Is the “Mars Effect” Genuine?

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Abstract - Gauquelin claimed that there is a statistically significant correlation between the positions of Mars and the times and places of birth of sports champions. Independent scientists have attempted to replicate this hypothesis without success. We provide a brief history: the Comité Para, the Zelen and U.S. tests, and a recent French test. Ertel and Irving, in sifting through the data, attempt to rescue Gauquelin’s thesis. Ertel introduced his “eminence test”, and Ertel and Irving their “IMQ bias indicator.” However, they presuppose what they set out to prove. We conclude that there is insufficient evidence for the “Mars effect”, and that this effect may be attributed to Gauquelin’s selective bias in either discarding or adding data *post hoc*.

I
Gauquelin’s Claim

French psychologist and writer Michel Gauquelin, in collaboration with his wife, Françoise Schneider-Gauquelin, wrote that although classical astrology was mistaken, there were “astrobiological” or “cosmobiological” correlations between planetary positions and birth times on the one hand and personality traits and professional achievements on the other – between Jupiter and military men, Saturn and scientists, Mars and sports champions, *etc.*

The planet Mars rises and sets just like any other celestial phenomenon. Mr. and Mrs. Gauquelin divide the time between rising and setting into six equal intervals, numbered 1 through 6. Likewise, they divide the time between setting and rising into another six equal intervals, numbered 7 through 12. These sectors coincide with one of a dozen or so popular astrological house systems, namely that of Placidus, though the Placidean numbering is different.

The Gauquelins suggested that sports champions were somewhat more likely to be born when Mars is passing through the first sector (approximately the first two hours after the rise of Mars) and the fourth sector (roughly the two hours after culmination). Interestingly, in the traditional astrology of Ptolemy (*Tetrabiblos*), the two most important points in the house divisions are the Ascendant (“oroscopos”) and the Midheaven or *Medium Cœli* – which are the initial boundaries of the Gauquelins’ sectors 1 and 4 (Rawlins, 1979-80, p. 27).

It would appear that the chance of being born in either the first or fourth sectors is 2 out of 12, or 16.67%. A small adjustment is necessary, however, to take two factors into account: (1) the astronomic factor, namely the positions of Mars as seen from Earth, or more specifically from the latitude of France, and (2) the demographic factor, namely the daily pattern of births (more in the early morn-

ing, fewer in the evening). This adjustment has been estimated at about 0.5%, making the chance expectation 17.17% (Rawlins, 1979-80, p. 30). Sports champions, said Gauquelin, were born in the first and fourth sectors about 22% (more exactly, 452 in 2,088, or 21.65%) of the time. The deviation was too small to be of any practical use. One must meticulously collect hundreds of cases to observe it at all. Yet if Gauquelin was correct, the deviation was theoretically interesting. [1]

Michel Gauquelin collected champions' names from sports directories, and then tried to locate their actual birth data in the town registries. In support of his claim, he published in 1955 a total of 567 French champions with their names and birth data (plus one erroneous name) (Gauquelin, 1955). In 1960 he reported 915 additional foreign champions, together with 717 "less well known" sports people that served as a control, but without specific names and data (Gauquelin 1960).

He added further data to his files from a replication by the Belgian Comité Para (more on this below). With Gauquelin's help, the Comité Para derived a sample of sports champions, which by 1968 produced 330 new names (mostly of French champions).

Gauquelin added another 276 names (among whom were 113 aviators and 76 rugby players) to his total sample, to yield 2,088 "well-known" champions (Gauquelin, 1970). Meanwhile he collected piecemeal another 278 "lesser" champions before 1976. We mention this, because in the discussions Michel Gauquelin and others have stated several times that the 2,088 consisted of 1,553 champions collected first, followed by the Comité Para test of 535 champions.[2]

The Test by the Belgian Comité Para

As mentioned above, the Belgian Comité Para, beginning in 1967, attempted to test Gauquelin's thesis.* Their 535 champions consisted of 205 already in Gauquelin's 1955 book (it was thus not an entirely fresh sample) and 330 "new" ones. In 1976 the Comité Para published its final report (1976), and found that 22.2% of these sports champions were born with Mars in sectors 1 or 4. (We shall hereafter refer to the percentage born with Mars in sectors 1 and 4 as the Mars percentage.) The Comité Para maintained, however, that Gauquelin's theoretical expectation (of about 17 percent) was not computed correctly. There was thus some dispute between the Comité Para and Gauquelin about whether the test constituted a replication. The Comité Para thought that demographic factors

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1. A sympathetic account of the Gauquelins' studies can be found in Eysenck and Nias (1982, pp. 182-209), and interested readers are encouraged to consult this source. Though the two authors were favorable to the Gauquelins' hypotheses, we believe that they might modify their views were they to examine the recent research and findings, which we summarize below.

2. For example, in "The Truth about the Mars Effect on Sports Champions" (M. and F. Gauquelin 1976) the Gauquelins refer to "the committee's results for 535 sports champions ... our former results for the group of 1,553 other champions." In the next issue of *The Humanist* (Abell and Gauquelin 1976, p. 40) this evolved into 1,553 champions from the 1960 publication and a separate sample of 535 making a total of 2,088. M. and F. Gauquelin (1977) refer in their report on the Zelen test to "our first group of 1,553" and "the Comité Para's group of 535," and in 1979 they distinguish "the effect observed by the Belgian Comité Para (22.2 percent) and by us (21.4 percent)," i.e. 119 in 535 and 333 in 1,553. (See also Gauquelin, 1978.)

were not properly taken into account: the births of the athletes were not uniformly distributed in the time-period studied (1872-1945), and the daily patterns of births varied during this period. Gauquelin insisted that the Comité Para's test had confirmed his hypothesis. The Comité Para denied it.

Gauquelin had helped to supply the data for the test of the Comité Para. However, the circumstances surrounding the data compilation of the test are most unclear. Ertel (1988) claims that the 332 "new" champions of the Para test (a counting error; it should be 330) had already been collected by Gauquelin in 1962, along with 76 Belgian soccer players that had not been used in the Para test. Luc De Marré states that at the meeting between Gauquelin and the Comité Para in 1967 the list of 535 names was already decided upon (Ertel & Irving, 1996, pp. SE-18, 19, and 50). This claim is hardly credible; it presupposes that the Comité Para was clairvoyant and knew in advance the precise number of champions about whom they would receive information from town halls. The Comité Para collected 430 French champions from their main source book, but Professor J. Dommanget has provided documents indicating that data of 589 champions from this book were requested.

Concerning the Belgian soccer players, it was decided to select only the 43 who had been chosen to defend the glory of Belgium at least 20 times. It is unknown how that decision was arrived at and if any prior knowledge of the Mars effect among Belgian soccer players played a role in that decision. If Ertel's information about the year 1962 is accurate then Gauquelin knew already the Mars positions of 119 Belgian soccer players when the decision about the cut-off at 20 was taken. We do know that above that cut-off the Mars percentage happens to be 21% and below that line it is only 12%.

We think there is sufficient reason to reject the result of the Para test. There are too many doubts surrounding the process of data gathering.

The "Zelen Test"

The dispute between Gauquelin and the Comité Para concerned the expected "Mars percentage" for the general population. Marvin Zelen, then a professor of statistics at State University of New York, now at Harvard, proposed a test of the baseline percentage. This became known as the "Zelen test," and it was developed in cooperation with astronomer George Abell and Paul Kurtz. Zelen recommended Gauquelin randomly draw 100 or 200 names from his sample of champions,[3] and then compare their Mars sectors with all other births occurring at the same times and places (Zelen, 1976). Zelen designed this test to help determine the baseline Mars percentage, and also to control for various other demographic aspects that had so far not been a matter of dispute.

Michel and Françoise Gauquelin assembled birth data on 16,756 ordinary persons born at about the same times and places as a subsample of 303 champions.

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3. Zelen specifically referred to "Gauquelin's sample of 1,553 sports champions." Apparently Zelen had been led to believe that there was a neat batch of pre-Comité Para champions that could be used to generate a null-hypothesis for the new batch of Comité Para-champions.

They observed that the Zelen method yielded a theoretical prediction of 51.4 births in sectors 1 and 4 among the 303, i.e. 16.96%. More precisely, this result means that the "true" percentage is between 16.4% and 17.5% (95% confidence limits) and that there is no reason to suppose that the astrodemographic correction should vastly exceed 0.5%. There was again some dispute as to the validity of the test, particularly since the Gauquelins did not follow Zelen's original protocol (Zelen *et al.*, 1977). For example, Gauquelin did *not* draw the names randomly. His total sample of 2,088 champions included 42 Parisian athletes, and he included all 42 in his subsample of 303. He selected the remaining champions from those who had been born in capitals of Departments of France or Provinces of Belgium.

The Gauquelins also chose the matching non-champions in Paris from only one of the 20 arrondissements. Strictly speaking, this was a breach of the protocol, but it is difficult to see what difference it could make for the expected percentage, as the weight of "Paris" in the 16.96% was less than one-seventh. A more important deviation was Gauquelin's decision to examine only capitals of Departments and provinces, though this might mean no more than a few tenths of a percent. Nonetheless, the results suggested that demographic factors were not the explanation of the "Mars effect".

The Gauquelins pointed out that among their 303 champions were 66 Mars champions, *i.e.*, 66 born with Mars in sectors 1 and 4. Given the expectation of 51.4, they wrote that this was "clearly significant at the 0.05 level." Now "significant" in statistics merely means "worth a closer investigation," even though it is often misinterpreted as "incontrovertible proof." Zelen, Abell, and Kurtz's comments on the Zelen test constituted such a closer investigation. Zelen pointed out that the subsample of 303 was not randomly drawn from the 2,088. More specifically, about half of the "excess" of 15 Mars champions came from Paris. In other words, the comments of Zelen, Abell, and Kurtz constituted a very mildly formulated suggestion that it was not the astrodemographic factor, but some peculiarity associated with Gauquelin's data handling and collection, that might be the ultimate explanation of the Mars Effect.

Some critics have interpreted these comments as inadmissible sample splitting. However, the core of these comments constituted a legitimate inquiry. If a non-random procedure compromises the representativeness of a sample, then surely it is permissible at least to discuss the effects of such deviations from the protocol.

Much of the ensuing controversies have centered on these remarks about champions from Paris and elsewhere. Dennis Rawlins was much more outspoken in his conviction that the main problem might be Gauqelin's data handling. He predicted that the outcome of the Zelen test would not been seen as the exclusion of an astrodemographic explanation of Gauquelin's findings, but instead would mistakenly be seen as an independent test of the Mars effect. He advised that the Zelen test episode should be treated as a tactical error. With the wisdom of 20/20 hindsight Rawlins no doubt was correct. The Zelen test was proposed and undertaken in the true skeptical spirit of seriously examining the somewhat implausi-

ble claim of a giant astrodemographic effect, in the expectation of possibly finding a naturalistic explanation of the Mars effect.

The U.S. Test

Since it was Gauquelin himself who had done almost all data gathering in support of the Mars effect claim, an independent replication was called for. When Marvin Zelen first suggested the Zelen test, he also said that at least one further replication, on a fresh sample, would be necessary. George Abell (1976) put this suggestion into print: "if ... [Gauquelin's] results hold up, then it is necessary to repeat the experiment with a new sample, say in the United States." (Since the genesis of this test actually predates the formation of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), we call it the U.S. test. And even before the publication of the results of the Zelen test, Paul Kurtz, Marvin Zelen, and George Abell met with Michel Gauquelin and together they outlined plans for a test in the U.S.)

Zelen, Abell, and Kurtz constituted themselves as an *ad hoc* committee to devise and supervise the test. It was a blind experiment, with the data drawn by students at the State University of New York, Germain Harnden and Frank Dolce. Dennis Rawlins in San Diego calculated the positions of Mars. Although the American group consulted with Gauquelin throughout, he had no direct role in compiling the data. The American group used five sports dictionaries – *The Lincoln Library of Sports Champions* (1974), *Who's Who in Football* (Mendell and Phares 1974), *Who's Who in Basketball* (Mendell 1973), *Who's Who in Boxing* (Burrill 1974), and *Who's Who in Track and Field* (Hanley, 1973).

The U.S. researchers believed that consulting sports directories or *Who's Who*s of sports champions would be the most reliable guide. These volumes generally publish lists of the highest achievers in sports and the best-known champions. Some do so by listing athletes across many sports, and others only provide a *Who's Who* for specific sports. Of the millions and millions of individuals engaged in competitive sports in America, these directories only contain a few thousand champions, the *crème de la crème*, as it were. These volumes were compiled by editors and authors who were surely unaware of Gauquelin's hypothesis. Some of them contain citations of several hundred of the leading champions in a specific sport. The *Lincoln Library of Sports Champions* contains 493 entries derived from all of the fields of sports. *Who's Who in Football* contains 1,397 of the most famous names from the very beginning of the sport to 1974.[4]

Who's Who in Basketball contains 921 names, *Who's Who in Boxing* 499 names, and *Who's Who in Track and Field* 420 names. When we delete the champions born outside of the U.S., the champions for whom insufficient birth data are supplied, the duplicate entries, the coaches, umpires, referees, reporters,

4. Ertel is rather arbitrary in classifying sports. He treats American and European football as if these two sports have more in common than the name (see Ertel and Irving 1996, p. SE-56, note 33) and he goes so far as to suggest that basketball is not a sport at all, but rather a form of art like ballet (Ertel and Irving 1996, p. SE-54, note 31).

managers, promoters, team owners, and so forth, we are left with a grand total of 2,419 eligible champions.

These numbers suggest that the standards used were comparable to Gauquelin's own, which had yielded his 22% hypothesis; for these 2,419 eligible champions are from a country with five times the population of France.

Gauquelin's 1,356 French published champions number more than half of this amount, and that is *after* he had selected the "best" and after the loss due to the untraceability of champions' birth data.

Given the then recently enacted Privacy Act in the United States, many registries would not supply birth data without the permission of the athletes, which was difficult to obtain. Accordingly, the American group deemed it essential to send for the birth data of *all* the sports champions listed in those directories, without selection, provided they were born in states that agreed to supply data. The American group was able to assemble a sample of 408 sports champions. The results were negative, with 55 (13.5%) of the sports champions born with Mars in the first and fourth sectors. With a null-hypothesis of 16.67% the *p*-value was reported at 0.09. Hence, Zelen, Abell, and Kurtz concluded that an effort at replication of a fresh U.S. sample showed no evidence for the Mars effect (Kurtz *et al*., 1979-80; Rawlins, 1979-80).

Those who sent for the data did not have any prior knowledge of the Mars positions. The Mars effect was calculated by Dennis Rawlins in San Diego only *after* the data were received. The U.S. researchers sent for *all* the data and they published *all* of the data received from the states waiving the Privacy Act. They at no time knew the Mars positions before they were included in the sample, nor were any data omitted.

These facts are clearly stated in the report on the U.S. test (Kurtz *et al*., 1979, pp. 21-23). ** One of our authors, Ranjit Sandhu, in response to an earlier version of Ertel and Irving's paper, independently reviewed (in late 1995) the original data from the U.S. test. He corroborates that the above test was conducted as the published accounts indicate.

Gauquelin immediately disputed the interpretation of the results of the U.S. test, claiming that too many U.S. champions were not outstanding enough (M. & F. Gauquelin, 1979-1980). For example, he argued that the names listed in *Who's Who in Football* were "surely too many to represent the top athletes." He wrote that the selections should have been made from *within* the five directories, and that one must winnow names from these lists. By what criteria? Only the "most famous," said Gauquelin, and only those, he insisted, who were "internationally known."

But these champions are famous! Anyone who wishes to examine whether the champions listed in these directories are anything less than eminent is encouraged to visit a library and inspect these books thoroughly. He or she will find that very many of these champions are still being mentioned frequently on the sports pages of U.S. newspapers. Gauquelin tried to select a subsample from the total set of 408 by pointing to other American reference books in which they were

mentioned. Eventually he included only 192 in his files, and he discarded 216 names entirely, even though they had been published. His selection, however, was clearly made in full awareness of the positions of Mars at the birth of all 408 champions.

Gauquelin's 1979 Test

Gauquelin had meanwhile published in 1979 another test of 432 French sports champions who had not been included in his original studies, which he claimed further corroborated his thesis (Gauquelin, 1979). The U.S. group was hesitant to accept his conclusion, because the criteria by which Gauquelin selected notable sports champions seemed to vary from test to test. They wondered whether Gauquelin's own *selective bias* was not the real explanation for the "Mars effect." Kurtz, Zelen, and Abell speculated that Gauquelin knew the Mars sectors of athletes beforehand and introduced the criteria *post hoc* in order to favor those with Mars in key sectors (Kurtz *et al*., 1980).

Gauquelin's study excluded the names of 423 "lesser" champions. Gauquelin never honored the American team's repeated requests that he provide this list. According to Gauquelin these 423 lesser champions were composed of two groups: "minor" French champions taken from Le Roy's *Dictionnaire encyclopédique des sports*, and Italian cyclists taken from the reference books *Vélo* 1968 and *Vélo* 1970. (Gauquelin, 1979, pp. 21 and 28). Gauquelin said that these three works together contain 42 champions in Mars sector 1. In Ertel's database of Gauquelin's champions there are 24 Italian cyclists marked "GMINV",[5] and only one of them is born in Mars sector 1. Thus one would expect 399 French "minor" *Dictionnaire* champions, with 41 of them born in sector 1. Between 1979 and 1986 Gauquelin located additional names (Ertel, 1988). According to Ertel, the original "minor" French champions numbered 432, not 423. So Ertel's database contains 455 champions marked "GMIND", though after correction for duplicates the number should be 453. However, these 453 contain only 32 champions born in Mars sector 1. So something does not tally: either Gauquelin's sector count in 1979 is wrong or Ertel's database is incorrect. The numbers Gauquelin reported give the impression that his "control group" had a Mars percentage of 16.8% (71 out of 423) instead of something like 13%. In actuality, the 453 "lesser" champions in Ertel's files contain 61 Mars champions (13.5%), and the "lesser" champions found by the CFEPP had a Mars percentage of 12.2%.

Gauquelin continued to collect data after he published this test. In 1982 he gathered, on his own, another 159 U.S. champions. By 1986 he had added another 50 French athletes (both "famous" and "lesser"), whose data he said had been untraceable in earlier studies.

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5. We understand that GMlNV means "Gauquelin Minor *Vélo* Champions," and that GMlND means "Gauquelin Minor *Dictionnaire* champions."

The New French Test

Until then the evidence for Gauquelin's "Mars effect" was based largely on French sports champions collected by Gauquelin himself. So it was proposed that an independent group of French scientists attempt a replication, using data on French athletes. Gauquelin agreed to this proposal.

The protocol for this test was published in the leading French popular science journal *Science & Vie* in October 1982. Of course these French athletes had been gone over thrice already, resulting in publications by Gauquelin in 1955, 1970 and 1979, and it was to be expected that not many new champions could be found. The value of the new test was entirely in its protocol, which covered all aspects of such a test from the setting of criteria to the final comparison with a control sample, and it aimed at a procedure that excluded even the smallest intrusion of Gauquelin's prior knowledge of Mars positions into the selection and data-gathering process.

This new test was performed under the auspices of the French Committee for the Study of Paranormal Phenomena (Comité Français pour l'Etude des Phénomènes Paranormaux, or CFEPP). Many researchers were involved at various times, and the support of two prestigious national institutes (INED, the National Institute for Demographic Studies, and ING, the National Geographical Institute) was enlisted. The French researchers tried to solicit sports journalists to assist them in the selection of champions. The journalists declined, though several recommended the use of the *Dictionnaire encyclopédique des sports* (1973) as the primary reference. Gauquelin urged the addition of another reference, *L'athlège* (1951), which the Committee added. The data were compiled independently of Gauquelin. Altogether 1,439 champions were selected, and these resulted eventually (after corrections by Nienhuys) in 1,120 reliable data received. The construction of the control group seemed to present insuperable logistical difficulties. By the time all the champions' data had been received, the CFEPP was plagued by various problems, and was unable to do any more data gathering, which would have required sending out many thousands of letters and gathering an estimated 24,000 names from Paris alone and many more from elsewhere. So a different method was proposed for the control group, namely the creation of a group of fictitious individuals by scrambling the data. Gauquelin agreed with this procedure (which had already been used by the Comité Para) and he was also given extensive opportunity to comment on both the selection criteria and the data received. The protocol stated that Gauquelin's proposals should be taken into consideration, which the CFEPP did by publishing them and analyzing them carefully. Among the 1,120 champions, 207 (18.48%) were born in the 1st and 4th sector, which does not differ significantly from the values obtained from the control group[6]. Thus the French test did not reveal a Mars effect.

Gauquelin's proposals to the French Committee consisted of modifications of the sample. He suggested that certain athletes be removed from the list because "they were not famous enough" and that others be added. His proposals appeared to the French Committee to be extremely biased. He suggested names to be

added that the CFEPP had not been able to find, he pointed out data received that contained errors, and he mentioned individual champions and entire groups that were overlooked or overrated, *but these suggestions skewed the results in the direction Gauquelin wanted*. For example, he conveniently recommended data corrections that increased the Mars percentage, and withheld those corrections that decreased the Mars percentage. Regarding the corrections that would neither increase nor decrease the Mars effect, he mentioned only one-sixth. The CFEPP researchers concluded: "In our considered judgment, the Mars effect study demonstrates *some bias in the selective process on the part of M. Gauquelin*" (Benski *et al*., 1996).

One of us (Nienhuys) carefully reviewed the CFEPP's study. He found a few errors and omissions, but these did not change the results of the study. Nienhuys formulates the conclusion to be drawn from the French test as follows: "*the whole point in this laborious test was to find out what remains of the Mars effect when one starts from scratch without the help of Gauquelin. The answer is: nothing*" (Benski *et al*., 1996).

Gauquelin committed suicide on May 20, 1991 in Paris. Ertel wrote:

> The root of the Gauquelin tragedy might be found in his struggle of many years, to no effect, for acknowledgement of his discovery in mainstream science. Great efforts at defending his empirical observations against successive attacks from three skeptical organizations had worn him out. In each of them he became entangled with ill-will and strategies so dubious some members of the adversary camps even left their organizations in protest. Michel Gauquelin stayed on the battlefield for nearly three decades, but toward the end of his life he repeatedly complained that the strain of those decades of combat had used up his physical resources.
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> Gauquelin did not leave behind any document explaining his decision, but there is one deplorable hint. By his last will he demanded that all empirical data amassed through his lifetime, more than 30,000 birth documents on file in perfect order, must be destroyed. His will was put into effect, and it is almost inevitable to understand his act as a charge, not only directed at those who had not played fair with him but directed at all scientists not serving and suffering as much as he did in the pursuit of scientific truth (Ertel, 1993).[7]

We believe that these charges are unfair. Gauquelin seemed seriously ill in January of that year, and it is said that he privately expressed negative opinions about skeptics. However, most of the skeptics who worked with Gauquelin were personally cordial, even though they raised legitimate questions whether there was sufficient evidence to corroborate his thesis.

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6. The CFEPP reports that the control group yields 18.2% as the "theoretical estimate." This seems to be the result of a slowly convergent algorithm, and Nienhuys found about 17.7%, a value close to what Ertel claims to be correct. This implies a *p*-value of about 0.42, *i.e.* a chance of about two in five of accidentally getting a value that deviates at least as much from 17.7% as the CFEPP's result. Even with this modification, however, the results are still not significant.

II
The "Eminence Effect"

A new dimension to the controversy is the work of Suitbert Ertel, who claims to find independent corroboration for Gauquelin's thesis. In the foregoing paper, he and Irving attempt to dismiss the negative tests disconfirming Gauquelin and to show why the Mars effect is still valid.

They base their method on Gauquelin's *post hoc* analysis of the U.S. test, in which he claimed that "internationally famous sports champions" (of a sample selected by him) showed a higher percentage born in Mars key sectors than the sports champions who were merely famous. The latter group had a Mars percentage far below the expected 17% attributed to "ordinary" people. It is at this point that the entire procedure became rather strange and peculiar. The independent tests of Gauquelin's thesis ultimately settled for simple criteria such as being mentioned in an authoritative source and meeting general across-the-board minimum quality requirements. The introduction of extra eminence criteria introduces a great deal of latitude into the analysis of data of people with known Mars positions.

In fact, Gauquelin's criteria of fame shifted from publication to publication. He would change criteria midstream. For example, in some studies only Olympic Gold Medalists would be included as sufficiently famous internationally; for other studies Silver and Bronze Medalists might be included.

Ertel claims to be able to resolve the question of "eminence" on more objective grounds, by proposing that we count the number of citations of an athlete in various directories. Ertel and Irving maintain that a citation count might mean only that an athlete was "referred to at least once." Referred to in what manner? A passing remark to an athlete or to a non-sport-related matter should not properly be counted as a citation or an indicator of eminence. Ertel assembled 18 directories, most of them European, and he claims to find a correlation between them and the Mars effect. The Gauquelin "Mars effect" hypothesis says that highly qualified champions show a high Mars percentage. The Ertel "eminence effect" hypothesis says that any group of athletes will show an increasing relation between "eminence" and Mars percentage. So if the Mars percentage happens to be low in a sample, the "eminence effect" still has a chance, thus allowing the claimant to bet on two horses for the price of one.

The contents of dictionaries are based on athletes' achievements. However, many items in a dictionary are not related to sporting achievement. Often a dic-

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7. The information about Gauquelin's "last will" is only a friend-of-a-friend tale: "I was told by one of Gauquelin's friends who said he had contacted relatives who had told him that Michel wanted the data to be burned" (e-mail message of November 14, 1994 from Suitbert Ertel to Jan Willem Nienhuys). The only thing that seems certain is that Gauquelin's files are gone. They might have been destroyed by people who were unaware of their value and who fibbed about it afterwards. There seems little evidence to justify the grand picture of a desperate last act directed to fellow scientists who had not played fair. All genuine research scientists hope that after their deaths their results, or at least their honor, will survive. Ertel ascribes to Gauquelin an uncharacteristic petty meanness. If Gauquelin thought his data of any value, should he not have made them available to any friend or colleague that was interested in them? If Gauquelin had actually left instructions to destroy his entire data file, this could only raise the most serious questions about the integrity of his research.

tionary will mention many recent “hopefuls” at the expense of champions of the past. The use of many dictionaries carries the risk of introducing a new bias. If one branch of sport happens to have a high Mars percentage, it is attractive to validate this by using a specialized dictionary for that branch of sport. If an investigator wants to argue that American sports people are less famous than Europeans, all he needs to do is avoid the use of American sources. In any case, there is *a priori* no reason to think that the mere mention in a directory is a better indicator of an athlete’s quality than the achievements of the champions themselves. And of course, until a test has unequivocally shown the contrary, there is also no reason to assume that Mars has anything to do with sports. So an “eminence effect” at best would be a result of exploratory analysis, and not support for a hypothesis framed before any data are collected.

In his “Commentary” on the CFEPP investigation (Benski *et al*., 1996), Nienhuys presented a method of rating sports people by their achievements. That method is – in its present form – perhaps not so suitable for comparing champions from different countries, but at least it avoids the arbitrariness in the choice of sources. Ertel decided in 1988 not to look at the sporting achievements themselves. We question whether that was because he did not know how, or because the method gave unsatisfactory results.

Ertel and Irving select groups of “eminent athletes” from the U.S. and CFEPP tests by various means, and they claim that both tests show some kind of eminence effect. These procedures constitute overanalyses of non-rejections of the null hypothesis. Such reanalyses – though less arbitrary ones – might be called for if planetary effects were firmly proved already, for instance by tests that had been independently replicated with fresh data. Further, Ertel’s conclusion that the CFEPP test confirms Gauquelin’s hypothesis was largely based upon his study of earlier incomplete and only partially corrected data. Strangely, Ertel *did not* include any of the five directories used in the U.S. test in his eminence appraisal, though they are easily available through interlibrary loan. Nor did he consistently use *L’athlège* or *Dictionnaire encyclopédique des sports*, the key French directories used by both Gauquelin and the French Committee. Moreover, Ertel drops and adds dictionaries in subsequent studies. These omissions are puzzling in a test that he claims is objective. Ertel’s eminence test thus is a function of the directories that he had in his possession at that time or that he borrowed from Gauquelin, and it is not based on *all* of the directories available, and we suggest that this might be attributed to Ertel’s own bias in selecting directories.

This eminence-effect hypothesis took its inspiration from Gauquelin’s efforts to dispute the U.S. test; so it is not strange that Ertel would claim that the U.S. test provided evidence for the eminence effect. But it is invalid to use the same data twice in such a way: once to provide the inspiration for a conjecture, and then to “explain” that same data by the conjecture. Incidentally, the evidence here is very weak: when a one-tailed test yields $p = 0.06$, it is clearly nonsignificant (Ertel, 1992).

What does the eminence effect show here? In 1988 Ertel observed that there was apparently a more or less linear relation between the number of times a Gauquelin athlete was mentioned in 18 specific sources and the athlete's chance of having been born in Mars sectors 1 and 4. Yet Koppeschaar (1992) observed that the total aggregate of Gauquelin's French champions did not show much of an eminence effect. This was confirmed by the CFEPP test. Ertel's database in 1988 included 933 of the 1,066 CFEPP champions. Though his results looked interesting, he made many errors in attributing citations to the *Dictionnaire encyclopédique des sports*. We have corrected these. We have also corrected the Mars calculations, using the sectors as computed by the CFEPP. The following table shows the result.

TABLE 1
Citation Counts for 933 CFEPP Athletes

Number of citations:	0	1	2	3	4	5	6	7	4+
Number of athletes (N):	130	385	270	95	30	17	5	1	53
In primary sector (M):	28	62	52	23	6	5	1	0	12
M as percent of N:	21.5	16.1	19.3	24.2					22.6

Percentages of numbers 30 and lower are omitted. The last column is the sum of all columns of 4 or more citations.[8]

As one can see, there is no trend, which is not surprising because the "eminence effect" had already been found to be statistically insignificant if only the French champions were considered (Nienhuys 1993). Ertel also reports that the Comité Para sample does *not* exhibit the eminence effect (Ertel & Irving 1996, p. SE-16).

Though there is no trend as such, the oft-quoted champions do have a somewhat higher Mars percentage. This calls for a closer investigation. Such a higher percentage is only meaningful if we can be absolutely sure that no data are suppressed. From the example of the 216 U.S. champions that Gauquelin discarded we see that this is doubtful.

Ertel has contributed something of value to this discussion. He visited Gauquelin in Paris in 1986 to obtain the data on Gauquelin's unpublished sports champions. It was known, of course, that Gauquelin had published the names of 2,889 champions, and from Gauquelin's publications it is obvious that he was continually collecting data. In his publications of 1960 and 1979 he mentioned 118 Germans, 599 Italians, and 432 French and Italian sports people that he had used as control groups, altogether 1,149. So there were many more names in his files, and it is not surprising that he had not yet published all of them. But there are two surprises. The first is the large number (347) of additional and unexpected unpublished data (raising the total of all Gauquelin champions to 4,385, of whom 6 had two near-identical records in Ertel's database), and the second is the large negative Mars effect in the total of unpublished data. In 1982 Michel and

8. Ertel's files did not contain 133 of the 1,066. And according to Ertel and Irving (1996, p. SE-43) there are 17 more that are absent. Ertel, who has had four years to compare the CFEPP's data with Gauquelin's, acknowledged in the summer of 1996 that 133 is correct.

Françoise Gauquelin published their version of an American test. It contained 351 names, including 192 from the previous U.S. test. But they did not retain the 216 other names of the U.S. test in their files[9] – an egregious procedure.
In their above paper, Ertel and Irving, however, corroborate the conclusions of the U.S. and French tests, namely that Gauquelin had allowed his bias to intervene in the selection process. Ertel and Irving state that (p. 2):

> Gauquelin had occasionally referred to his exempting low-eminence athletes from analysis, which is a legitimate procedure in principle, *if done without awareness of planetary positions*. Ertel suspected, however, that on occasion Gauquelin might have been aware of Mars positions when he decided whether an athlete was or was not eminent enough to be added to the final sample. With Gauquelin's permission, Ertel searched out and analyzed this unpublished data, finding that *indeed, Gauquelin had tended not to exclude marginal athletes from his high-eminence sample when Mars at their births was in either the rising or culminating zones*. In other words, he tended to rank Mars G-sector cases among low-eminence athletes more favorably than non-G sector cases… . This indicates that Gauquelin must have been aware, to a certain degree, of Mars-sector positions ... (emphasis added).

Ertel discovered in Gauquelin's archives 1,503 champions whose birth times Gauquelin had requested but whose names and birth data he had not published. The Mars percentage among these was 14.77%, whereas among the total of 2,888 published champions it was 21.75%. The discovery of such a strong bias should have been reason to dismiss all of Gauquelin's data. However, Ertel did not do so. He assumed that a bias in judging people's athletic prowess was Gauquelin's only bias, and he developed his new "eminence" definition of the Mars effect. Moreover, from Ertel's collective writings it seems that he feels free to demonstrate the Mars effect, as either the original "high percentage" claim or his newer "increasing eminence characteristic."[10]

Let us explore Ertel's discovery of Gauquelin's bias further. In his comments on the champions collected by the CFEPP, Gauquelin objected to several names because their birth data did not seem "reliable" enough, arguing that certain birth records that were difficult to find might be the wrong records. Gauquelin, of course, did not reject all records that were difficult to find, and we can examine those that he retained. During the Belgian Comité Para's investigation a total of 88 champions turned up whose birth records we may say were truly difficult to find – for example, those born in Paris or in a place other than what the dictionary stated, or those born in a place with a fairly common name (like Saint-Etienne, Fontaine, Montreuil, or Saint-Nazaire), or those in which the actual birth month did not agree with the month found in the dictionary, including all cases where the dictionary gives the wrong year or only the year. Among these 88 "difficult to find" Para champions the Mars percentage is about 30%. This refers to the champions taken from the *Dictionnaire des sport*s (Seidler & Parienté, 1963).

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9. Ertel also discards these 216 names (Ertel & Irving, 1996).

10. An illustration of the failure of Ertel's eminence criterion is his recently published test of *1083 Members of the French "Académie de Médicine"* (coauthored with Arno Müller). This refers to a correlation with eminent physicians and the planet Mars. In this he explicitly contradicts his own eminence criteria on key points; for the Mars effect *decreases* as higher levels of eminence are achieved; he also abandons his own citation-of-biographical-dictionaries method to determine eminence. We quote from this study.

> For Mars the eminence relationship in the present study was less satisfactory. The effect *decreased* at higher eminence levels, but did not increase at low eminence levels. However, it is possible that G% for certain planets (here Mars) *is less related to eminence* for certain professions (here physicians) (Müller & Ertel 1994, p. 28, emphasis added).

> The great majority of members of the Académie de Médicine did not obtain any citation in 586 biographical dictionaries indexed by Arlan Appelletier. *Therefore a count of citations cannot be used as a measure of eminence* (Müller & Ertel 1994, p. 19, emphasis added).

Ertel changes the criterion of "eminence" and uses instead the physicians' ages of admission to the Academy. He ranks the total group of 1,083 according to this age. Then he splits the group into six almost equal portions. The first portion are the 181 youngest academicians. The next portion are the 180 next youngest academicians, and so on. Ertel here uses a wider definition for the Mars percentage, based on the inclusion of the "initial sectors" (see part III). As the total Mars percentage among these academicians is 25.2%, we can compare the expectations on the basis of this percentage with the actual numbers.

Age groups	1	2	3	4	5	6
Group size	181	180	181	180	181	180
Expected Mars number	45.6	45.4	45.6	45.4	45.6	45.4
Actual Mars number	40	50	45	44	47	47

This is after Ertel decided for himself that a division into 6 groups (and not into 3, 4 or 5 groups) would be most appropriate. Ertel shows the results only in graph form, without mentioning the actual numbers. We submit that the numbers display only one peculiarity: they are unusually close to their expected values. A chi-squared test yields $p > 0.9$; in other words, it rarely happens (1 in 10 times) that actual values are all this close to the expected values. Subdivisions into different numbers of groups give even flatter pictures. Since the French Academy physicians served as the original inspiration for the planetary hypotheses, the Mars percentage of 25.2% cannot very well serve as support for this hypothesis. Moreover, randomizations of the CFEPP's sports champions – admittedly not the same population – yield a Mars percentage of 23.6%, and the 25.2% of the physicians does not differ by an impressive amount from that.

Yet Ertel believes that this table shows that planetary effects increase up to medium eminence and then decrease at high eminence. He calls this "curvilinear." We would say the eminence hypothesis falls dead flat. Incidentally, his co-author Müller does not agree with Ertel's interpretation, and instead finds the results "ambiguous" at best.

Further, in *The Tenacious Mars Effect* (Ertel & Irving, 1996, p. SE-43), Ertel's table 10 contains many indications of the use of invalid statistics. We merely discuss the column referring to "all athletes" combined. The number of these is said to be 1,683. This number is wrong. Apparently Ertel did not notice 13 champions who occur both in the Comité Para sample and in the CFEPP sample. Then Ertel includes 5 champions from Gauquelin's proposed additions, plus some of Gauquelin "corrections," raising the Mars score by 4. More than a year before publication of *The Tenacious Mars Effect* Ertel was informed about the nature of these additions and corrections; he even refers to the very e-mail messages that contained this information. Ertel reports that he scrambled the data 200 times, simply by alphabetically shifting the birth years, a rather crude method. Of these 200 scrambles, 14 yielded a *higher* Mars score than the actual value, but one should, of course, look at *equal or higher*. These 14, divided by 200, yield then a "*p*-value" of 0.07, which is dubious, because to get a theoretical number like a *p*-value in this way with any precision, one needs about ten times as many scrambles. Then a few lines further the average Mars percentage of these 200 scrambles is reported, together with a *p*-value of 0.09. Given the error of including these Gauquelin proposals, the correct value is probably something like 0.13. As this is a one-tailed test, there is nothing special about this. Nonetheless, the result is called "near significant" on the strength of "$p = 0.07$." So this one column is, so to speak, a panoptic display of things that can go wrong in statistics: inefficient data search, unwarranted inclusion of data, confusion of "more" with "equal or more," inaccurate computation of theoretical probabilities, and attaching meaning to a result that is both *post hoc* and not significant by the words "near significant." ***

The champions in Gauquelin's 1979 publication were partly obtained from Le Roy's *Dictionnaire encyclopédique des sports* (1973). Of these, 134 were not French, and again these yielded a set of "difficult to find," again with a large Mars percentage. Gauquelin also extracted 224 famous French champions (born before 1950) from Le Roy's *Dictionnaire*, and the same situation prevails. The actual numbers are shown in the table below. The overall Mars percentage of 30.9% in these three groups is very high, even compared to Gauquelin's 22% hypothesis. These champions are as a group not better qualified than the groups from which they are taken.

TABLE 2
"Difficult to Find" Champions in Three Gauquelin Investigations

	All	"Difficult to find"	Born in key sector	Percentage
Para *Dictionnaire* champions	430	88	27	30.7%
Foreign Le Roy champions	134	37	13	35.1%
French Le Roy champions	224	40	11	27.5%
Together	788	165	51	30.9%

The 1963 *Dictionnaire des sports* contained many more potential candidates for selection, namely French people born in either France or Algeria. Though Gauquelin later found data for a few of them, he was never able to find information on many others – at least they are not in Ertel's files. The CFEPP investigation found quite a few of these, mostly from Paris, with a small Mars percentage. Also, in the CFEPP's investigation there were 104 champions that were difficult to find, as the month or place of birth was different from what the source indicated. Again, a number of these were not in Ertel's files, and again the Mars percentage among them was low (see table below). The last two groups combined have a very low Mars percentage, even more so compared to the three groups in the previous table. They comprise champions that Gauquelin must have tried to find.

TABLE 3
Two Subsets of Champions Found by the CFEPP and Not in Ertel's Files

	All	Born in key sector	Percentage
Para candidates	34	2	5.9%
"Difficult to find"	20	1	5.0%
Together	48	3	6.3%

This suggests that Gauquelin was suppressing data he thought "unreliable," and also that the data of the champions born in a favorable Mars position were not rejected as easily as the others. Possibly this happened more often with champions about whom he had information from different sources.

Ertel's finding can be summarized by the observation that the champions who occur in many books, especially books known to Gauquelin, have a somewhat higher Mars percentage than the others. Could this be due to a Gauquelin bias? Ertel says no, because Gauquelin may have been selectively publishing, but he did not discard data. But it is rather clear that Gauquelin was taking unwarranted liberties with the data. The evidence in short is (1) conflicting numbers between his 1979 publication and Ertel's database, (2) the muddled matter of the 76 Belgian soccer players that were never published, (3) the deletion of 216 Americans, and (4) the matter of the "difficult to find" cases. Moreover, (5) in his comments on the CFEPP investigation, Gauquelin was strangely silent about 39 champions that he almost certainly must have tried to find, but that are not included in Ertel's database (Benski *et al*., 1996, pp. 141-142).

In the light of these five points the 1988 Ertel eminence effect becomes only mildly interesting, rather than an ultimate proof that science as we know it must be revised drastically. It suggests that it was primarily not fame but having available several sources of possibly conflicting information that correlated with Gauquelin's selection bias. The 1995 Ertel eminence effect raises another matter. It still relies heavily on Ertel's assumption of the integrity of Gauquelin's data, which we question.

We indicated that we believe there is sufficient reason to reject the result of the Comité Para test. And as the doubts also extend to the foreign champions collected by Gauquelin, we see no reason to consider the eminence effect as anything but a side effect of Gauquelin's bias. Whether Gauquelin's bias was intentional or not is irrelevant; his data are unreliable, scientifically speaking.

III
The IMQ Bias Indicator

In their above paper, Ertel and Irving introduce a concept they call the "IMQ" (Initial versus Main sector Quotient), which, they argue, shows a likely bias in the data selection in the U.S. test. Again, for the general reader, we will first explain the term "initial sectors".

In Ertel and Irving's G-zone scale, the time between rising and setting is divided into 18 (rather than 6) equal intervals, numbered from 1 through 18, which we shall call minisectors. So when a planet rises it will first be for some time in minisector 1, then pass into 2, and so on. Likewise, the time between setting and rising is divided into 18 minisectors, numbered 19 through 36. Sectors comprise three consecutive minisectors. So sector 1 consists of minisectors 1, 2, and 3; sector 2 consists of minisectors 4,5, and 6; and so on.

Gauquelin's original planetary hypothesis claimed that sectors 1 and 4 (minisectors 1, 2, 3, 10, 11, and 12) were the ones with special meaning. These are the ones that Ertel and Irving now call "main sectors". What we have loosely called "Mars percentage" in the preceding, actually means the percentage born with Mars in the main sectors. A planet will enter main sector 1 by leaving minisector 36, and it will enter main sector 4 by leaving minisector 9. Hence these two minisectors may be called "initial minisectors". In Gauquelin's view these initial

minisectors were almost as important or even equally as important as the main sectors. This can be seen from diagrams in his books, written in 1955 and 1960; and he stated this explicitly in 1972.

Ertel claims that to explore the Mars effect properly, one should look at the enlarged "G-sectors" or "plus zones" of the tests that had already been conducted on the 12-sector scale. We submit that this is just one of the many arbitrary choices made *post hoc* in full knowledge of the data. Such delicate refinements might be studied once the main effect is proved beyond reasonable doubt, which is not the case here. As Abell, Kurtz, and Zelen stated in their reappraisal of the Zelen test: "This sort of exploratory data analysis is common in analysis of complicated data sets, but only for generating hypotheses to be tested, not necessarily for generating conclusions" (1983). Neither can it serve to detect significance. Ertel and Irving, however, attempt to generate conclusions and to detect "significance."

Suppose, for the sake of argument, that a new test would yield a significant result in terms of classical sectors, but not in terms of "plus zones." Would the Mars effect then be dead? Surely not. It would have been proved just as well, and the puzzle of the failing initial sectors would be the subject of further tinkering; such as the tinkering that Ertel performed (Ertel & Irving, 1996, pp. SE-16 − SE-19). So introducing the "plus zones" is just another ploy to bet on two horses for the price of one, and after the race has been run as well.

Ertel and Irving investigate the ratio between the number of champions in a sample that is born in an initial minisector and the number born in a main sector. They call this ratio a "bias indicator." Their idea is that a low value indicates a bias that raises the Mars effect, and a high value indicates a bias that lowers the Mars effect. The idea seems to be that whoever has some kind of bias cannot know about the special significance of the initial sectors. Since they maintain that the IMQ indicates a Gauquelin bias, it can also serve to indicate bias in the U.S. test. But this is a *non sequitur*. It is not necessary to demonstrate Gauquelin's bias by such complicated means. The IMQ concept was developed by them with the specific purpose of attacking the U.S. test.

More specifically, the IMQ test *presupposes* that there is a Mars effect – thus begging the question. Ertel and Irving apply the IMQ test to a grand total of three samples by skeptics: (a) the test of the Comité Para, (b) the French CFEPP test, and (c) the U.S. test. On that basis, Ertel and Irving imply that there could have been data manipulation in the U.S. test.

Apparently Ertel and Irving consider the U.S. test to be a stumbling block for the Mars effect; so now another peculiarity of the U.S. test is used to seek to discredit, in one fell swoop, both its results and the researchers who set it up. This peculiarity is the rather low Mars percentage of 13.5% that was found, where about 17% was expected. Ertel and Irving look at the initial minisectors, and try to argue that the percentage shown by them is suspiciously high.

Ertel and Irving's laborious computations are irrelevant. The purported small negative correlation between IMQs and key-sector percentages holds when one

considers the full spectrum of possible outcomes. But we are dealing here with a tactic developed in full knowledge of the results of the U.S. test. So the conditional probability applies: one should look at the chance that the IMQ would score so high in a test that accidentally scored as low as the U.S. test. The IMQ is a quotient and naturally one would expect it to have a high value when the denominator is small. Ertel and Irving do not give any specific data for the IMQ of the U.S. test, but we shall endeavor to do so. The U.S. test concerned 408 champions, of whom 55 were born in the main sectors and 29 in the initial minisectors. Ertel and Irving argue that the ratio 29:55 is rather large. We maintain that it is not. We have examined by computer simulation how often such a high IMQ might occur. A random process was simulated in which 408 times a random "champion" was drawn that had a chance of about 17% of being born in a main sector and a chance of 2/36 of being born in an initial sector. This was repeated 10,000 times, and the cases in which at most 55 were born in a main sector were retained; among these we counted the cases in which the IMQ ratio reached at least 55:29. These turned out to amount to just over 20% of the total.

If data are analyzed *post hoc* some prudence is called for. Two rules of thumb apply. First, only use the most straightforward statistical tests and avoid selecting only one peculiarity out of many possibilities. Second, only attach meaning to highly significant ($p < 0.001$) results. Ertel and Irving do neither and they feel there must be some kind of explanation for the low Mars percentage. They point to "low-eminence" admissions. This is implausible for reasons indicated already: the so-called "low eminent" were still famous sports champions.

Ertel and Irving offer another contrived "explanation" in their footnote 10. They suggest that the protocol of the U.S. test admitted the possibility that one of us received data by mail, then had them quickly computed by a computer astrologer, and then suppressed unwanted data. Of course, other outcomes of the IMQ test could have produced the same accusation. If it had been low, then the sum of the initial sectors and main sectors would have come out "significantly" low, also pointing to a "bias." Ertel and Irving are trying to play a game of "heads I win, tails you lose," after the coin has fallen.

It is amusing that Dennis Rawlins's statement that the American researchers did not have the expertise to compute the Mars effect is now used to insinuate a clever plot (involving astronomical insight and sleight of hand in experimental design) to deceive the other investigators of the project. Such insinuations are truly irresponsible, to put it kindly. In retrospect, the U.S. test did have a design error in that it did not foresee that it should have to defend itself against such unfounded charges, 18 years later. However, the records of that test are still available, and they clearly show that all names requested from the 22 cooperative states are there.[11] Champions who could not be included are mentioned either with the indication that no records could be found or that their birth times are not given.

Interestingly, the IMQ test does *not* apply to the Para test. Hence Ertel and Irving conclude that the Comité Para's IMQ deviation in the negative direction is

probably fortuitous, “due to chance.” Apparently the reader needs the guidance of Ertel and Irving to decide when mere chance is operating and when deviations have a deeper meaning.

Let us take a close look at the IMQ in the case of the Para test. The Comité Para reported 119 champions born in the main sectors. Actually both Gauquelin’s and the CFEPP’s investigations showed that the birth date of the boxing champion Hippolyte Annex was wrong – so there are only 118 main-sector champions in the Para sample. In any case, the number of champions born in initial minisectors was 27. A computer simulation comparable to the one mentioned above demonstrated that an IMQ ratio on or below 27:118 has a chance of 50% in this situation. So there is nothing special there.

Again, Ertel and Irving give no explicit numbers, but from their graphs it seems they assume there are only 23 in those initial minisectors. This is a consequence of taking Gauquelin’s data too seriously. The table below shows this.

TABLE 4
Three Mars-Sector Computations of the Para Test

Minisectors	Initial 36&9	Main 1&10	Main 2&11	Main 3&12	Total Main (1,2,3,10,11,12)
According to Gauquelin (1970)	23	33	39	51	123
According to Ertel & Irving	23	33	38	51	122
According to Comité Para	27	35	36	47	118

Clearly Ertel and Irving have relied on the 1970 numbers of the Gauquelins, and did not use the values of the Comité Para. We have checked the Para computations. There are a few minor errors, especially in the geographical locations; but there are over 60 differences between the Gauquelin minisectors and the Para computations, and in almost all cases the Comité Para had the correct result. Gauquelin’s data are clearly unreliable. They must be suspected of suffering from at least three types of bias: computational errors, errors from rejecting “unreliable” data (such as in the “difficult to find” cases), and errors in judging the “quality” of the athlete. As Ertel attempts to validate his theories by these data, they are likewise irrelevant or, at best, further evidence for Gauquelin’s bias. Thus Ertel and Irving’s IMQ test is a fiction.

The CFEPP investigation has made it clear that such studies have a great error potential: erroneous sources, clerical errors, etc. As long as the majority of the records is correct, there is little risk that a genuine effect will vanish in the noise. But when error correction is not kept strictly separated from knowledge of the results, a spurious signal can be produced. Irving and Ertel’s statistical manipulations with old and well-known data sets constitute such a spurious signal.

11. Ultimately Alabama, Arizona, California, Colorado, Delaware, Hawaii, Kansas, Kentucky, Massachusetts, Minnesota, Montana, Nevada, New Hampshire, New Jersey, North Carolina, North Dakota, Ohio, Oregon, South Carolina, Utah, Virginia, and Wisconsin all supplied data.

IV
Conclusion

What are we to draw from this protracted controversy? Researchers have spent decades patiently sifting through the claims of the so-called evidence adduced to support the Mars effect. There are two possible inferences:

First, that there is a genuine Mars effect. If this were the case, however, it would apply to only about 2,888 sports champions in the Western world, and would exclude many other famous sports champions (published and unpublished and discarded by Gauquelin). If the Mars effect was real, it should be confirmed by independent researchers. The U.S. and French tests have been unable to replicate the "Mars effect".

Second, that the Mars effect is based upon Gauquelin's bias. There are several rather compelling indications that this is so. His reactions to the U.S. test showed his efforts to redefine eminence, in full knowledge of the results, after the test was over. An examination of his files showed that he was doing the same thing privately with his own data. He also tried to influence the French CFEPP test in various ways by adding, deleting and changing records. We have adduced evidence that both in the 1960s and in the 1970s he discarded data that he thought unreliable. Perhaps some of this evidence could have been discovered 20 years ago or more, if scientists at that time had focused less on astronomy and more on Gauquelin's procedures in data collection. In other words, the key witness who claimed a remarkable effect turns out to be unreliable, and we must return a verdict of "not proven." The witness was probably the victim of his own illusion.

Some proponents of Mr. Gauquelin's hypothesis have repeatedly accused skeptical researchers of being biased. These charges have been hurled against the Belgian Comité Para, the U.S. group, and the French CFEPP. Yet the many scientists and scholars associated with this work who questioned Gauquelin's theories have exerted every caution to be fair-minded and objective. Regrettably, pro-paranormalists often accuse "establishment scientists" of being dogmatic. But Gauquelin's theories have not been summarily rejected with an appeal to infallible authority. They have been carefully examined, and a great deal of effort has been spent on that.

We conclude that after persistent and painstaking examination, there is no evidence for the Mars effect. It is time, we submit, to move on to other more productive topics.

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Comments

The above appeared in *Journal of Scientific Exploration*, Vol. 11 , No. 1, pp. 19-39, 1997.
The page numbers (here below the pages) are as in the original. The layout is slightly changed but line breaks are preserved.
The paper was an answer to:
Ertel, S. and Irving, K. Biased Data Selection in Mars Effect Research. *Journal of Scientific Exploration*, Vol. 11 , No. 1, pp. 1-18, 1997.

In abridged and slightly altered form, it appeared also as a chapter in Paul Kurtz, *Skepticism & humanism, the new paradigm.* Transaction Publishers, New Brunswick and London, 2001.

It must be pointed out that prior to publication the editors of *JSE* showed it to Ertel and Irving, so they could comment on items that contradicted facts, inaccurate quotes or interpretations. Ertel used this opportunity to make an enormous number of remarks, only some of which were honored by minor adaptations of the text.

In the book version the language is somewhat more florid (‘bordering on the bizarre’) and the whole section about the IMQ is deleted. Tables 2 and 3 are verbally rendered in the text. The text leading up to footnote 7 was moved to the footnotes. The references are less succinct than in the *JSE* article, and this is reflected in the above text. Some extra material is added, in the text above indicated by stars.

* footnote: Jim Lippard records in his “Chronicle of Events” in Suitbert Ertel and Kenneth Irving’s *The Tenacious Mars Effect* (London, Urania Trust, 1996) that the Comité Para completed data gathering on October 22, 1968, and he quotes a source: Michel Gauquelin, *Birth Times: A Scientific Investigation of the Secrets of Astrology* (New York: Hill and Wang, 1983). [It’s on page 102. Incidentally, this page 102 also has a graph comparing the “Gauquelin experiment (number of cases 1553)” with “Para Committee experiment (number of cases 535)” JWN] Jean Dommanget says that the first meeting between Gauquelin and the Comité Para took place in 1967 (unpublished paper read at the European Skeptical Conference of 1994).

** inserted text: Ertel and Irving do not believe it. Ertel had taken the effort to try to badger the former assistants Dolce and Harnden with many questions about the exact procedures 15 years earlier.

*** footnote added: This letter is mentioned in an abridged version of J. Lippard’s “Chronology of Events” (Ertel and Irving, *The Tenacious Mars Effect*, p. A2-26). [The words ‘an abridged version of’ are incomprehensible, JWN]

*** text in footnote added: In other columns this is compounded by arbitrary choices of sources to be used to assess eminence, and the number of citations that is most suitable to demonstrate the desired effect (surprisingly this number is 1 and not 2 or 3), and the conclusion is that “a Mars effect is clearly conspicuous.” We do not concur. There’s only a conspicuous Ertel effect.