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ABOUT DOGMA IN SCIENCE, AND OTHER RECOLLECTIONS OF AN ASTRONOMER

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DEFINITIONS

The notion of dogma, or the unquestionable acceptance of certain propositions—the doctrines—is usually associated in our minds with religious (or antireligious) beliefs. Yet it has a much wider application. However alien to science, and not widespread there, it *de facto* sometimes infiltrates the realm of research. Usually based on some recognized authority and accepted in a group or "mini-establishment" of true believers, scientific dogma lacks the punitive aspects of its religious counterpart and therefore is open either to ultimate destruction when it is proven wrong, or to logically justified acceptance when finally it is vindicated by facts. Dogma differs from a hypothesis by the refusal of its adherents even to consider the aspects of its validity. Legitimate disagreement or controversy creates dogma when arguments are no longer listened to. Although usually belonging to the realm of theoretical models where direct experiment (or observation) is not possible, dogmatism may sometimes induce its followers to misquotation or misrepresentation of the most undisputable facts, even of the statements made in print by their opponents: if the statements themselves may be subject to doubt or erroneous, the fact of the printed word is indisputable. As shown in the following, my astronomical experience has met several examples of such a "prejudiced blindness." In any case, these misquotations are not intentional but seem to be caused by a specific "dogmatic" superficiality, something like knowing in advance what the other fellow would say and therefore not listening to him, or not reading properly his work.

A UNIVERSITY TREK TO CENTRAL ASIA

Nevertheless, sometimes the dogmatic counterpart may listen, and even may see the light. In the following, a dramatic episode of this kind is described.

In the beginning of 1919, when Bolshevik rule was firmly established in most of Russia, with some fighting still continuing on the fringes, the new rulers decided to found a university in Tashkent, the capital of the just reconquered Russian possessions of Central Asia (so called, although they are rather in the West of the continent). Over 100 professors and other teaching staff with their families volunteered to leave starving Moscow and to start a new life in the food-rich but otherwise risky Asiatic surroundings. As the only astronomer in the group, I was to be Chairman of astronomy and put new life into Tashkent Observatory. A former military geodetic observatory, it had been reorganized by V. V. Stratonoff, but after three decades of respectable research activity it had somehow fallen into disarray during the revolution, and it was now to be made an integral part of the new Turkestan University.

Rail communications in Russia were at that time in a state of disorganization, and so it was no wonder that our legendary trek of 3000 km from Moscow to Tashkent took 70 days, from end of January to beginning of April, 1919. The obstacles were chiefly on the first half of the journey; after entering Asia behind the river Ural the sponsorship of the Tashkent authorities helped us to make the second half of the journey in only three days. There was no coal or fuel storage, so we had to saw and split the raw timber for the locomotive ourselves. Beyond the Volga, there are oak forests, and, to our surprise, freshly felled raw oak turned out to be excellent fuel despite its wet appearance! In March, at a small town on the border of Europe and Asia, we were prevented by the local Soviet from proceeding further for a whole fortnight: it was an act of sheer ingratitude. When we arrived in the town, the Soviet, aware of the presence of so many learned persons, asked us to give a lecture and perhaps a variety show to the populace of the isolated township, and we readily agreed. There had just been a splendid display of aurora, with streamers reaching to the zenith, and secret rumours were spreading that these were the artillery flashes of the cavalry of Dutov, advancing from the north to wipe out the Communists (who were not too much liked by the population). So I gave a lecture on aurorae, geomagnetism and solar activity, and then a concert followed. There was quite a good tenor among the professors; I accompanied him at the piano, and other items of entertainment followed.

As a result of our success, the next day we got a letter from the Soviet requesting a repeat of our performance, with a threat that they would not let us proceed unless we complied. We did not bow to the threat and, in protest, refused to deal with them; instead we sat out in our carriages until a strong order from Tashkent forced the local Soviet to lift its embargo. At the next station we again lost three days: our engine with its fuel—the fruit of many hours of our hard work—was stolen by a trainload of Red Marines while we were asleep. After having obtained another engine and prepared the fuel again, we put out sentries overnight (I was one of them), and not in vain: another migrant group approached us at night, intending to take over the fruits of our labour, but they retreated without a fight. The next morning we went on without further adventures—changing engines and preparing fuel, no longer of regular logs but from ragged, thorny, yet very dry saksaoul shrub of the Kirghiz semidesert steppes; "goblins, not fuel" as some of us resentfully remarked. But it burned well and carried us in three days through all Central Asia to our final destination. Before departure, on the meter-thick snow cover, frozen hard on the surface under the intermittent action of the springtime sun and the frost at night, we celebrated in jubilation and I performed a wild improvised dance in long leaps—I called it the dance of the polar bear: my only solo dance in my life.

A CONFRONTATION RESOLVED

After arrival in April, 1919, I served for two years on the faculty of the newly organized Turkestan University, with my main concern being the revitalization of the Tashkent Observatory. Placed on the outskirts of Tashkent at an elevation of only 440 m above sea level, but near the Central Asian plateaus and the highest mountain ranges of the world, the Observatory enjoyed a most favorable astronomical climate. Besides astronomy and astrophysics, it comprised meteorology and seismology as well—the latter being of especial local importance in view of the frequent earthquakes (one night I was almost thrown out of my shaking bed, but the tremor stopped, and I—quite lightmindedly—did not run out into the safety of open space, as the other inhabitants did). The observatory grounds occupied a vast area, and the single detached structures—laboratories (I had a living room alone in the Astrophysics Laboratory), telescope domes, and seismic and meteorological instruments as well as living quarters—were widely spread over the area without a proper view of each other. Most of Russia was already under the Bolshevik (Communist) rule, after the second or Red revolution of October 1917, and this included also Central Asia. In the transition time, a local physics teacher, V. N. Milovanoff, consented to take over the Directorship of the Observatory, and after my arrival he stayed on and became mainly concerned with administration, while I became Vice-Director and looked after the scientific side. The changing circumstances, especially the takeover by the new rulers while law and order were far from being guaranteed over the vast, sparsely populated expanses of Central Asia, created many administrative problems.

Seismology was represented by one person, G. V. Popoff, an athletically-built bearded religious fanatic who lived with his middle-aged housekeeper Maria Abramovna. She looked also after me, in so far as laundry and cooking of lamb chop was concerned; I bought the provisions myself (Tashkent was at that time a cornucopia for food, the envy of starving Russia). Popoff volunteered to meet the visitors, much to our relief, and hundreds flocked to his popular lectures, which were held on open ground. What he was lecturing about, we did not know—he was competent enough to deal with all the aspects of astronomy, meteorology, seismology. Then suddenly we got a shock: a letter came from the local Commissariat of Education informing us that Popoff was preaching religion under the cover of scientific popularizations, and that the people were flocking to his lectures because of their religious superstitions. The letter insisted that this was contrary to the proclaimed antireligious principles of the ruling party and was also nonscientific. Therefore, the letter concluded, Popoff was not worthy of keeping a scientific post and must be immediately dismissed.

We were deeply worried by this. Not only because we believed in humanitarian considerations and the freedom of speech and thought, but also because Popoff was a very much wanted member of the staff whom we did not wish to lose, Milovanoff and I decided to plead for him at the Commissariat. This was indeed a dangerous enterprise, in view of the political situation and the sensitivity of the new rulers to violations of the Marxist dogma, but we did not mind the risk, and it went off quite happily. The Commissar, a young man of about my age (26) named Dvolaitsky, received us in the entrance hall of his office. We pointed out that Popoff was a good scientist and the only seismologist available, and that, while supplies of photographic registering paper were cut off by the revolution, he managed to resuscitate the almost forgotten mechanical method of using smoked paper scratched by the seismograph needle (with levers in between) and thus continued a makeshift uninterrupted registration and recording of Earth tremors. "Well," said the Commissar, addressing me, "you are an astronomer and you should know that astronomy has proven that there is no God." The naïveté of this sentence was obviously genuine—not just an officially imposed party attitude—and seemed to invite discussion. I burst into laughter and explained to the perplexed Commissar that, while belief or disbelief in God is a matter of inner feeling and personal freedom, science is powerless to prove or disprove His existence. We had quite a long talk, and after listening to me attentively Dvolaitsky then decreed: all right, Popoff may remain in office, but he must limit his activity to purely scientific professional functions and he shall not be allowed to lecture to the public any further.

THE AFTERMATH

After a couple of years—I had already left Tashkent at that time for my homeland Estonia—I learned that Popoff was not at all grateful and almost strangled Milovanoff, the Director. Popoff apparently did not appreciate what was done for him and considered Milovanoff the culprit who terminated his lecturing. One day, when Milovanoff descended underground to have a glance at the seismic laboratory, he was accused of "spying" and attacked by Popoff. A passerby heard the screams and rescued the Director.

Many years later, during Stalin's purges of his colleagues, among the lists of comrades—Bolsheviks put to death by their overlord—I recognized the name of Dvolaitsky. The name is uncommon—I never have seen it before or afterwards. It was almost certainly the name of my Tashkent Commissar of Education, a top Communist who listened to reason against the party dogma. Could this have been the cause of his "liquidation" by Stalin?

A WARNING

In 1940/41, at Tartu Observatory, during the first Communist occupation of Estonia, I had another experience along similar lines, although not so dramatic. I gave a course of popular lectures to the public at Tartu Observatory, with philosophical digression into the mysteries of existence and the meaning or purposefulness of the Universe. People flocked to my lectures and demonstrations in about the same manner as to Popoff's in Tashkent, and when I finished my course, I was specially thanked by representatives of the audience for idealistically lifting them up from Earth, nearer to Heaven. Although religion was never explicitly mentioned in my lectures, somebody apparently reported on my idealistic approach, and one evening, while I was outdoors explaining the constellations to my audience, an important-looking young man approached and warned me that I was on a slippery path and that I must avoid the themes of God and religion; otherwise the lectures would have to be stopped. "You know," he said, "many years ago there was an old professor at Tartu who said that when searching the heavens his telescope had never shown him either angels or God." This time I did not burst into laughter—a more sinister threat was behind this naïve sentence. Neither was the course of my lectures interrupted.

DOGMATISM IN SCIENCE

While religious—or antireligious—abstract dogma stands beyond the reach of science, in science itself the dogmatic approach has played, and is still playing, a conspicuous role, but with a difference: the preconceived ideas in science are subject to verification or rejection through further research. They remain "dogma" only as their adherents refuse to consider alternatives to their doctrines, rejecting criticism beforehand. A group of scientists proclaiming such a dogma would thus form an "establishment" or "mini-establishment" (depending on the extent of the group), an extreme case being a single person persistently building on a certain unproven assumption.

Scientific dogma as here defined may prove correct, and even when ultimately disproved, it may serve a useful purpose, as a stimulant to research and the accumulation of facts. Often it may also be harmful, as an obstacle to freedom of research, especially when influencing editors and reviewers of scientific magazines or directors of institutions.

The discovery of America by Christopher Columbus, who underestimated the size of our globe and until his death firmly believed that he had reached the outskirts of Asia, is a case to this effect. It has been often said that he would not have embarked on the voyage had he known the actual distance to be covered. Yet—and the analogy may also hold in research—Columbus had other indications, such as old stories, washed-up twigs of unfamiliar trees, etc., which implied that a continent must exist not too far away in the West. Even knowing the true dimensions of the Earth, he could have concluded that land was not too far away across the Atlantic, and he could still have gone out in search of it, Asia or not.

Scientific dogma or "mini-dogma" is still usually based on some recognized authority whose pronouncements are unconditionally accepted by his followers or, at least, by himself. Newton's laws of mechanics and gravitation are a splendid example of dogma justified by centuries of research and still basically valid, despite the *corrigenda* introduced by the Theory of Relativity. On the contrary, the dogma that lunar craters are volcanoes, which was maintained for so long by the mini-establishment of (chiefly) amateur lunar observers, has been proven wrong, although this does not detract from the value of their observations.

In the following are briefly described some cases of astronomical dogma that are part of my personal experience. Of course, by a kind of "natural selection," only examples of misjudgment or misinterpretation are pointed out, while those instances that proved correct are not mentioned, simply because it is difficult to distinguish between the roles of dogma and of critical research in such cases.

STELLAR STRUCTURE

While Emden produced purely hydrostatic models of gaseous spheres, the genius of Eddington put life into them by introducing the concepts of energy transfer, chiefly by radiation, and of energy generation. Eddington's merits in deciphering the internal structure of stars are incomparable and his work, especially in laying out the physical principles, still serves as a basis for continuing research, despite his failure in explaining the structure of giants. And the failure itself was not in the lack of physical or mathematical methods of approach; everything was already contained in Eddington's own papers and equations. Yet Eddington's models were conveniently assumed to be of uniform chemical composition, which became a doctrine for himself and for the mini-establishment of his followers and which, as we know now, cannot produce "inflated" or giant stellar structures. Against the well-known fact that the recognized nuclear energy source—the conversion of hydrogen into helium—creates a concentration of matter with heavier molecular weight around the stellar core, Eddington produced the von Zeipel concept of forced rotational convection in operation in the hydrodynamically stable radiative-equilibrium layers (1). However, he failed to put numbers into his equations, and when I did it (2), it turned out that the time scale of would-be mixing for the Sun is of the order of 10^{14}–10^{15} years, 10,000 to 100,000 times longer than the age of the solar system or the time scale of nuclear reactions. This refers to most slowly or moderately rotating stars, especially to giants, and the assumption that the helium produced from hydrogen in the hot central regions would somehow get mixed into the entire volume of the star is untenable, and by a large margin. It should be emphasized that in this respect I did not add a single bit to Eddington's admirable creation; I only performed the calculation according to his prescribed formulae. Eddington's failure to pursue the consequences of his own theory can only be explained in terms of a "blind spot," a dogmatic refusal to abandon his model of convenience—that of uniform chemical composition. My next step was the numerical integration of "composite" nonuniform models, which was much more complicated than the application of Eddington-Emden's homology formulae. However, it was realistic and brought the reward of explaining the structure of giant stars, with high central temperatures and densities that are adequate for advanced nuclear reactions, but with large radii and low mean densities (3).

MIXING LENGTH

A model of convection, initiated by Schmidt and Prandtl, pictures the vertical transport of excess heat in a gaseous or liquid medium through the symbol of a "mixing length," L_m, such that a hotter element ("a bubble"), while rising over this length, does not exchange heat with the colder surroundings and delivers all its excess at the top. It is matched by a similar cold bubble descending from the top and absorbing its prescribed share of excess heat only when arriving at the bottom. Such a lateral isolation of the moving "bubbles" or streams could be achieved only by a miracle, and I devised a realistic model of convective transport (4, 5), taking into account lateral exchange, which agrees with laboratory experiments within $\pm 20\%$, while the mixing-length model predicts a transport by $+3000\%$ in excess of the true value. In a model of cellular convection, the rising current is, of course, always warmer than the descending current *at the same potential level*, but, because of lateral exchange, this is only one tenth of the total adiabatic temperature excess between the extreme levels; the rising current is gradually precooled through lateral contact with its gradually preheated descending counterpart. The stream velocity, proportional to the square root of the equipotential temperature difference, is reduced to one third, and the real convective transport is thus reduced in a ratio of $\frac{1}{10} \times \frac{1}{3} = \frac{1}{30}$th of the mixing-length prediction, while dimensionally the transport equation remains unaltered.

The mixing-length symbolism, probably meant only as a simile, was grasped by a school of astrophysicists in its literal sense and used for numerical applications. In the deep stellar interiors, with their high temperatures and densities, the super-efficient mixing-length model would require deviations from the adiabatic temperature gradient of about one part in three million, while the realistic model would require about one part in one hundred thousand—both small enough not to be reckoned with in the calculations of stellar models. However, in the outer layers near the stellar surface the difference is enormous. In this context, the dogma of the mixing-length has become an obstacle to progress, as can be seen from the following incident.

In 1970, D. J. Mullan—a pupil of mine who has now risen to prominence with numerous researches, especially in the physics of stellar atmospheres, offered for publication in *Astronomy and Astrophysics* (the European Journal) a paper on "Cellular Convection in Stellar Envelopes." By applying my theory, he explained a score of various spectral traits, created by the bottleneck of inefficient convection in stellar atmospheres, which could not be accounted for by the mixing-length doctrine, with its over-efficient transport. On the report of a referee who disagreed with my theory, the paper was rejected. The fact of rejection on such grounds, even if disputable, is in itself very ominous. Although the story had a happy end—the paper was then published without much delay by the Royal Astronomical Society (6) and represents undoubtedly a gem of a contribution to the knowledge of stellar atmospheres—the attitude of the referee (a staunch believer in the mixing-length, yet officially anonymous) was characteristically dogmatic. By misrepresenting—apparently from unwittingly misreading the texts but perhaps true to his creed—the unwanted alternative to the mixing length, it is a remarkable example of wishful thinking. Here are a few citations from the referee's report, which was communicated to the author. He (or she) writes (exact excerpts are in quotation marks, with my comments following):

1. In Öpik's investigation, "The largest part of the convective upward heat transport is assumed to be transported down again since the matter presumably

cannot get rid of its surplus energy." Quite contrarily, I show that the contact heat transport is so powerful that the excess heat escapes laterally into the downward current before reaching its ultimate destination and that matter gets rid of its surplus energy much too readily and sooner than in the mixing-length analogue. Further, convective transport is not to be equated to the total heat content of matter moving upwards, but is only the net difference delivered at the top, and this, once delivered, cannot "be transported down again," a physically meaningless suggestion and something I nowhere had intimated.

2. The referee then continues, "This then leads to the conclusion that a hot gas stream ($\Delta T > 0$, $\Delta \rho < 0$) will sink in the atmosphere, which seems impossible to the referee." The naive term "hot" betrays the root of his misunderstanding: there is nothing like absolutely "hot" or "cold," nor is there any absolute definition of the temperature excess, ΔT. What matters is the difference between the warmer rising current and the colder descending current at the same potential level, and for the "sinking" stream ΔT is always less than 0 or colder, although the difference is decreased ten times compared to the mixing-length figment, while, compared to the bottom level, both rising and descending currents are colder because of adiabatic expansion.

This would suffice, though there is more to it. The quotations as cited above are typically similar to the dogmatic criticism of misrepresented tenets of another faith, often as wholehearted as it is prejudiced; unwillingness to get acquainted with the actual pronouncements of the other side is common to both. Such an attitude, though alien to science, is nevertheless there as the consequence of human weakness, and we have to reckon with it as a fact.

As an outstanding example of dogmatism in science, the mixing-length syndrome still persists and comprises an influential mini-establishment, though it is harmless as long as it is confined to stellar interiors and keeps clear of the surface layers, or of planetary atmospheres.

CRATERING

Impact cratering is important in the process of shaping the surfaces of planets, especially of the Moon and Mars, as well as in cosmogonic processes of building the planets from aggregates of smaller stray bodies. Until lately experiments at cosmic velocities were not available, and I developed a theory based on first principles which now, when compared with the experimental data, turns out to allow the calculation of crater volume, diameter, depth, and ellipticity of craters with an unexpected accuracy of better than 20% in linear measure and at all velocities (7). No empirical parameters are used. The crater volume is essentially proportional to the *translational momentum* of the projectile, with a *corrigendum* for vapor formation and a secondary shock caused by it, while the cohesive strength of the target enters as an independent variable determining the coefficients of proportionality. The success of this a priori theory, now empirically verified over a range of velocities from 2 to 20,000 m sec^{-1} without using ad hoc coefficients of proportionality, is its main justification. Consideration of the *radial momentum*, created in the target by the intruding projectile, is the main feature of the theory.

Regrettably, experimenters in hypervelocity impact have used and are still using kinetic energy as the argument for interpolation, even without proper regard for the cohesive strength of the target. While this procedure may be practically satisfactory for representing experiments with the same materials within a limited range of velocity and a more or less constant mass of the projectile, extrapolation by kinetic energy beyond the experimental range may lead to errors of many orders of magnitude: mv^2 can never be made to correspond uniquely to mv. Actually, because of vapor formation and second shock, the velocity exponent for crater volume may be about $\frac{4}{3}$, while at constant velocity the crater volume must be proportional to the mass of the projectile. An intermediate formula for cosmic velocities, something like $mv^{4/3}$, could be suggested, instead of mv. My theory actually allows for this, although without the mathematical oversimplification. The empiricists, however, having discovered the lesser power of velocity but insisting on kinetic energy as the argument, would put the crater volume proportional to $(mv^2)^{2/3} = m^{2/3} v^{4/3}$, with the unnatural $\frac{2}{3}$ power for mass (instead of 1). There are more details to it, to be looked for in the relevant publications.

At present, however, a mini-establishment exists around the doctrine of kinetic energy as the impact argument. This is an impediment to progress and, until the successful correct theory (especially with regard to cohesive strength) is applied, extrapolations and speculations on the cosmogonic role of cratering are subject to major pitfalls.

Estimates of the mass of projectiles that produced meteor craters on the Earth and Moon offer a relevant example. For the Arizona crater, estimates based on the doctrine of kinetic energy were about 40 times lower than the mass corresponding to the criterion of translational momentum, and independently confirmed by the depth of penetration. If such were the efficiency of meteorite impacts (which actually waste most of their energy on the inelastic radial shock and heating of the target material), the number of craters in lunar maria would be by almost two orders of magnitude higher than observed, amounting to saturation cratering and equal to that on the continentes. It has been shown (8, 7) that, with the observed population of stray bodies in the solar system and my theory of cratering, the frequency of small and medium-sized craters in the maria is closely accounted for by impacts during the past 4,500 million years, while larger craters show an excess, accountable by survival of premare craters through the event of mare lava flooding (itself a result of a major impact). This in itself is a most impressive confirmation of the cratering theory, obtained well before experiments with hypervelocity (and non-hypervelocity) impacts were made on Earth.

By arguing ad absurdum, we could say that, if energy were directly relevant to the size of a crater, a bonfire lit on a rock surface should lead to "progressive cratering" because heat is also kinetic energy. Cratering is the result of action of forces, and action is in direct relationship to momentum. In a kind of transfiguration of momentum, the translational momentum of the projectile creates radial momentum of the displaced target (rock) material in a constant ratio of from two to five (depending on velocity as determining the secondary shock from vaporization); the total momentum of the symmetric radial shock is, of course, zero, while the translational momentum of the projectile is absorbed by the main body (planet).

In the target, the inelastic shock conserves its momentum separately in each radial direction as long as destruction of the solid material and hydrodynamic flow takes place. The velocity, U, of the radial motion thus decreases as the volume and mass involved increase, until the hydrodynamic resistance ρU^2 (ρ = density) becomes equal to the crushing strength of the material, s. This determines the limit of destruction and the volume of the crater.

Agreement with experiment and with observation (frequency of lunar craters) completely supports this theory (which, of course, is more complicated than could be sketched here). It can only be wished that the dogmatic eclipse of the realistic cratering theory by the kinetic energy scaling would be lifted and that the overlooked and neglected *perfect* theory be raised to its proper place, for realistic dealings with cosmic or cosmogonic events. (Years ago it was found that the theory correctly predicts the armour-blasting properties of artillery shells, a "practical" confirmation of, unfortunately, too sombre associations).

LUNAR AND MARTIAN VOLCANOES

Despite all the eloquent statistical arguments of interplanetary astronomy, the thesis that lunar craters are presumably of volcanic origin was quite widespread, chiefly among amateur astronomers and professional geologists. Decades ago some of them even tried to deny a meteoritic origin of the Arizona crater. The dogma was very strong and was proclaimed by a considerable group or establishment of true believers. At present it has been completely destroyed, at least as concerns the Moon, by direct space exploration and landings, and need not interfere with scientific progress any further. Of course, Mars has now become the refuge of planetary volcanoes, although no longer in a dogmatic sense. It is conceded that most of the Martian craters also originated from impact, because their frequency (surface density) corresponds to statistical expectations for the fringe of the asteroidal belt. But a few large structures are still called "calderas," perhaps wishfully implying their volcanic origin. From the total evidence available, I still prefer to consider them impact craters, surviving an immense lava flooding in the Martian northern hemisphere from an impact of a large asteroid "in the beginning" (9). Since they are similar to the larger surviving lunar craters, and since they are placed on elevated ground well above the average level, such survival in the midst of a lava sea is quite plausible. The matter, however, cannot yet be considered as finally settled. Besides, from the slowness of erosion on Mars—which on Earth is a necessary link in mountain building—Mars cannot yet have entered the phase of volcanism that may be billions of years ahead.

ANCIENT MARTIAN "RIVERS"

The identification of some gigantic meandering cracks as the beds of ancient rivers (of water or lava) on Mars is in danger of becoming a mini-dogma, misleading and perhaps impeding progress. The only reason for such an identification is their meandering shape and formation of systems of succursals closely reminiscent of terrestrial river systems. I have pointed out that exactly similar meandering and branching systems of cracks are omnipresent on asphalt or concrete sidewalks, e.g. on all university campuses I have visited (10, 9). These are caused by the pressure of encroaching vehicles without any relation to fluid flow, and the cracks or clefts on the Martian surface are most probably of similar origin, caused by the pressure of readjustments of the deeper crust on which the top layer rests. Water rivers are definitely excluded—with large amounts of water, cloud and snow formation (at present solar luminosity, though it was *lower* in the past) would have depressed the mean global temperature on Mars from the present low temperature of −42°C to −62°C, equal to the coldest Siberian midwinter. Water would everywhere be completely frozen under such circumstances, unable to flow and create rivers. Although lava flow, dubious as it is, cannot be excluded by such an argument, cracks on the surface of a thermally evolving planet must inevitably arise, and before looking for the "rivers," let us look for the few real cracks: they are there, relegating the "rivers" to the realm of fantasy until better confirmation is available. The lunar "rilles," which also were regarded as traces of liquid flow, are now more definitely identified as cracks or rifts (10), and this may serve as an analogy for Mars.

ORIGIN OF METEORITES

The physical and mineralogical structure of meteorites implies that they are collision fragments of asteroidal or sublunar sized bodies. Consideration of encounter probabilities ensures that collisions between members of the asteroid belt do happen; hence there is a possibility that meteorites actually arrive from the asteroid belt, their orbits being changed by the velocity imparted at collision and by subsequent planetary perturbations. The newly determined density of the largest asteroid, Ceres, indeed confirms the hypothesis that asteroids are compact stony bodies and not fluffy objects like cometary nuclei. This falls now in line with the hypothesis of the asteroidal origin of meteorites, which is now seemingly becoming a mini-dogma, accepted without further doubts. Yet it has been shown (*11*) that contemporary collisions and perturbations cannot account either for the yield or for the orbits of meteorites, which resemble those of short-period comets brought inside Jupiter's orbit by nongravitational forces. The orbits of the so-called Apollo class of "asteroids" belong to the same type, which suggests that they are extinct remnants of disintegrated gigantic cometary nuclei. The apparent conclusion, to be substituted for the fruitless dogma, would relegate their origin to collisions among quite another class of primeval asteroids and to the dawn of the solar system. The original fragments would then have become incorporated into the ices of accreting comet nuclei, and been ejected by planetary (Jupiter) perturbations to the outskirts of the solar system (Oort's sphere of comets), where they would be stabilized by stellar perturbations and sent back to the inner regions of the solar system by similar perturbations. After being captured by Jupiter into short-period orbits, the "rocket-effect" of evaporating gases would cause some of the orbits to shrink (namely those with retrograde rotation of the nuclei) and thus to escape Jupiter's dangerous vicinity. With evaporation of the volatiles, the meteoritic fragments or the Apollo "asteroids" are released into our interplanetary surroundings, to be ultimately removed by planetary encounters within a lifetime of the order of 100 million years. This model is also in harmony with the cosmic-ray ages of meteorites, which represent the time since they were released from shielding inside the cometary nuclei, and not the time of their collisional break-up. If this were so, their relevance to the origins of the solar system would be greatly enhanced.

TIDAL ORIGIN OF THE SOLAR SYSTEM

The hypothesis by Chamberlain and Moulton, so diligently pursued by Jeans, that a close tidal encounter of the Sun with another star led to the formation of the solar system, has enjoyed widespread (though not universal) acceptance and is still on the books, despite its improbability bordering on impossibility. It offers an example of dogmatic attitude with formation of its peculiar establishment group, which is especially strong in popular writings. Such an encounter, of course, is quite possible but extremely improbable. Further, as pointed out by Russell, the hot gases ejected from the Sun could not condense into planets, especially not during the short time of the stellar passage, but would instead disperse into space. Their angular momentum (which raises the cardinal challenge to all cosmogonic theories) at ejection would be short of the requested value by a factor of the order of 20. This difficulty, and then putting the planets into circular, regularly spaced orbits, created formidable problems that Jeans attempted to answer through appropriate perturbations by the passing star, a gigantic mathematical task never convincingly concluded. The problem is in itself interesting and it was worthwhile to treat it, but physically the outcome of the encounter would have been the ejection of uncondensed gaseous matter, which would at first have formed a nebula. Any wisps of gas, ejected into various intersecting or interpenetrating orbits, would then through collision settle into a circularly rotating aggregate with conservation of the sum of angular momentum, i.e. a primeval nebula from which later the planets could have condensed. Yet this returns us to the nebular hypothesis, and there is no way whereby we can distinguish between a nebula directly condensed from interstellar space, and one formed in the tidal encounter—except for the criterion of probability. And in this respect the solar system gives an answer. The systems of the satellites of the outer planets show the same kind of regularity exemplified by the mother system itself: a coplanar succession of near-circular orbits with a more or less regular spacing (Titius-Bode Law). Instead of the extremely low probability of stellar encounters, the formation of solar or planetary satellite systems appears to be the rule rather than an exception. The nebular hypothesis is thus able to account for everything. While the improbability of a tidal encounter can be partly brushed away by assuming that it happened when the stars were much closer together (this, however, would require an improbably high age for the solar system), the ensuing regular spacing of the planets (or the satellites) requires the intervention of another improbable configuration, so that the idea of a tidal encounter can hardly be maintained in this context. Of course, there could have been tidal encounters during the early phases of evolution of our stellar universe, and the theoretical work done in this respect is not quite in vain. Yet the outcome of such encounters could be very different from the formation of something resembling the solar system.

PUBLICATION BIAS AND EXCHANGE

When, in 1938, my papers (3) on "Stellar Structure and Stellar Evolution" (with calculations of unmixed stellar models and those of giant stars) were printed in the *Publications of the Tartu (Dorpat) Observatory* in Estonia, I soon afterwards received a letter from George Gamow, underlining the importance of my work but reproaching me for publishing in such an "obscure" place, wherefore—in his opinion—progress in the study of stellar structure must have been unnecessarily delayed.

The view that, by all means, publication must be achieved in the internationally recognized "important" journals pervaded and still pervades the astronomical establishment and especially the young generation; the latter of course for obvious practical reasons. Yet the fact that Gamow—within a year—got hold of my papers, and that others soon continued on these lines (sometimes referring, sometimes not, to my work), is the best answer. Tartu Observatory, in the centuries-old astronomical tradition of exchange of publications (possibly explained by the fact that we are all dealing with the same cosmic laboratory called the Universe) exchanged its publications with all astronomical institutes of the world, so that the work did not remain unknown to those who cared (physics and most other branches of science do not adhere to such a tradition). And, as to the economical side, the publication costs in Estonia were very low (as they are also at Armagh where the same tradition of exchange is continued). Although, for instance, the editorial setup of the *Astrophysical Journal* was friendly toward my aspirations, the page charges and reprint costs for some 200 pages of mathematical and tabular material would have been absolutely prohibitive. Also, it was certain that full-length publication of such extended papers would not be possible. I had already had previous experience with editors and reviewers requiring great reductions in size, to the detriment of detail which is so essential in pioneering work.

I wish here to emphasize further the importance of the traditional exchange of publications. Not all observatories (especially the smaller ones) are in a position to subscribe to all the "important" journals. Also, a search in libraries for the relevant articles a scientist may need would involve unnecessary psychological effort and waste of time. Thus, because of human weakness, communication between scientists would considerably suffer unless, as in the astronomical tradition, reprints and independent publications of an observatory are systematically numbered and kept in one place. In such a case it is easy and even rather tempting to look among the systematized publications of another astronomical institution for the collected printed papers of a colleague who is known to work on a definite subject. Theoretically, the convenience may appear to be irrelevant, but practically it is of utmost value.

The institutions that do not follow the tradition of exchange usually send out Lists of Reprints, available on demand. This procedure disregards the fact that any research of value is not meant to satisfy the interests of individuals of today, but should address itself also to the future. It cannot be known in advance which work will be of relevance within decades (or even centuries) to come. A selection made to satisfy the temporary interests of today may and certainly will miss the works of relevance for tomorrow. A library always contains more works than would be ever needed or read: but it is impossible to foretell the interests of the future, and the collection must necessarily be very much more complete than the actual needs that may arise.

In the matter of exchange, the giving hand should take the initiative. As his moral and vocational obligation, a creative spirit must make his results known, at least where similar work is, or could be done. It is like seeding. Few of the seeds may fall on fertile soil, yet nobody can predict for certain which of them will grow. And, where there is no seeding, there will be no growth.

THE CRITICS: EDITORIAL REFEREEING AND CENSORSHIP

Another reason for not always publishing in the accepted "important" journals is a danger of being rejected, either because of a sincere failure of the editorial apparatus to appreciate new developments (often because of the fear of appearing ridiculous), or because of dogmatic and personal prejudices. These last two should not enter into the editorial judgment, which should be as impartial as possible, yet actually they sometimes do.

As an example of editorial changes that do not infringe on impartiality, I would cite a case with *The Irish Astronomical Journal,* where I am Editor, and where, in an article on the "Lunar Surface" offered by Patrick Moore (12), he voiced his support for the volcanic theory of lunar craters. Although completely disagreeing, it was not my business to interfere except for one minor change: the two groups with opposing views were called "authorities" by Patrick Moore—one, favoring the volcanic origin, consisting chiefly of amateur astronomers, the other, siding with the impact origin, consisting of professional scientists (one of them a Nobel laureate); I had the word "authorities" changed to "authors," with the author's consent.

In my experience, however, editors have not always been so impartial, although usually

my microphotometric measurements of Mount Wilson photographs during the Opposition of 1958 was accepted by *Icarus* for publication, on the condition that I omit two concluding pages and a figure, actually containing my chief results. The photometrically measured diameters showed a consistent variation with areographic latitude, closely similar in the two colors—the blue and the yellow—and were interpreted as revealing climatic zones of atmospheric circulation similar to those on Earth. I maintained namely that what we measure as the limb is the top of a dust layer that is mainly responsible for the reflectivity of the atmosphere, and not its gas (this is now confirmed by the Viking 1976 landings on Mars). An upward current would lift the dust up, a downward current would carry it down. The measurements showed an equatorial uplift, a subtropical depression as for the anticyclone trade-winds, again an increased diameter in the zone of middle latitudes corresponding to the terrestrial temperate zone of westerlies, and again a subpolar—apparently anticyclonic—depression; only the polar diameters (all relative to an equipotential surface) were increased, contrary to the expectation of an anticyclonic depression, but this clearly seemed to be caused by snow or ice crystals of high albedo replacing the yellow dusty haze of the other latitudes. Now, on this *observational* evidence (whatever its interpretation, right or wrong), the Editor of *Icarus* and my professed friend had put his veto! Another journal was then ready to accept publication, but on the evidence of previous rejection the Editor changed his mind. The article was ultimately published in The Irish Astronomical Journal (13).

From my long experience, both with my own papers, and with those sent to me for refereeing, I have a feeling that the "recognized" journals usually accept without difficulty papers with a middle-of-the-road content, useful contributions to research which already has established itself or accumulations of additional new material. Papers that make little sense are mostly rejected, but some of them are slipping through. As to pioneering work, papers of this kind often are running the risk of rejection or of excessive curtailment.

The practice of anonymous referees is much to blame for editorial malfunctioning. A referee for a scientific journal is a scientist, morally committed to seek and openly proclaim the truth as he feels it, and he should never hide behind the screen of anonymity. As referee, I always send the author an exact carbon copy of my letter to the Editor with all my comments. Among about 150 of such reviews, I have received many letters of thanks (for my suggestions) and only one of what practically amounted to abuse; in most cases, however, there was no reaction. There were a few cases when I was asked to be arbiter in an unfavorable referee report, and I succeeded in rehabilitating some authors from unfair criticism by anonymous colleagues who appeared to think that they alone were entitled to write about a certain subject. Anonymity in refereeing is like kicking somebody in the dark, without a chance of response; it "protects" the reviewer but not the author. The sooner this scourge of anonymity is abandoned, the better for the honest pursuit of research. If fewer referees can be found when there is no anonymity, it will be only to an advantage: those who consent will be a more qualified selection for the job of critics.

We may ask here how many geniuses have been crushed by the unsympathetic and prejudiced attitude of editors and critics in the sciences as well as in the arts, and remained unknown forever? The late discovery of forgotten geniuses testifies to the existence of a graveyard of misunderstood or mishandled originalities which did not fit into the "establishment" of the critics. The sad record of George Bizet, who died in desperation witnessing the failure of his "Carmen" in the eyes of the critics and the Paris public influenced by them, serves as a reminder: "Carmen" has become unquestionably the most popular opera of all times. By independently printing in the "obscure" Tartu or Armagh publications, my work has ultimately made itself known. Would I have been forced to limit myself to the "recognized" big journals, much of it—possibly some most original contributions—could have remained buried forever.

THE BIRTH OF A MYTH

A remarkable article by Leighton (14) supplied with artistically rendered humorous illustrations almost true to life, emphasizes the fact that scientists seldom listen to others and, if they are not dozing during lectures, they may either be preoccupied with their own thoughts or enter into private discussions. Although partly explained by acoustical difficulties, this attitude may not be limited to conference lectures.

The following example describes a case to the point. The printed word is an indubitable fact, irrespective of whether or not its statements are subject to debate. Yet here we have a critic who first completely misrepresents a published work, and then—rightly—sets out to destroy this figment of his own imagination which, incidentally, is just the opposite of what the author of the criticized work was saying.

In the universally recognized international journal *Science*, R. K. Ulrich (15) refers unfavorably to my work (16) on stellar structure and variations of solar luminosity. He purports to describe my model and calls it "physically untenable," but the described model is not mine—only the critic's invention and, so to speak, the very opposite of what I had proposed. In my reply (17) I point out that, while I consider inward diffusion of *hydrogen* into the core depleted by hydrogen burning, he objects to inward diffusion of the *heavy elements* which in my model are not diffusing at all. In my model, turbulent mixing suddenly transports more hydrogen to the core, triggering thus an increase in the nuclear energy output, this being the most important point in my theory of variability. Yet Ulrich never mentions "hydrogen" by name or "nuclear energy generation" in this context. While I trace the heavy-element content in the Sun to interstellar *diffuse* matter (dust) during the process of star formation by accretion, thus predating all the development stages of the future Sun, the critic insists that I am putting them into the core by internal diffusion inside the Sun! etc. etc. Possibly, the words "diffuse" matter and negligent reading, with a mind concerned with gas diffusion, may have led him to this gross misrepresentation.

Now, as often happens, other authors could rely on the second-hand information of such a source, and a ready-made legend or myth, perhaps a new dogma could emerge, something of the sort "Öpik wants diffusion of the heavy elements in the Sun to be responsible for its variability, which of course is too slow on the time scale of stellar evolution." Note that a similar, perhaps not so extreme misstatement about "hot bubbles going down" has been mentioned above in connection with the ill-conceived notion of the "mixing length."

It is not a question of whether I am right or wrong in my theory, but only of what I had actually said in print, thus of the complete distortion of an undisputable fact. In this case—as probably in many others—the editorial reviewer system has goofed, while the critical author, instead of a straightforward apology and admission of fault, in a "reply" (18) just vaguely expresses some of his own views on solar models and restricts himself to considering the (irrelevant and practically nonexistent) diffusion of the heavy elements "relevant to hydrogen," instead of considering the diffusion of hydrogen itself into the depleted core.

FOR A POSTSCRIPT

These nonsystematic recollections, based on personal experience, are concerned chiefly but not exclusively with preconceived notions and dogmatism. The research topics mentioned above as examples are of necessity those close to the author's scientific activity; he considers the points raised in their connection of great importance in the study of the Universe, but by no means implies that he is always right. He sincerely wishes that his words may not completely remain a lonely cry in the wilderness, but may perhaps at some time help someone in the impartial search for truth.

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