.

Table 3. Means, SDs and t-ratios for the total sample of Water, Air, Fire and Earth signs on Neuroticism.

Element Sub-groups	Mean	SD	N
Water signs	8.68	4.72	109
Air signs	9.96	4.94	135
Fire signs	9.29	5.14	112
Earth signs	9.05	4.98	118

CONCLUSION

The results of the present study are in agreement with those of Kanekar and Mukherjee,¹⁶ Eysenck and Nias⁴ and Mohan, Sehgal and Bhandari¹² that there is no significant difference between a person's Extraversion and Neuroticism scores on the one hand and their Sun-sign on the other. It can be concluded, therefore, that an individual's personality scores on the Eysenck Personality Questionnaire are independent of his Sun-sign, given the sample size and techniques used in this study.

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LETTERS TO THE EDITOR

A double-blind test of astrology

Dear Sir,

A recent article by a young physicist, Shawn Carlson, in *Nature* (1985;318:419-25) described a very carefully conducted experiment on the validity of astrology. The negative outcome of this study has been quoted many times and seems particularly impressive when it is realised that the whole plan of the experiment was welcomed by the astrologers who took part in it. In actual fact the experiment illustrates the danger of carrying out psychological experiments (and any experiment using personality variables in its design must be a psychological experiment) without having psychologists with experience in this field directing the relevant portions of the research design. This absence of experienced psychologists is noticeable both in the list of people who advised the author and in the ranks of the astrologers who approved of his design.

Briefly, the experiment seemed particularly appropriate as it involved interpretations of whole charts done by astrologers in the usual way. The test used was the California Personality Inventory (CPI), which has 18 scales measuring personality traits of various kinds. Results of the test are displayed on a chart profile, and astrologers failed to predict, from the horoscopes of their subjects, their actual position on the 18 traits of the CPI profile. In view of the fact that astrologers had confidently anticipated that they would be able to carry out this matching, it would seem that the conclusion of a failure of astrology must inevitably follow. However, this is not so.

The CPI is a well-known and widely used personality test, but it was constructed on an *a priori* basis and not on the basis of a proper correlational and factor-analytic analysis. Such analyses, carried out more recently, indicate that most of the variance of the test items is carried by two factors, Neuroticism-Stability and Extraversion-Introversion, as is the total validity of the test in predicting various types of behaviour. Thus there is a defect in the test itself which may be crucial.

Another defect, which is widely recognised, is that the test profile is not self-explanatory. Psychologists have to be trained to use a test and the notion that untrained astrologers would be able to interpret test scores is contrary to claims made in the test manual itself. Astrologers may have failed because they are unable to do what they say they can do, but it is equally possible that they failed because they misinterpreted the trait-names on the profile, as many people have done who did not have the proper training!

This interpretation of the results gains credence from the fact that in another part of the experiments, students themselves failed to recognise their own profile when asked to pick it out from three profiles! This means very simply that a person cannot recognise his own personality profile, and it would be absurd to expect an astrologer to do better than the person involved himself in choosing the profile. It seems to me that the failure of the experiment to produce meaningful and interpretable results is due to the faulty choice of the personality inventory in question. If the CPI had to be used, then a lengthy period of training should have been given to the participating astrologers and students; without this period of training they would be as incapable of reading the results as would be a novice looking through a microscope without having received any instruction! Indeed, the way the whole experiment was designed, and approved by the astrologers, indicates clearly that both sides regarded psychological expertise as a negligible quantity, and felt that anybody can do as well as a trained psychologist in choosing, interpreting and evaluating results of the application of personality inventories. This may be a widespread belief but it is an erroneous one, and we must be grateful to Carlson and the participating astrologers for making the point so clearly that psychologists should always be

involved in experiments using psychological material. One feels that it should not be necessary to stress an obvious point like that, but apparently the need to do so does exist, and indeed in our book, *Astrology—Science or Superstition?* Dr Nias and I came across study after study where the whole experiment had to be faulted because of quite elementary errors in the choice or interpretation of psychological measuring instruments, errors which would be obvious to a first-year student of psychology. If the Carlson experiment has no other effect than to alert astrologers and their critics to the need for expert psychological participation in the design and carrying out of such experiments, it will have been well worth the while.

Institute of Psychiatry
University of London.

Professor Hans Eysenck

Bibliographic chronology of Michel Gauquelin's planetary heredity research

Dear Sir,

My experiments on planetary heredity are often inaccurately reported. The first and main references are generally unknown, probably because they were not published in English. At the request of several *Correlation* readers, I would like to give a brief bibliographical account of my investigations.

Following the hypothesis I outlined in 1960,¹ I presented a detailed methodology and the first evidence for planetary heredity in 1961 in a German scientific journal,² and then in French.³ In 1966 my most comprehensive book on the subject was published, based upon 25,000 birthdata.⁴ Several other contributions, also in French and German, followed this book and provided new observations. These were published in 1968 and 1969 and I am the sole author of these articles.⁵⁻¹⁰

I thought then that it was high time for me to break the language barrier and to publish my work in English. In 1970, my best friend, Aimé Durand—who started working with me on astrological research in the late '40s—financially helped my laboratory to publish all the birth and planetary data of the hereditary experiment.¹¹ In 1972 he helped me again to publish the results.¹² It is worth stressing that data and observations given in these volumes are the same material previously published in my 1966 French book—I just translated the most relevant parts of it.

Several years later, I decided to carry out a second series of experiments in the city of Bourges and in the 14th arrondissement of Paris. The results were published in 1977 in one of my laboratory reports.¹³ They tend to vindicate clearly my previous observations.

During the 1979-80 winter in San Diego, with the help of Astro Computing Services, I carried out a re-analysis by computer of all the heredity data previously calculated by hand. For the 1966 experiment, the computer results confirmed my original results but were somewhat lower in significance, due to inadvertent bias in the hand calculation when interpolating from tables.¹⁴ The same control was carried out for the data of the 1977 experiment and, again, the same bias was noted, making the results of the second experiment less significant than the first ones.¹⁴ Worried by that, I decided in 1981 to undertake a third series of experiments on a very large scale. It took me and my collaborators three years of hard work to gather 50,000 subjects divided into five separate tests, four in Paris and one in the city of Lille. Planetary data were provided by Astro Computing Services. In 1984 I published the results of this study.¹⁵ None of the five tests that were carried out showed a positive effect and the replication was devoid of any significance.

As Dr David Nias rightly pointed out in his review of my latest study, these observations constitute a serious failure to reproduce the results of my two original studies.¹⁶ I tried to

explain such a discrepancy between past and present results but a recent analysis comparing planetary hereditary for two samples of births occurring in the *same* arrondissement of Paris (12th arrondissement) led to inconsistent and puzzling results.¹⁷

In a recent letter to *Correlation*, Marie F Schneider-Gauquelin claims that my third experiment does not provide any convincing evidence against the planetary heredity hypothesis.¹⁸ I am afraid that her confidence is based on a very superficial analysis of the new set of birthdata I used and *published*. This material seems perfectly all right. I suggest she devotes her time, energy and money—as I did—to undertaking her own independent replication on a fresh sample of 50,000 subjects. Such data would be very interesting to see and discuss.

As far as the planetary heredity research is concerned, Marie F Schneider-Gauquelin's involvement has been limited to the gathering of the data for the first experiment. Since the beginning, all investigations on heredity (and on famous people) have been published first under my sole name and responsibility.¹⁹ If, eventually, it turns out that there is no planetary heredity, I am the only person to suffer, so to speak. That is why I am refusing to rest on my—perhaps dubious—laurels.

LERRCP
8 rue Amyot,
75005 Paris

Dr Michel Gauquelin
Director

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