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Bulky Mars Effect Hard to Hide: Comment on Dommanget's Account of the Belgian Skeptics' Research

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Abstract—In 1997, Jean Dommanget gave an account of investigations on the Mars effect claim conducted by the Belgian skeptics´ committee PARA. Gauquelin had shown that their data revealed a strong Mars effect. Dommanget rejects Gauquelin's claim, alleging justification through the committee's "model of the mechanism." The present paper scrutinizes the committee's model using a simpler data set to facilitate its intelligibility. It turns out that the committee's model is misconstructed. The Belgians' arguments resort to confused complexities through which apparent evidence for a Mars effect is dispelled. Gauquelin's early but insufficiently founded objections to the committee's critique now appear fully justified. The Belgian committee is urged to clarify persisting contradictions in the early history of its resistance to Gauquelin's discovery.

Keywords: Mars effect — planetary correlation — Gauquelin claim — skeptics — Comité Para

1. Introduction

Jean Dommanget, chairman of Comité PARA (CP), the skeptical Belgian committee for the scientific investigation of claims of the paranormal, has given an account of the committee's early research into the Mars effect (1968–1976) and of a sequel to that work (Dommanget, 1997). Their research started with a replication of Gauquelin's Mars effect claim on a sample of 535 sports champions, through which Gauquelin's prediction found strong support. CP, however, eventually found problems with Gauquelin's Mars effect calculation, above all with expectancy (Committee PARA, 1976). This issue is of prime importance because the Mars effect is defined by a significant deviation of *observed* birth frequencies, partitioned across 12 diurnal Mars sectors, from corresponding *expected* birth frequencies.[1]

CP proposed "a model for the theoretical mechanism of the purported phenomenon" (p. 275). Their model, the Belgian critics allege, invalidates

[1] More precisely, a planetary effect is defined by significantly deviating birth frequencies occurring while the planet occupies certain sectors, e.g., Mars sectors 1 and 4 on a 12-sector scale, for Comité PARA's experiment.

Gauquelin's expectancy calculations. If this were true, the Mars effect claim and all other planetary claims of the Gauquelins must be abandoned. A scrutiny of CP´s position, as presented in Dommanget's account, is therefore urgent.

A note on the intelligibility of CP's model is prompted by Dommanget's repeated complaint that "misunderstandings have unfortunately occurred with nearly all...authors who started studying this question. Many authors did not understand (or did not want to understand)...[our] position"[2] (p. 279). Dommanget should consider, however, aside from possible limitations of his readers, possible deficiencies of his own presentations. After wading through his puzzling text, after repeated queries addressed to him (to some of which he kindly replied), and after receiving answers to pertinent queries from others who labored through his article,[3] a review is now due. Novices in the field might welcome my clarification of elementary concepts.

Every test of an association between planetary positions and births of professionals requires *expected* birth frequencies along the planet´s daily path, subdivided among planetary sectors 1 through 12.[4] These serve as a reference to evaluate *observed* birth frequencies in individual sectors or sector zones (combinations of sectors). One broad consensus, encompassing both detractors and supporters of the Mars effect, exists on the rationale underlying the necessary calculations: Expected birth frequencies must accommodate *astronomical* and *demographic* factors. These two sources of intrusive variance, not at first familiar to many readers, will become intelligible by using an easier to understand analogous problem invented for the purpose.

2. Astronomical Factors

The Gauquelin Approach

Suppose someone claims that eminent *singers* are more often born between sunrise and sunset than would be expected by chance. Doubt prompts us to test this claim, so we collect birth data (date and hour) of $N = 535$ singers. Note

[2] Dommanget (1997) does not refer in his article to his correspondence with astronomer George Abell who apparently did understand CP's model and who was the first to discover certain flaws in it. Abell, in a letter to Dommanget, dated October 16, 1982, wrote, "I must say that I have read and reread your paper, and I think I understand it but am still perplexed about a matter, as I shall explain below."

[3] I am grateful for Geoffrey Dean's, Ivan Kelly's, Ken Irving's, and Marcello Truzzi's suggestions for improvement.

[4] Due to the earth's rotation—one turn in 24 hours—Mars seems to circle the earth. The planet's full circle of 24 hours, seen from, say, Brussels, can be divided into 12 sectors: six above and six below the horizon. For example, champion Jean de Bie was born in Brussels at 17h00, while Mars, seen from his birthplace, was in sector 9. Had de Bie been born at the same time (17h00), but 4 months later, he would have had Mars in sector 10. Another 4 months later, again at the same time, Mars would have been in sector 11, etc.

that the claim of our fictional interest refers to only two *solar* "sectors" (roughly *day* and *night*), instead of to 12 *Mars* sectors, as in CP´s research on sports champions, which demands more attention.

What do we expect by chance? Depending on the calendar day, the sun can be above the horizon for periods of varying duration between very short, roughly 8 hours (winter minimum), and very long, 16 hours (summer maximum), assuming geographical latitude of 50 N. For each singer's birth date and location, we therefore need to know at what time the sun rose and set. This information is needed to calculate sun-above-horizon proportions for each individual birth date, i.e., the probability ${}_a p_{up}$ of the sun's presence above the horizon (subscript *a* for "astronomical" and *up* for "above horizon"):

$$ {}_a p_{up} = (h_{up})/(24) \tag{1} $$

h_{up} is the difference between the sun's set and rise time on an hourly scale. Probabilities ${}_a p_{up}$ are diurnal (daily) proportions of sun-above-horizon periods. They vary, taking the above h_{up} range 8 to 16 hours, between 0.33 (winter minimum) and 0.66 (summer maximum). For each singer's birth date, we obtain one sun-above-horizon proportion. Summing them over N = 535 birth date occurrences, we obtain the *expected* number of births with the sun *above* the horizon, ${}_a N_{up}$.

$$ {}_a N_{up} = \sum_{j=1}^{535} {}_a p_{up_j} \tag{2} $$

The expected number of births with the sun *below* the horizon is simply ${}_a N_{down} = 1 - {}_a N_{up}$. Equation 2 accommodates the *astronomical* factor (index *a*) arising in our project from the sun's season-dependent variation of its daily circle. *Demographic* complications are brought in below.[5]

Michel and Francoise Gauquelin´s method to obtain expected birth frequencies for *planetary* sectors accommodates astronomical factors in the same way as our example, though using Mars, not the sun, and dividing the diurnal motion into 12 sectors, not two as in the case above. Critical astronomers and statisticians (Abell et al, 1982; Couderc, Porte, Rawlins, 1978), most of them members of skeptical committees, have approved of the Gauquelin procedure, in some cases after subjecting it to detailed scrutiny supported by additional studies and data (e.g., Rawlins, 1978).[6]

[5] In view of problems with CP's approach, to be discussed, it should be emphasized again that the total of expected births of a sample requires one probability for each individual of the sample.

[6] Details on Gauquelin's algorithm are provided by Gauquelin and Gauquelin (1957), Gauquelin (1988a, p. 203–217), and Gauquelin (1988b).

CP's Approach

CP's researchers, however, contest Gauquelin's rationale underlying his expectancy calculation. Their arguments are puzzling, and unraveling them requires space and time.

CP's model is based on a matrix, an arbitrary and unusual construction. The matrix comprises six rows and nine columns, i.e., $n = (6) \times (9) = 54$ cells, whose entries are so-called *configurations*, tagged $\boldsymbol{C_k}$ (see their table 2, Dommanget, 1997, p. 285). Configurations are frequencies of Mars in 54 combinations of Mars and Sun daily positions. They are calculated for the first of each month of the experimental period, i.e., for the 888 months making up 1872–1945.

Configuration, by the way, is a misleading term to denote what is actually a *cell unit* in the matrix, just as the term *wall* would be misleading to denote a brick.[7] Readers become confused all the more because Dommanget occasionally uses the term *configuration* to denote the 12 sectors of the daily Mars circle (which is quite different from what is represented in table 2). In such cases, *configuration* would mean "the set of the twelve classes [*class* is Dommanget´s unusual term for *sector*]" for any particular date (Dommanget, 1997, p. 283).

Why did CP set up table 2 at all? We will postpone this question until we are through with all necessary technicalities.

Our understanding of Dommanget's table 2 is facilitated by creating an analogous table for the singers project. We follow his rules and sort all sun-above-horizon periods, observable within the time span 1872–1945, into eight intervals (irrespective of whether the singers' birth dates actually occurred in the sun-above-horizon periods) starting with the shortest possible 1-hour period 08h00–08h59, 09h00–09h59, etc. up to the longest, 15h00–15h59. Next, we partition each year of the observational period, 1872 - 1945 = 74 years, into 12 calendar months (January, February, March, etc.). A matrix of 8 rows (representing eight sun-above-horizon periods) by 12 columns (representing 12 months) is thus established, comprising $n = (8) \times (12) = 96$ cell units C_k (k equals 1–96).

Next, following Dommanget, the cells of the matrix are filled with counts of days. One day from each month of the observation period is entered into the table; Dommanget selected the first of each month, so we follow suit. To give an example, on January 1, 1872, the sun was 8h07 above the horizon (obtained from computer software or from an ephemeris), so we put 1 in the first column for January and first row for 8h00–8h59. The last entry is December 1, 1945 (column 12, December and row 1, 8h00–8h59 sun-above-horizon duration on January 1, 1945). The table eventually contains $(12) \times (74) = 888$ days distrib-

[7] "Matrix C_k is not a configuration. Each of its cells is a configuration" (Dommanget, 1995).

uted among 96 C_k cells comparable to Dommanget's sample of 888 days, which in his case were distributed among 54 C_k cells. In Dommanget's table 2, *Mars´-stay-in-sectors periods* (rows) correspond to the *sun-stay-above-horizon periods* of our singers table, and his *Mars–Sun distances* (columns) correspond to our months. To make the analogy more explicit, *month* may be roughly conceived as *distance between the sun's present and its winter solstice position on the ecliptic.*[8] These technicalities are needed to understand the essentials of CP's procedure.

Table 2 of the singers study, when filled with day counts, shows that sun-above-horizon periods are longest in summer months and shortest in winter months with gradual changes between them. We are aware of seasonal variation, of course, without consulting this matrix.

Many people, however, are not aware of Mars' celestial motions. They will discover in table 2 of CP's study, when filled with day counts, that Mars-sector periods manifest lawful variation no less than the sun's "sector L" periods. That is, Mars, too, has "seasons," although roughly twice as long as solar seasons.

This might be interesting for novices in celestial motion. But how to get from here to expectancies for births in Mars sectors? CP´s model, taking shape with their formula 6, was constructed to somehow transfer C_k counts onto sectored birth expectancies (Dommanget, 1997, p. 286). Their figure 5, a pictorial aid with many arrows, is introduced to make the intended transfer visually persuasive.

But this figure is puzzling, not persuasive, for three reasons. First, to arrive at expectancies for births in Mars sectors, there is no need to adopt CP´s C_k statistics. In my view, which I share with CSICOP astronomer George Abell,[9] CP researchers are forcing superfluous data about celestial Mars movements on human birth expectations. Resuming the singers study, we can straightforwardly and safely state that the sun-above-horizon proportion, e.g., for May 2, 1903, when Bing Crosby was born (4 p.m., in Tacoma, Washington, at 47.14N, 122W26) is determined solely by sunrise and sunset in Tacoma on May 2, 1903. Secular (seasonal) changes of the sun's rise and set before and after Crosby´s birth date, i.e., the day count statistics of Dommanget's table 2 over 74 years of observations, are entirely irrelevant for the sun-above-horizon period on Crosby's birth date and place. The same applies to Mars-in-sector pe-

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[8] In one respect, $\mathbf{C_k}$ entries (day counts) for CP's champions project (Mars) differ from C_k entries (day counts) for the singers project (sun): CP's Mars day counts in the champions project vary on the secular time scale: Analogous sun day counts for the singers project change within years only, not over the years. This, however, is of no account here.

[9] Abell (in a letter to Truzzi, November 10, 1982) already pointed at the irrelevance of pC_k.: "The pC_k...is simply the probability of obtaining a particular configuration of Mars in the sky at the time of the athlete's birth. I think it may be relevant if one assumes a statistical average for Mars distributions, but not if the *theoretical* frequencies are calculated, as I have indicated in a letter to him."

riods and sports champions. CP's model, in a generalized perspective, amounts to a nonsensical claim, namely that effect $E_{t,}$ caused by C_t at calendar time *t,* is affected, aside from C_t, by $C_{a,}\ldots C_{t-3,}\ C_{t-2,}\ C_{t-1,}\ C_{t+1,}\ C_{t+2,}\ C_{t\pm3,}\ldots$ + $C_{z,}$ i.e., by the entire time series t_a through t_z of continuous C_i occurrences, past and future, of which C_t is one temporal slice in the middle.

Second, understandably therefore, CP's model cannot be put into operation. The transferring device of their formula 6 is incomplete. It does not and cannot indicate how C_k—the information in their table 2—should be translated into expectancies. The arrows in Dommanget's figure 5 (1997) are misleading pictorial substitutes for a missing formalization, which in turn seems to be due to a confused conceptualization.

Third, we have shown above that calculations of expected birth frequencies for N = 535 champions must be based on Mars-in-sector durations for each champion's birth date. In CP's model, however, expressed by their formula 6, no link to those 535 individual birth date parameters exists (index *i* refers to sectors, index *k* to day counts of their table 2, there is no index for individuals):

$$p_i = \sum_{k=1}^{54}((pC_k) \times (p_i \mid C_k)) \qquad i = 1, \ldots, 12,\ k = 1, \ldots, 54.$$

For each of 12 sectors *i,* one probability p_i is formalized by summing over 54 C_k-based probabilities instead of across 535 person-based probabilities.

What purpose does the expression $p_i|C_k$ in CP's formula 6 serve? Apparently, it should bring *k*-indexed C_k-day counts and *i*-indexed Mars-sector observations together. But readers are not told to which observations $p_i|C_k$ refers, and they will hardly dare to question this construction, which Dommanget refers to as "well known" even though no one can have encountered an odd formula such as this one before. More discussion was needed to fully dissolve the intricacy. The main point is that in CP's model, no variable referring to N = 535 birth dates is provided on which, as pointed out above, expectancy calculations must be based. Whatever the model may be, N individual expectancies must be summed and they must add up to some value around 535/12 = 44.5.

Why did CP ignore individual birth date information? Dommanget's answer to this brings deeper factors to light. He holds that a researcher who wants to "compute the theoretical histogram[10]...is not allowed to make use of the sample itself" (Dommanget, 1997, p. 279). For him, using "the sample itself to establish the reference diagram [is a] disadvantage" (Dommanget, 1997, p. 292). This is the exact opposite of what proper methodology demands: Pertinent information obtained from *the sample itself*—i.e., from each

[10] Dommanget's terms "theoretical histogram" and "model of the mechanism" sound substantive and significant, yet they are ambiguous. Replacing "theoretical histogram" with "expectancies" increases conceptual precision.

individual in the sample—is neither a disadvantage nor an advantage, rather it is an indispensable source of reference. Expectancies are required for an actual sample and not, say, for the entire world population. This goes without saying among experts calculating expectancies, whether by randomization, Monte Carlo, or by bootstrapping procedures, etc. Being based on misconceived statistical presumptions, CP's model is not appropriate to accommodate birth-related astronomical factors.

3. Demographic Factors

The Gauquelin Approach

Aside from astronomical variance (a), the average variance of birth frequencies over hours of a day (d) must be considered: People are not born with uniform frequency day and night. Hourly birth rates for natural births generally peak before sunrise, decline during the daylight hours and are lowest in the afternoon.

This so-called *demographic* or d factor, however, is easily accounted for with the help of average diurnal birth counts, occasionally called *nycthemeral* distributions. Let us use the distribution of ordinary people from Gauquelin studies (N_{op} = 24,614)[11] and calculate from them expected birth rates as needed. For our simplified singers example, the question in need of an answer for each birth is this: Which proportion of a day's general birth output is obtained between the time of sunrise and sunset occurring on the subject's birth date? This is the *demographic* birth proportion required to adjust the *astronomical* birth proportion as explained above.

We proceed as follows. Above we obtained, for Bing Crosby's birth date May 2, 1903, a sun-above-horizon proportion of ${}_{a}p = 0.60$. On that day, the sun rose at 5:53 a.m. and set at 8:18 p.m. Next, we take birth counts of ordinary people, i.e., for the time section between 5:53 a.m. and 8:18 p.m. (sun-above-horizon considerations are here irrelevant), and find that this temporal section contains 59.5% of the total. Thus, the probability for a person to be born on May 2, 1903, with the sun above the horizon was ${}_{a|d}p = 0.595$. In a similar fashion, just as for Crosby's birth date, we obtain ${}_{a|d}p$ values for each singer of our sample. The expected number of births with the sun above the horizon, demographic condition adjusted, is obtained analogous to Equation 2, by

$$ {}_{a|d}N_{\text{up}} \sum_{j=1}^{535} {}_{a|d}p_{\text{up}_j}. \tag{3}$$

The expected number of sun-*below*-horizon births is simply ${}_{a|d}N_{\text{down}} = N$ -

[11] The Gauquelins collected the data for their so-called heredity studies attempting to test possible similarities of planetary positions between parents and their offspring.

${}_{a|d}N_{up}$. Equation 2 considered astronomical factors only while Equation 3 takes into account astronomical *and* demographic factors. Equation 2 served merely as a didactic first step.

We have made it clear how Gauquelin's expectancy algorithm works in principle, using singers and two simple solar-sector expectations. For sports champions and 12 Mars-sector expectations, the same principle applies. Additional complications are few and can be passed over here.[12]

CP´s Approach

How does CP's "model of the mechanism" accommodate *demographic* factors? Surprisingly, this model (their formula 6) ignores demographic conditions. This is puzzling because Dommanget (1997) actually refers, in passing at least, to "taking into account the nycthemeral distribution" (p. 285). He seems to make readers believe that demographic variation has been incorporated into the model. He even refers to Gauquelin's nycthemeral birth curve (Dommanget, 1997, figure 4) *as if* CP's formula would account for it, but it does not.

Moreover, remember that Dommanget's main objection to Gauquelin's algorithm points at allegedly wrong *demographic* assumptions, not wrong *astronomical* assumptions, i.e., CP objects to the neglect in Gauquelin's algorithm of secular inconstancies of the diurnal birth rhythm.

But on close inspection, one finds that CP's criticism of Gauquelin's demographic assumptions does not result from their formalized model. Rather it is a mere verbal patch to cover a hole in what CP is really saying. In view of CP's verbalized concern with demographic factors, no one would expect that these factors are entirely neglected in their own formula. Dommanget's summary of CP's position turns the facts upside down: *"[CP established] a model for the theoretical mechanism of the purported phenomenon. [From this analysis,] it clearly appears that the theoretical principles proposed by Gauquelin to support his research have to be rejected because they do not correctly take into account the fundamentals of the problem—the secular and diurnal sociodemographic factors"* (Dommanget, 1997, p. 275f). However, CP's "theoretical mechanism" cannot logically provide any grounds to reject Gauquelin's procedure. The reason is that it is unrelated to sociodemographic factors.

Can CP's "only correct formula" (Dommanget, 1997, p. 290) be put to a test? This is a reasonable question, and a straightforward response would be to just test its expectancy output. Yet, here we encounter an astounding deficiency. An expectancy output from CP's formula has never been provided, despite early and repeated demands, especially by Gauquelin (1982): "He [Dom-

[12] Unfortunately, Dommanget's account of the Gauquelin procedure is as unintelligible as that of his CP procedure.

manget] should publish his own theoretical...distribution, [but] he *never* did so" (p. 77). Dommanget, when challenged by such demands, used to object that expectancy computations are "unfortunately impossible," since according to CP's model, all factors contributing to diurnal birth curve variation must be considered, but that some/many factors are "unknown" (Dommanget, 1997, p. 294).

But we may simplify the test by creating a fictitious sample without any diurnal birth curve variation, assuming that birth frequencies are evenly distributed over 24 hours of the day and that no secular variation occurs. What is the expectancy output of CP's "only correct formula" for the simplest of all conceivable conditions? There is no such output. CP's formula manifests its uselessness convincingly under most favorable test conditions.[13]

CP´s Main Objection to Gauquelin´s Demographic Conception

CP was concerned that observed birth peaks on a Mars-sector scale might be due to secular variation of diurnal birth distributions. It must be conceded that such demographic artifacts, even though improbable, are at least conceivable. The CP's concern over them does not logically result from their model, but one may look at them regardless. So I checked CP's claim, but on close examination of their criticism of secular variation, I did not find any evidence for it. In contrast, elementary logic and empirical facts contradict it, as is shown in what follows.

First, recall the Gauquelin claim that whenever deviations of birth counts from chance occur, they invariably do so in key sectors 1 (rise of the planet) and 4 (culmination), irrespective of planet (Mars, Jupiter, Saturn, Moon, etc.) and profession (champions, actors, physicians, etc.). CP's suspicion that Gauquelin planetary effects might be due to secular demographic variation thus implies the contradiction that *invariable* deviations of birth counts in *invariable* sectors arose from *variable* and unpredictable demographic causes. This is like claiming that weekly maxima of accident cycles on Saturdays were due to weather factors. No doubt, the weather is known to affect accident occurrences, but its influence for particular weekdays is entirely unpredictable.

The second argument is empirical. Dommanget, aside from *ignoring constancies* of birth frequencies for Mars sectors 1 and 4, tried to *emphasize inconstancies* of birth frequencies over Mars sectors 1 to 12. He used his figures

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[13] Dommanget (1997) attempts another way out of CP's dilemma: "It is surprising that nobody seems to be aware of the need for computing the theoretical histogram" (p. 290). However, Abell computed a theoretical histogram (expectancies) in 1982 (Dommanget received the result by letter), Gauquelin published more than a dozen volumes of data including expectancies, and Müller, Ertel, CFEPP, Nienhuys, and Pottenger computed expectancies that agreed quite well with one another. Dommanget is aware of at least some of these successful efforts. CP is the only party not providing expectancies. Dommanget thus turns crucial facts upside down, thereby misleading inattentive readers and puzzling attentive readers.

9 through 11 to demonstrate how the distribution of births over Mars sectors varied over the years. In his figure 9, e.g., we find a breakdown of birth rates over Mars sectors for three successive secular periods. From eyesight, Dommanget (1997) concludes that “there is a dependency of the results from the secular sociodemography” (p. 290), meaning that (a) over secular periods, birth frequencies for Mars sectors are inconstant, and (b) this is caused by corresponding secular variation of daily birth distributions.

Dommanget´s focus is on Claim 1 only, the alleged effect. He does not consider Claim 2, the alleged cause, which is a remarkable omission. Worse, he actually fails with both claims, as shown in Figures 1a and b. Figure 1a represents birth rates of CP champions on the Mars-sector scale after a breakdown of the total into Dommanget’s three successive cohorts. His figure 9 (Dommanget, 1997, p. 291) showed the same three curves, though side by side and small, which impedes perceiving invariant features. Such features manifest themselves clearly by an overlay display, as in Figure 1a: Peaks in Mars key sector 1 are invariably predominant. The synchrony of key sector 4 peaks is also present, although less conspicuously. More to the point, the three birth count distributions are significantly correlated among each other (by Spearman’s rank correlation r_s):

$$r_s(1{:}2) = .63, \quad p = .02, \quad N = 175$$
$$r_s(1{:}3) = .51, \quad p = .04, \quad N = 182$$
$$r_s(2{:}3) = .18, \quad p = .28, \quad N = 178$$

In view of the smallness of the three subsamples, each subdivided over 12 sectors (leaving on the average circa 15 champions per sectorial unit), the indication of stability in Figure 1a is remarkable.

Dommanget did not calculate such correlations, and what he did instead actually veils them. For his figure 10 (p. 291), he doubled the subdivisions of the same data (using six cohorts instead of the three of his figure 9), thus throwing even more random noise into his readers’ eyes, with no additional information provided.

As noted above, Dommanget claims that Mars-sector variation (the alleged effect) is due to diurnal birth time variation (the alleged cause), but he did not actually investigate the latter. Making up for this neglect, we show pertinent results in Figure 1b. For the three Dommanget cohorts, birth frequencies are plotted over time of the day. Dommanget would predict that variation of birth rates over time of the day is more marked than variation of birth rates over Mars-sector position, because causal oscillations should be stronger than effected oscillations.

Figure 1b, compared with Figure 1a, however, does not bear this out. Birth

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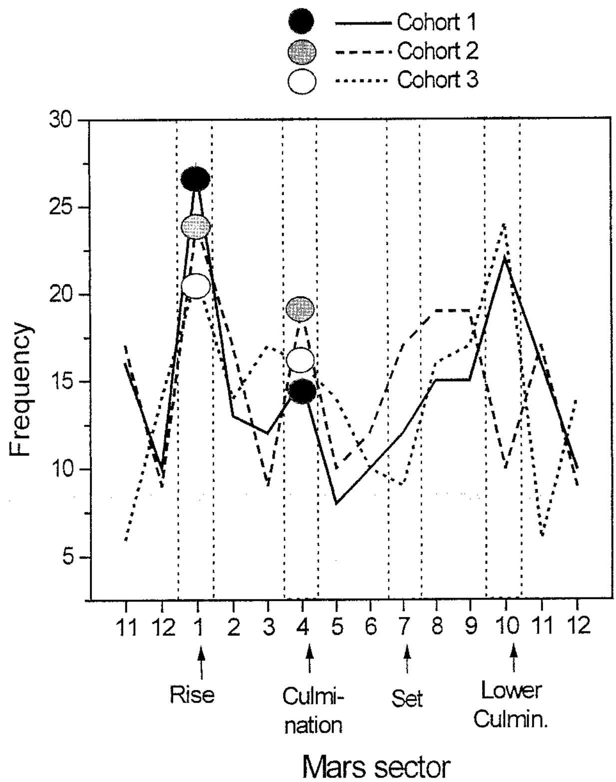
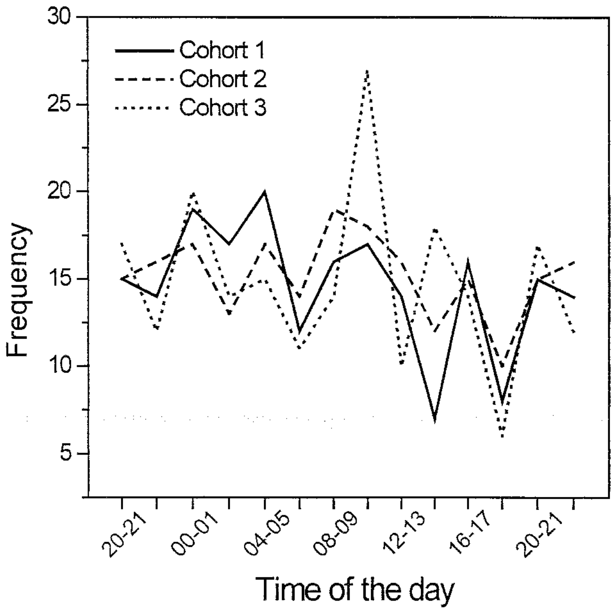
Fig. 1 Birth counts of CP’s athletes, separately for three CP-defined successive cohorts, (a) across 12 Mars sectors, (b) across 12 daytime periods.

(a)


Cohort 1
Cohort 2
Cohort 3
Frequency
30
25
20
15
10
5
11 12 1 2 3 4 5 6 7 8 9 10 11 12
Rise
Culmi-
nation
Set
Lower
Culmin.
Mars sector

(b)


Cohort 1
Cohort 2
Cohort 3
Frequency
30
25
20
15
10
5
20-21
00-01
04-05
08-09
12-13
16-17
20-21
Time of the day

rate variation is smaller, not larger, for hours of the day (Figure 1b) than for Mars-sector position (Figure 1a). It is inconceivable that a strong effect (represented by ragged *Mars–diurnal* (sector) distributions with large peaks and troughs) could ever result from a weak cause (a smooth *solar–diurnal* distribution). Only one of 3 × 12 = 36 counts is conspicuous, a statistical fluke probably. Dommanget (1997) concluded, again from mere eyesight, not from measurement, that there exists a "dependency of the shape of the histogram [births over Mars sectors] on the secular distribution of the births dates." This, he concludes, "confirms the role of the secular sociodemography" (p. 294). Figures 1a and b show that his conclusion is wrong. The evidence leads to the opposite conclusion.

4. Missing Result: Expected Birth Frequencies

As noted above, CP's work of 1967–1997 did not culminate in the publication of expectancies. Surprisingly, however, as early as 1970, before inventing their "model of the mechanism," CP actually *did* calculate expectancies using a widely accepted method. However, they did not publish these results. In his "rough historical sketch," Dommanget skipped this period of CP's intensive research. Why did he leave this out?

Let us first look at CP's unpublished expectancies (see Figure 2). The solid line in Figure 2 shows, first, *observed* birth rates of CP's champions. Full circles represent CP's *expected* birth rates. These were obtained by shifting the birth time from each actual champion to the next in line when listed alphabetically. Birth year, month, and day remained unchanged. Full circles in Figure 2 represent the average sector positions of nine such successive shifts. They are taken from an unpublished Dommanget document.[14]

CP's expectancies in Figure 2 may be compared with expectancies obtained by Gauquelin's classical technique (explained above). This technique yields birth rates of a fictional control group whose size equals that of the experimental sample (small open *circles*). Small open *triangles* represent expectancies obtained by using another strategy. Expectancies were here obtained by summing sector positions of ordinary people (averages of a noneminent population). Ordinary people may be regarded as a genuine control sample, which, however, is much larger than the experimental (champions) sample. (The *Zelen test* method is more precise, but of the same kind.) It can be seen that expectancies by CP's shifting procedure hardly differ from those obtained by the two other methods.[15]

[14] Source Dommanget, "Tableau extrait d'une note de M. Dommanget," 1970, an unpublished appendix of a letter by de Marré, a copy of which was transmitted to Gauquelin, Hoebens, Hövelmann, and Ertel.

[15] CP's randomized expectancies show larger variance than the other two, which apparently indicates that nine randomizations are insufficient for obtaining an average curve as reliable estimate. The CFEPP ran 80 randomizations; Ertel, 200.

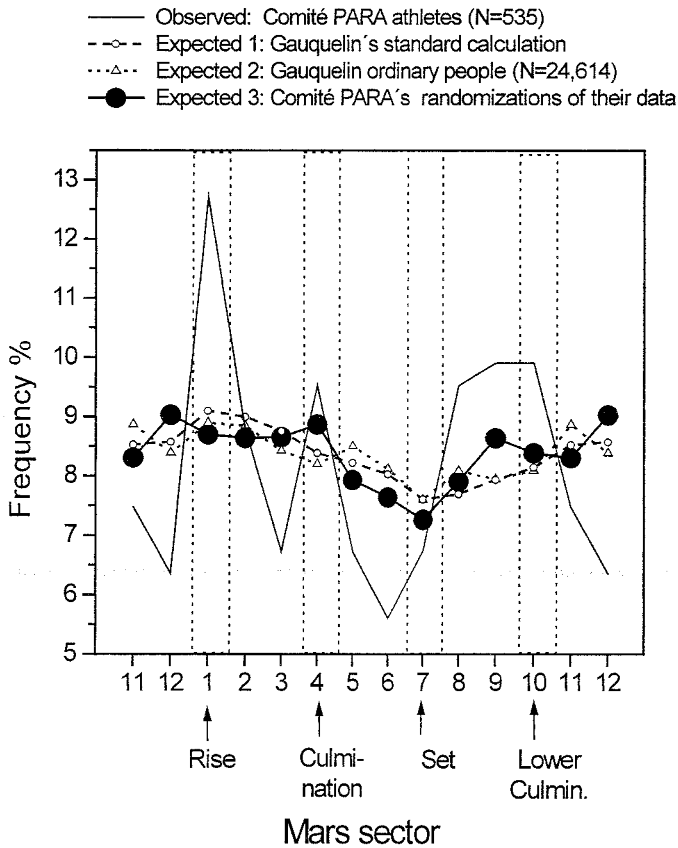


Fig. 2 Observed birth counts (%) of CP's athletes across 12 Mars sectors and pertinent expectancies, CP's unpublished ones included.

The Mars effect controversy is certainly worth being settled and CP could help at that by clarifying the following details of their investigation:

1. In his paper, Dommanget (1997) contends that the results of CP's randomization experiments, *counterexperiments* using his terminology, "were ready for publication in the years 1977–1978 in our N.B. No. 44" (p. 292). But documents exist showing that Dommanget had tabulated the results of these experiments *already by 1970*.[16] CP member Luc de Marré expressly confirms this in a letter to Kurtz (November 8, 1975). He informs Kurtz that the counterexperiments were conducted "be-

[16] The table was attached as a copy of a letter (April 21, 1975) by CP's member Luc de Marré addressed to Jean Dath, chairman of CP at that time.

tween 1967 and 1970." De Marré complained that "during three years [1970–1973], the PARA committee refuses to publish the results of this investigation," and he resigned from the committee in protest.

2. According to Dommanget (1997), CP's counterexperiments "were conducted by PARA...*on the basis of its theoretical conception of the problem* [italics added]" (p. 292). This is puzzling for two reasons: First, as just noted, the counterexperiments were conducted right after CP's data collection in 1968. Their theoretical model was developed later, around 1973–1975 (de Marré). *CP's experiments thus cannot have been conducted on the basis of CP's "theoretical conception of the problem."* Second, following Dommanget, CP's theoretical conception of the mechanism contradicts the assumptions on which their own counterexperiments are based, i.e., CP's own experiments have "the disadvantage of using the sample itself to establish the reference diagram" (Dommanget, 1997, p. 292). Even on logical, not only temporal grounds, therefore, the committee's model cannot have served as the *basis* for their counterexperiments.
3. The results of CP's counterexperiments, which were "ready for publication in 1978 (N.B. No. 44)," were not published, according to Dommanget (1997), "because no one seemed to accept our analysis of what was needed to understand these experiments" (p. 292). However, the French skeptics (CFEPP), Nienhuys, and Ertel, all used the CFEPP´s early technique independently without any consideration of the CP model of 1976. An acceptance of this model is not needed for understanding and applying their procedure of 1970.
4. According to Dommanget (1997), research on *secular variation* (shown in his figures 9 through 11) was part of the CFEPP's "research conducted since 1976" and this "was not communicated to anyone" earlier because "they would have needed an understanding of the mechanism of formation of the histogram expressed by our model" (p. 289). Eventually in 1997, while publishing this research, Dommanget apparently credits his readership with somehow understanding their difficult mechanism.

   But why does this author still exempt from publication the results of those counterexperiments of 1968–1970? We were told that they had not been published because, in a similar vein, the CP deemed the public to be too uninformed to understand them. But now, although crediting the present readership with being capable of understanding CP's idea of secular variation, Dommanget withholds the results of CP's counterexperiments, which in 1976 became model inconsistent. *Can anyone be blamed for gaining the impression that CP swept its model-inconsistent results of 1970 under the carpet, concerned over a possible detection?*

Dommanget should be prepared to dissolve the above contradictions and make all pertinent facts transparent. This would be in keeping with his own philosophy of science, because he contends that "the committee is only interested in seeing science progressing on a firm, stable, and rocky basis, whoever could be right" (Dommanget, 1982b, p. 66).

5. Conclusion

Dommanget (1982a), in published and unpublished Mars effect disputes, often challenged researchers either to accept the Belgian committee's model or to "indicate without possible doubt… on which precise point this analysis could appear erroneous" (p. 74). Our scrutiny of CP's work has revealed quite a few points of error. The conclusions to be drawn from those flaws have been anticipated by previous authors, to whom due credit is given in what follows:

1975: R. Chauvin (professor of biology at Sorbonne, in a letter to J. R. Dath, April 21, 1975, then chairman of CP) said, "I conclude:

- You did observe the same significant results as M. Gauquelin…
- You did not calculate the theoretical expectancies…
- Your counterexperiment apparently removes the two main objections that you raised to Gauquelin's method."

In 1975, L. de Marré (just-resigned member of CP, in a letter to Kurtz, November 8, 1975) said, "It looks as if the committee's majority, conducted by its president, were seeking, under pseudoscientific pretexts, to hide a fact that they themselves have verified…: the relation existing between Mars and the champions."

In 1982, G. Abell of CSICOP, U.S. skeptics, astronomer, in a letter to M. Truzzi (July 21, 1982) said, "I admit that I cannot understand CP's [negative] points about [Gauquelin's] expected distribution in light of the [positive] results of the Zelen test [data comparable to Gauquelin's ordinary people data]. I think they [CP's objections] are just wrong."

In 1982, M. Gauquelin wrote, "Why did Dommanget never publish his own expected… [birth] frequencies? Many people asked him, too. [His]… answer: 'Such calculations...were never done...it is impossible.' The so-called mathematical model… was created to hide the Mars effect and to mislead the general reader" (p.68).[17]

1998: J. W. Nienhuys (Dutch skepter): Nienhuys considers possible variation of theoretical expectancies, as might occur by Dommanget's secular factors, as minimal ("unimportant") (here visualized in Figure 1b) in view of

[17] For me, it is surprising that Gauquelin did not look deeper into CP's misconceived model. His position would have been considerably stronger had he demonstrated its absurdity in detail. Abell made the first unraveling steps, which Gauquelin missed to continue. Dommanget's note to Truzzi on September 20, 1982, was apparently true: "We know that M. Gauquelin never understood the real significance of what we call the pC_k." Understanding pC_k means piercing an absurdity veiled by some stupefying formalism.

those large ups and downs of observed key sector frequencies, as were manifest in the data of the Belgian committee (see Figure 1a). Nienhuys' own reservation about the Mars effect has different grounds (Wunder, 1998, p. 82).

1998: E. Wunder (German skeptics GWUP): "An answer to the question whether the Mars effect does or does not exist does not necessarily presuppose the solving of the theoretical problem brought forward by Proffessor Dommanget [S.E.'s translation]" (Wunder, 1998, p. 82).

The CP's approach has in fact never been endorsed by anyone, not even by skeptical committees outside Belgium. The present review aimed to show in hindsight, and in detail, why the above-quoted reactions were justified. Nevertheless, CP's Mars effect research, as well as efforts by other skeptical committees on this issue, should be acknowledged. Their continuous resistance to the Mars effect claim has challenged rigorous tests by Gauquelin, Müller, and Ertel (see Ertel, 1998, 1999; Ertel and Irving, this issue). The planetary anomaly, if it were due to mere imagination, would certainly have disappeared under such scrutiny, but it did not disappear. For readers entering this arena for the first time, the dynamics of this controversy might be helpful to understand the challenge of this phenomenon, which deserves extensive research in the future.

References

Abell, G. O., Kurtz, P., & Zelen, M. (1982). The Abell-Kurtz-Zelen 'Mars effect' experiments: A response. *Skeptical Inquirer, 7(3),* 77–82.

Committee PARA. (1976). Considérations critiques sur une recherche faite par M. M. Gauquelin dans le domaine des influence planétaires. *Nouvelles Brèves*, 43, 327.

Dommanget, J. (1982a). Comments on Patrick Curry's research on the Mars effect. *Zetetic Scholar, 9,* 73–74.

Dommanget, J. (1982b). On the Mars effect. A last answer to M. Gauquelin. *Zetetic Scholar, 10,* 66.

Dommanget, J. (1995). Personal communication.

Dommanget, J. (1997). The "Mars effect" as seen by the committee PARA. *Journal of Scientific Exploration, 11*(3), 275–295.

Ertel, S. (1992). Mars effect survives critique of Dutch skeptics. In Nienhuys, J. W. (Ed.), *Science or Pseudo? The Mars effect and other claims: Proceedings of the third EuroSkeptics congress* (pp. 185–203). Utrecht: Skepsis.

Ertel, S. (1998/99). Is there no Mars effect? *Correlation, 17,* 4–23.

Ertel, S., & Irving, K. (2000). The Mars effect is genuine. On Kurtz, Nienhuys & Sandhu´s missing the evidence. *Journal of Scientific Exploration, 14,* 421–430.

Gauquelin, M. (1982). A reply to the statement of committee PARA. *Zetetic Scholar, 10,* 75–77.

Gauquelin, M. (1988a). *Written in the stars.* Wellingborough, UK: Aquarian Press.

Gauquelin, M. (1988b). Is there a Mars effect? *Journal of Scientific Exploration, 2*(1), 29–52.

Gauquelin, M.F., & Gauquelin, M. (1957). Méthodes pour étudier la répartition des astres dans le mouvement diurne LERRCP, Paris.

Rawlins, D. (1978). Memorandum on the relation of Mars' solar proximity to Michel Gauquelin's Mars–sports results and claims. *Phenomena, 2.3-2.4,* 22.

Wunder, E. (1998). Reply to Prof. Dommanget's letter to the editor. *Skeptiker, 11(2),* 82.