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RESEARCH ARTICLE

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Jürgen Ehlers—and the fate of the black-hole spacetimes

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Abstract This article—written in honour of Jürgen Ehlers—consists of two different, though interlocking parts: Sect. 1 describes my 54 years of perpetual experiences and exchanges with him, both science and episodes, whilst Sect. 2 describes the history of astrophysical black holes, which evolved during the same epoque though largely independently, with its activity centers in other places of the globe, and has by no means terminated.

Keywords Jü, rgen EhlersBlack holes, Sgr A*

1 Jürgen Ehlers: our 54 years of close interaction

Jürgen Ehlers and I both studied physics and mathematics at Hamburg University. My studies began with the summer 'Semester' of 1950, Jürgen's 1 year earlier; (he was born on 29 November 1929, $\gtrsim$ 18 months before me). When we scientifically matured to terminate our standard education—in the fall of 1953, I think—we were both attracted by the personality of Pascual Jordan, and jointly started weekly seminar work with him. Soon we were joined by Engelbert Schücking, and soon again, Pascual Jordan guided our collaboration, both mentally, and financially. The Hamburg group in General Relativity had been founded.

Jürgen, Engelbert, myself, and 'der Meister' took turns as weekly seminar speakers. Initially, Jordan's contributions aimed at improving the second edition of his book 'Schwerkraft und Weltall'. Jürgen's contributions educated us primarily on other people's work: they helped us understand it thoroughly. And Engelbert excited us with brand new stuff which had somehow entered his mind; unless he did not show up at all; whilst absorbing this stuff, possibly in a bar near the train station. My own contributions were often somewhat premature, introducing new problems to the group in order to receive feedback, and to profit particularly from

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Jürgen's suggestions for deepened understanding. On 4 June 1955, Jürgen met (his later wife) Anita, on a large faculty dance in the (decorated) mensa of the university which I had organised.

Our scientific work was done largely independently, mainly at home, except when an 'airforce report' had to be written, for Jordan's contract with the U.S. army, or when during the early 1960s, survey papers had to be composed for the 'Akademie der Wissenschaften und der Literatur in Mainz', of which Jordan was a member. Such joint publications helped us gain a certain group knowledge. But there was enough time left for continued learning, and for approaching new problems. We tried to get deeper insights into the different known classes of exact solutions of Einstein's field equations, their invariant properties, and global continuations. Besides, Jürgen improved his—and our—understanding of relativistic thermodynamics, electromagnetism within GR, and relativistic statistical mechanics. And we searched for reliable ways to decide when two given line elements could be transformed into each other, by a suitable change of coordinates.

This program began with a discussion of Kurt Goedel's (1949) homogeneous and stationary cosmological model (with a non-vanishing cosmological constant Λ), in which suitably accelerated observers can travel into their own past. In 1957, our work received a forceful boost by Felix Pirani's physical evaluation of Petrov's 1954 algebraic classification of the Weyl tensor for normal-hyperbolic metrical spaces, i.e. of the traceless Riemann tensor, as for vacuum spacetimes. Petrov's classification turned out to be a valuable tool for analytic evaluations, in particular of (strong) gravitational-wave fields, but also for other classes of 'algebraically degenerate' spacetimes in which the eigen values of the Weyl tensor tend to yield invariant coordinates. Other tools for characterisation came from Elie Cartan's (1946) basic work on Riemannian spacetimes, in particular a solution of the (local) 'equivalence problem'—later also called 'isometry problem'—of two given spacetimes, and from results on algebraic invariants by Weitzenböck (1923), Eisenhart (1927, 1933, 1935, 1949), Thomas (1934), Weyl (1939), Chevalley (1946), and Ruse (1946), as well as by Blaschke (1950), Taub (1951), Schouten (1954), Boerner (1955), Komar (1955), Lichnerowicz (1955), Yano (1955), and Trautman (1958). We thus arrived in addition at the 'theorem of complementarity' (of scalar differential invariants and group trajectories) which states that a possible r-dim group of isometries of a given n-dim spacetime V_n—with q-dim trajectories, $q := r - s$—contains complementary information about its geometry in the sense that one can find $n - q$ independent scalar invariants which complete a system of q symmetry coordinates to a full (local) n-dim coordinate system of V_n; where $s = r - q$ denotes the dimension of the isotropy group of a generic point. Homogeneous subspaces are invariantly embedded.

These two theorems—on constructing (or not) an isometry between two given V_n with or without homogeneous subspaces—were contained in my 1958 Ph.D. thesis (including proof sketches), and can be found again in my 1960 [11] and 1962 [5] joint publications with Jürgen listed in the references, though only partially highlighted as theorems. Jürgen had done the proof-reading at Hamburg whilst I was in Syracuse (NY)—working in Peter Bergmann's group for a bit over a year—at an epoch with only snail-mail correspondence. Jürgen knew that I had discovered an oversight (in my first writeup, and in other people's proofs) which applies only in the rare case of spaces with an indefinite metric, and of a special type of their Riemann tensor, for which the complete system of rational scalar invariants (formed from their metric and Riemann tensor and its covariant derivatives) gets functionally dependent, so that one has to take recourse to new methods of constructing scalar invariants, involving differential calculus. One such nasty special case turned up in the subclass of the pp waves (see below), with higher (than the usual 2-dim) symmetry, and served as a counter example to published results even though it was fairly clear how to repair them. Should we have formulated the theorems as conjectures, or should we have relegated them to the text? Jürgen's honest mind preferred the latter.

The years 1959–1962 were brainstormy ones for us. The Petrov–Pirani insight had likewise reached 'relativists' like Ivor Robinson, who had shown me how to integrate the gravitational field equations analytically, in suitable coordinate gauges, and Andrzej Trautman, Hermann Bondi, Ray Sachs, Roger Penrose, and Ted Newman, among others, who showed us how to analyse the far-field behaviour of radiating sources. For me in particular waited the class of plane-fronted gravitational waves, plus its subclass with parallel rays—called 'pp waves' for short—which turned out to be extremely instructive: the plane-fronted gravitational waves show strong resemblances to their electromagnetic analogs in flat spacetime, but are richer as a class. They are among Einstein's vacuum fields with the largest groups of isometries, and with the most extreme behaviour of their scalar invariants, as already mentioned above. Their action on clouds of test particles could be elucidated, and they could be shown to be geodetically complete. Clearly, during the years of their analysis, Jürgen's interest and advice meant a great challenge to me.

But like every human activity, Hamburg's relativity group did not enjoy eternal life: the year 1962 saw me build my house in Hamburg-Hoheneichen, and move into it in November that same year. Jürgen and Anita spent several years abroad, between 1961 and 1971, in Syracuse, Dallas, and Austin (Texas), and Ulrike and I spent our honeymoon year 1966/7 in Pittsburgh (PA), as guests of Ted Newman and his family. Pascual Jordan's sixty-fifth anniversary was celebrated (in Hamburg) in October 1967. In all these years, our relativity group was kept alive by new (younger) members, like Manfred Trümper, Bernd Schmidt, Wolf Beiglböck, Klaus Bichteler, Volker Enß, Michael Streubel, Eva Ruhnau, Hans-Jürgen Seifert, Henning Mueller zum Hagen, Hans Seiler, and a few more. We met at international conferences, like the Texas Symposia on Relativistic Astrophysics (introduced by Ivor Robinson, Alfred Schild, and Engelbert Schücking), the Varenna Schools (on lake Como), the Royal Society Meetings in London, and the Marcel Grossmann Meetings (organised by Remo Ruffini, beginning at Trieste). There were frequent, lively international cooperations.

There was another intense collaboration during the early 60s, of Jürgen and myself: we partcipated actively in a weekly seminar on the selected book on QED by Jauch and Rohrlich, jointly with Hans-Jürgen Borchers, Hans Joos, Werner Theis, Klaus Helmers, Peter Stichel, Georg Süßmann, and Bert Schroer, because we wanted to learn how to quantise the gravitational field. (Today I think it must not be quantised (2007 [21]), because there are no observable effects of quantised gravity: only the equations of state require quantised treatment). I still remember the scene—as though it happened yesterday—when Jürgen was the speaker of the day, and was cautioned by Helmers about an inconsistency of his statement (on the supposedly unitary correspondences between interacting and free fields): Jürgen understood immediately, was upset, and threw the library copy of J & R on the desk of our seminar room in disgust, because the formal elegance of the book's presentation had fooled him, had made him believe that the offered correspondence was mathematically exact. This has been the only case of its kind which I can remember, in our whole joint life.

Our scientific interests in General Relativity shifted gradually, from a deeper understanding of its local structure towards a study of its global properties: for instance, we could show how Newton's theory of gravity can be obtained from Einstein's (more) general theory as an exact limit, by a widening of its light cones (Trautman, 1964; my habilitation talk, 1965; Ehlers [4]; listening to Jürgen was always rewarding, even when he returned to long-standing problems). Increasing insights were likewise gained into the asymptotic behaviour of outgoing radiation, and also into topological peculiarities—like 'trouser worlds'—which are forbidden by the global regularity of the spacetime geometry. And then there was the class of metrics describing mass concentrations of extreme compactness, the Schwarzschild and Kerr-Newman fields and their analytic continuations (through their 'horizons') which were eventually termed 'black-hole' spacetimes by John Wheeler and Remo Ruffini, in 1971; they will be the theme of the second section. Such non-local problems were attacked originally by Roger Penrose, Brandon Carter, George Ellis, and Stephen Hawking in Cambridge (England), later also by Bob Geroch (Princeton), Werner Israel (Edmonton, Alberta), Kip Thorne (Caltech), Petr Hajicek (Bern), and by a small number of others, including Seifert and Mueller zum Hagen from our Hamburg group; (cf. my 1971 review at Halifax, published in 1972 [14]; also [6; 9]). Exploiting Einstein's theory kept us engaged.

Yet another branch of physics caught my attention when I returned from Pittsburgh, in 1967, whilst Jürgen started his professorship at Austin: Four young men wanted to start their diploma work with me, plus a fifth his Ph.D. work. We joined forces with my close friends Klaus Hasselmann and Gerd Wibberenz, in an intense, wide-ranged seminar on statistical mechanics which lasted successfully until 1971. Jordan's relativity seminar survived in parallel, until his formal retirement in 1970, and even a few years beyond. In addition, I was elected as the principal investigator of experiment 11 on the German–American spaceprobe Helios, to test the validity of Einstein's theory beyond the 1% level—a task that kept my collaborators and me actively engaged from 1969 until 1979, passively even until 1983—even though our experiment had to be sacrificed to the ten active ones on board when in both missions, one of the two amplifier tubes burned out when data transmission was interrupted by range measurements.

All these activities allowed me to extend my insight gradually throughout most of physics, at CERN (1972), Bielefeld (1973–1974.3), and Bonn (1978–now), with several informative visits abroad in between. I moved from general relativity through quantum field theory and statistical mechanics to cosmology—after the detection of the 2.73 K background radiation, and after Rolf Hagedorn's proposal of a highest temperature, near the pion rest energy, which would have a distinct impact on the first nuclear reactions in the Universe—and subsequently into astrophysics, planetary physics, geophysics, and even biophysics, all of which fields have left traces in my 2005 [20] Springer book *Astrophysics, A new Approach*, and in my 2008b [23] article contributed to the book *Against the Tide*.

They brought me increasingly into conflict with general wisdom, through the attempt to keep physics as clean as possible from inconsistencies, a demand that ought to be self-understood, but unfortunately is not. Such conflicts are collected in the form of 79 'alternatives' in my 1998 [4] birthday book 'Understanding Physics'—the same book, and symposium, to which Jürgen contributed his 'Newtonian limit' version (1998 [4])—and had grown to 100 alternatives in the 2005 [20] edition of the Springer book; their number has meanwhile reached 124. The alternatives emerged through the desire of thorough understanding, and whenever I had a chance to meet Jürgen, I discussed some of them with him, even if and when he did not call himself an expert in that particular area. To know Jürgen's reaction has always been helpful. His co-authored book on gravitational lensing (1992 [31]) ranks high in the world list of citations. Our last oral, long conversations took place in the summer of 2006, during the eleventh Marcel Grossmann meeting in Berlin, in between the main lectures, but mainly at leisure, on banks in a nearby park.

You care to know what my alternatives are all about? The two toughest among them, 15 and 27, claim that we have not detected a single black hole in the sky yet, no matter whether of stellar mass, or supermassive, and that we may never detect any. But this is the subject of the next section. Most alternatives reached me similarly to how they reached Fred Hoyle, according to his 1955 [10] book *Frontiers of Astronomy*: certain explanations lack convictive power and/or lack the harmony of the grand design, noticeable on the timescale of seconds to minutes. Sure enough, with the relevant documents at hand, it then does not take more than a couple of days until conjectures mature into solid reasonings. Tom Gold was my great example and teacher in pursuing them, during multiple encounters in multiple places and on various occasions. I owe him a lot of insight, as I owe to Jürgen. So let me end this sketchy history of 54 years of close interaction with Jürgen by adding three further, rather recent examples.

The first example is my alternative 101: the second law of thermodynamics, its continuum form: it cannot even be found under this name in Landau and Lifshitz, nor in Ehlers (1973 [3]). L&L VI §49 call it shily the 'heat transport equation'. The latter describes quantitatively the rate at which ordered kinetic energy is converted into disordered one, and interpolates the discrete entropy formulae of box thermodynamics, hence merits the name 'entropy theorem', and serves as a restriction to many thermodynamic processes which would otherwise be permitted, under the sole constraint of 4-momentum conservation [21]. It has many applications to various practical problems: How do the galaxies, stars, and planets generate their large-scale magnetic moments? How do plants raise the needed water to their crowns, against gravity? How do celestial objects accelerate ions and/or electrons to huge particle energies, $\lesssim 10^{20.5}$ eV? The literature contains answers to these three questions which violate the second law (I claim). I was glad that during a short e-mail correspondence with Jürgen, I could settle this fundamental point with him.

The second example is my alternative 2, which appeared in print as a (somewhat distorted) letter to Nature in 1976: What is the physical meaning of the expression called 'black-hole entropy' by Stephen Hawking 1974 [8]? I argued that his (gigantic) expression measures the entropy liberated during the hole's evaporation, lasting 10^{67} years for a solar mass, not during its formation, measuring in microseconds for the same mass (also: [25]). Jürgen had no objections to my letter when I told him about it, whereas my friendship with Stephen degraded.

The last example concerns a weird claim in the literature which reached me shortly before Jürgen's seventieth birthday (to whose celebration in Golm I was invited). Abhas Mitra, an Indian physicist of by and large respectable career, provoked the Western establishment via internet with the claim that the black-hole literature was faulty, that BH spacetimes could not exist. He bombarded me with calculations and quotations (via e-mail) which did not convince me, but I could not point at an obvious error. I asked a number of friends from the old days for help, with the same result. Finally, within less than a week after his birthday symposium, Jürgen caught A.M. on an oversight: he had confused partial derivatives with total ones. It needed Jürgen's care and patience to find the mistake.

And this was by far not my last communication with Jürgen: our last encounter was in July 2006, in Berlin; his last mail reached me in December 2007, containing the statement that Euclidean cosmologies—as proposed recently by a number of experts—had not convinced him.

2 A short history of black holes

This second section will be devoted to the role of black holes in present-day astrophysics, my alternatives 15 and 27, [20]. What are the black-hole spacetimes? Why were they considered relevant for astrophysics? How were the black holes hoped to be discovered? And why do I question all those hundreds of claimed detections?

As sketched in Sect. 1, exact solutions of Einstein's field equations have been searched for ever since their proposal in 1915, but the systematic finding of large classes with specified properties had to wait for the 50s and 60s. Among the first goals were solutions for isolated massive bodies, both without and with spin, i.e. static and stationary. The first (1-parametric) class was found almost immediately, by Karl Schwarzschild, in 1916. The second (2-parametric) class was first published in 1963, by Roy Kerr; it required his skill and perseverance. The fascinating story of its 8-yr-long discovery is described by him in his 2006 plenary talk in Berlin, printed in 2008 [12]. See also [9], and [2], for supporting descriptions.

The primary goal of the community was to find the exact outer vacuum solutions for compact, massive bodies. During years of hard work, it came as a surprise that the complete set of such spacetimes was only 3-parametric, interpreted as the mass M, angular momentum J, and charge Q of the field-generating body; John Wheeler coined it by "a black hole has no hair". Another surprise was that whereas the outer vacuum fields were stationary or even static, their analytic continuations towards their center changed into time-dependent geometries, across a lightlike (or null) bounding hypersurface termed 'horizon'. This structure of the complete class of black-hole spacetimes—so termed in 1971 by both Remo Ruffini and John A.Wheeler and by Yakov Zel'dovic and I. D. Novikov—was interpreted as the vacuum spacetime geometry left behind a collapsing body in both its outside, and inside world. Less symmetric, collapsing bodies would radiate away all their non-fitting higher multipole moments, it was thought—and calculated by [30]—before they cross their horizon, and approach a black-hole geometry at spatial and lightlike infinity.

Once the hairlessness of black holes is ultimately proven, their multipole moments are those of the (stationary) Kerr–Newman class of spacetimes, which are

$$M_n = Ma^n, \quad Q_n = Qa^n, \quad n \geq 0 \tag{1}$$

according to [29], in which $ac := J/M$ is the hole's specific angular momentum, M_n are the hole's {mass, spin} multipole moments for {even, odd} n, and Q_n are correspondingly the hole's {electric, magnetic} moments. As already stated, all initial deviations of a collapsing body's higher multipole moments from the above sequences are thought to be radiated to infinity, via gravitational and electromagnetic waves. Remarkably, a charged, spinning black hole has the same gyromagnetic ratio $Q_1/M_1 = Q/M$ as the electron.

The three parameters M, J, Q cannot all take arbitrary values: the area A of the (outer) event horizon of a rotating, charged black hole (BH) reads

$$A/4\pi = 2m^2\left[1+\sqrt{1-(a/m)^2-(q/m)^2}-(q/m)^2/2\right] \tag{2}$$

with: $m := GM/c^2, q := \sqrt{G}Q/c^2$; where m is called the Schwarzschild length of the BH, and the length q measures its electric charge, in natural units. This area A should be positive and real, forbidding superluminal rotation, and excessive charging. It cannot shrink during accretion, hence the inference that energy can be extracted from a spinning BH by braking its rotation—whereby its mass may even shrink—but only to the extent that this mass stays above its 'irreducible' value $M_{\text{irred}} := M\sqrt{1-(a/m)^2-(q/m)^2}$. It was speculated that such an extraction of energy could happen via accretion of matter, part of which is forcefully re-ejected from the ergosphere—with excess forward angular momentum—whilst a less energetic fraction falls in, and brakes the hole. In the case of two merging BHs, their horizon areas A_j should satisfy the inequality: $A_3 \geq A_1 + A_2$, and again it should be possible to extract energy, with a theoretical efficiency ε given by $\varepsilon = (m_1 + m_2 - m_3)/(m_1 + m_2) < 1/2$. Instead, realistic situations—if such exist—may have efficiencies which are lower by several orders of magnitude, depending on the state parameters (density, opacity) of the accreted material, and on its advected angular momentum [25].

The horizon area A serves to define the (famous) Schwarzschild radius R_S of a BH via

$$R_S := \sqrt{A/4\pi} = 2GM/c^2 = 3\,\text{Km}\,(M/M_\odot), \tag{3}$$

the latter two equalities for vanishing spin and charge; no self-gravitating object can have a smaller size than given by R_S. The corresponding critical mass density ρ_{crit} for BH formation follows as $\rho_{\text{crit}} = \rho_{\text{nucl}}(7M_\odot/M)^2$; it exceeds nuclear density for a solar mass, but requires only a density of a terrestrial high vacuum for galactic masses.

All these highly structured and aesthetic results about the BH solutions of Einstein's gravitational equations—obtained essentially during the 1960s—meant a great challenge to the minds of the young astrophysics community, to search for their verification in our Milky Way and beyond; where do the most convincing candidates hide? When the first pulsar was detected, in 1967 by Jocelyn Bell, it was soon interpreted (by Tom Gold) as a neutron star, with a mass in the vicinity of $1.4M_\odot$. Could neutron-star masses have a broad distribution, with an upper cutoff near their stability limit, some $3M_\odot$, and BHs beyond? Can BHs be born directly in supernova explosions, of the most massive stars? Or can they form via neutron-star accretion of mass from a binary companion, in X-ray binaries, (of which the first pulsating one was again detected by Jocelyn Bell–Burnell, in 1974)? Either of these two possibilities sounded plausible to us, including myself when I heard, spoke, and wrote about them in 1972 [14], 1973 [15], and 1976 [16]. Most impressive for me was the Varenna summer School in 1972 where the newly suspected X-ray binary system Cyg X-1 was discussed at length by the high-brow assembly, even during the lunch break, on the beach of lake Como, as the best-bet BH candidate. And there was another class of BH candidates, typically a million times larger in mass, often highly variable, located at the centers of galaxies—the supermassive black holes (SBHs)—whose power can exceed that of their host galaxies by a factor of up to 10^2. Its nearest representative, Sgr A*, at the rotation center of our Galaxy, received its name only in 1982 (by Brown), but was already considered an SBH candidate in 1971, by Donald Lynden-Bell and Martin Rees. Only at the 1976 Texas Symposium (at Chicago), the central engines (CEs) of all the active galactic nuclei (AGN) were irreversibly called BHs by Martin, after some five years of friendly competition between them and ‘supermassive stars’ (SMSs), ‘magnetoids’, and ‘spinars’.

All these thoughts and facts were more than tempting: Einstein’s GR had finally found its true crowning, by offering the likely structure of many of the most luminous sources in the sky. Whenever a physical theory had made a convincing and testable prediction in the past—I had learned—this prediction had soon been verified. I still needed friends like Hans Heintzmann, to introduce me to real astrophysics, and Tom Gold who told me about Jearl Walker’s ‘flying circus’ of physics, to form a judgement of my own.

In the present case, conclusive tests were not easy to achieve: no expected BH was near enough to Earth to just go there and look at it. Nature could have put up hurdles to their formation. None of the proposed formation mechanisms was ultimately conclusive. Angular-momentum conservation and explosive nuclear burning could delay their formation, or even prevent it for aeons. Besides, most of the suspected BH sources were (i) extremely powerful, (ii) extremely variable, and (iii) spectrally hard. The efficiency ε of BH engines in converting accreted mass-energy into (hard) radiation was (i) often judged very high, of order 10% and higher, but never realistically assessed: ordered gravitational infall into large BHs could be almost traceless for the outside world, meaning $\varepsilon \ll 1$. Next, high variability of a source (ii) requires strong deviations from axial symmetry, which (spinning) neutron-star sources are thought to achieve via strong transverse magnetic moments. BHs cannot anchor a transverse magnetic moment, was the discouraging conclusion of my younger collaborators King and Lasota 1975 [13]; their variabilities would have to be blamed on their accretion disks, a difficult task to perform. And the spectra of the suspected BH sources, which (iii) got harder and harder throughout the years, with improving observational facilities, have now grown into the TeV range (instead of peaking at X-ray energies); do they not require rapidly rotating strong magnetospheres for their boosting, according to the estimate: $\Delta W = e\int(\vec{E} + \vec{\beta} \times \vec{B})d\vec{x} = 10^{21}\mathrm{eV}(\beta_\perp \times B)_{12}(\Delta x)_{6.5}$, (2005)? After an unsuccessful (1 year) bet with Ed van den Heuvel fixed in 1976—in which he

Fig. 1 Complete rotation curves—with $10^{11} \lesssim r/\text{cm} \lesssim 10^{23.5}$—for a representative set of well-sampled galaxies, taken from [22]. For a better understanding of galactic centers, the ordinate presents average surface-mass density $\sigma(r) \lesssim v^2(r)/G\pi r$ (instead of rotational velocity $v(r)$): Whilst $\sigma(r)$ is tiny in the outer parts of a galaxy, where it is controlled by Jeans instability (to star formation), it grows considerably with decreasing r, but cannot exceed stellar values ($\sigma_* \approx 10^{11.5}$ g/cm^2), due to pressure forces, hence sets a bound on revolution speeds near the center. Observations indicate that galaxies have ringlike domains of insignificant (gravitating) mass density, between $\gtrsim 10^{14}$cm and $\lesssim 10^{20}$cm, in which their rotation is solely controlled by the mass of their central engine (CE), and $M(r) = const$. Note that the detected CE masses all stay below the BH formation limit of $10^{10.5} M_\odot$ - marked in grey - beyond which they would enforce (among others) extremely relativistic galactic revolution speeds

mentioned a possible neutron star (n*) in the Cyg X-1 system (!)—I convinced myself that an n* interpretation was indeed plausible (1979 [17]).

Returning to the possible formation modes of BHs, my decades-long engagement in supernova (SN) explosions led me to the conviction that core collapse is the only viable SN mechanism: Some day, the burnt-out, magnetized core of a massive star, of (Chandrasekhar) mass $1.4\,M_\odot$, collapses under its own gravity, transfers a significant fraction of its spin energy to its overlying magnetosphere, by flux winding, and ejects its extended, overlying, massive envelope by the joint action of a magnetic torque plus an adiabatic expansion of the magnetosphere's decay product, a relativistic cavity (2008c [24]). This mechanism is robust, sufficiently energetic to leave (even) a ms pulsar behind, and satisfies all the (phenomenological and stability) SN constraints. It may well be successful up to the highest stellar masses. It may therefore be extremely difficult to form a stellar-mass BH right away, in a SN explosion. No convincing case is known to me, after several decades of checking the literature, in which a SN had given birth to a BH: neutron stars can easily hide at the centers of SNe, and more and more of them have been detected.

So the easiest way to make a stellar-mass BH appears to be to dump matter on a n*. Now there is the Eddington limit, which prevents a n* from accreting mass at a rate exceeding $10^{-8} M_\odot/\text{year}$, due to the Leidenfrost phenomenon. All excess matter, supplied by a binary companion star, will accumulate in a surrounding (accretion) disk, conceivably piling up to five or more solar masses during its lifetime. Such massive, self-gravitating disks have the reputation of being unstable, for no ultimate reason (1979 [17], 2005 [20]). They make the encircled n* look superheavy. Binary systems of this kind are known as black-hole candidates (BHCs). Almost all other properties of the BHCs are indistinguishable from neutron-star sources: their hard spectra, fast variabilities, QPOs, lightcurve anomalies and strong emission lines, superhumps during outbursts, and in particular their jet-forming capabilities [26; 27].

What about the SBHs, with their relativistically broad iron lines in emission, which are supposed to lurk at the centers of all massive galaxies? Have they formed, and grown, during cosmic epochs? Fig. 1 shows a representative set of galactic rotation curves $v(r)$, or rather of their (equivalent) average surface-mass densities $\sigma(r) \sim v(r)^2/r$. Its upper right grey area is the BH corner; it is free of entries. Mass distributions outside of the BH corner are stabilized against collapse: radially by centrifugal forces, and vertically by pressure forces. No single CE in the plot is unstable towards gravitational collapse. Instability towards collapse is

Fig. 2 (Estimated) mass distribution of 14,584 quasar central engines (CEs) with $z \geq 0.2$, as functions of redshift z, from the Sloan Digital Sky Survey Data Release 3, within an effective sky area of 1644 $\deg^2$, taken from [33]. Squares denote median masses in each redshift bin. The dashed curve indicates faint SDSS flux limits

expected for masses in excess of $10^{10.5} M_\odot$, implying that galactic rotation speeds reach the speed of light at their periphery.

When I started contemplating about the nature of the CEs of active galaxies, back in 1977, it occurred to me that the earlier pursued models mentioned above, SMS, magnetoid, and spinar, already caught essential features of the central engines, but had predicted properties which slightly disagreed with the facts, like evolving (spin) periods, and short lifetimes. Instead, gaseous galactic disks have predicted mass spiral-in rates of order $M_\odot$/year, they will continually re-charge whatever sits at their center. Why then not simply study the innermost parts of galactic disks, whose mass densities per area are expected to grow in proportion to r^{-1}, and approach stellar values in their solar-system sized centers? These centers should behave as flat stars, or as (nuclear-) burning disks (BDs). Their radiative efficiencies should (i) easily exceed those of BH accretors; they should have (ii) strongly variable outputs, and (iii) generate relativistic pair plasma in their reconnecting magnetized coronae, ready to drive jets; their burning (iv) produces large quantities of nuclear ashes, in particular of iron [32], and they should certainly (v) blow strong winds (and thereby discharge in mass). And moreover, (vi) these BDs would conform with the cosmic evolution of the AGN phenomenon plotted in Fig. 2: Their masses would shrink with age—not grow with age, as BH masses must—when they discharge efficiently, via their strong winds. Such ideas found support by people like Peter Scheuer, John Biretta, Ski Antonucci, and others, and were published, among others, in 2002 [19], 2008a [22], b [23], and 2009 [25]. BH proponents are aware of the inconsistent (sign of) evolution documented in Fig. 2, of course, but have forced it to agree with their expectation by using words like “downsizing”, “antihierarchical”, “co-evolution”, “feedback from SNe and AGN”, and the like; the BH paradigm must not be sacrificed.

Two recent discoveries are worth mentioning. One of them is the QSO SDSS J1536+0441 with its two broad-line emission systems (plus one unresolved absorption line system), interpreted as a SBHC system by [1], as an exceptional case among $10^{4.24}$ similar entries in the SDSSurvey. The binary BH interpretation is motivated by the two broad-line systems in the spectrum, which differ in speed by $10^{3.5}$ Km/s, and asks for masses of $10^{8.9}$ and $10^{7.3} M_\odot$. As you may expect, my preferred interpretation of this source is a single BD with a second broad-line region (BLR), outside of the first (and normal) BLR, which stems from the rare event of a somewhat faster ejection of iron traversing a galactic hydrogen cloud.

The second discovery concerns SgrA*, the CE of our Galaxy, on which I wrote a detailed review in 1990, explaining all known observations within the BD model. (This paper was submitted before a compelling mass determination of SgrA*; I was not the only one, then, who got it wrong, by a factor of $\gtrsim 10^3$). Another review on this subject was written by my younger friends Fulvio Melia and Heino Falcke [28]. They kindly mention my 1990 [18] model in their section 5.3 called “Alternatives to the Black Hole Paradigm”, but state that “it can be comfortably ruled out, based on the low NIR flux at the position of SgrA* ". They did not ask me for my opinion, before submission, on their devastating statement, perhaps because they were afraid of getting involved in an embarrassing dialogue on their draft. I only saw it years after publication, by accident, whilst working on several unrelated problems. Had I been asked, I would have reminded them that the underluminosity of SgrA* at NIR frequencies is a problem which their BH model shares with mine: An engine that emits a broadband spectrum between high radio frequencies and TeV energies, with a power of $\gtrsim 10^{41}$ erg/s in its (inferred) relativistic wind, and $\gtrsim 10^{39}$ erg/s in its (mapped) thermal wind, should not be fainter than (the visible) bright stars at wavelengths above $2\,\mu$m (where foreground absorptions disappear), unless it is screened by a thick, dusty absorption layer. I would also have reminded them that from Earth, we look at SgrA* through our Galactic disk, like for (extreme) Seyfert galaxies of type 2 whose hot centers are thought to be occulted by dusty tori. These "tori" have never been resolved completely, but are thought to extend radially to distances of $\lesssim 1$ pc, and extend to fairly high ($\gtrsim 30^0$) latitudes. The tori may well be dusty winds from the hot central disk, remotely similar to the winds from supergiant stars, which are known to be opaque at almost all wavelengths. For a central galactic disk, the windzone may well have the shape of a cusp, enveloped by a dusty collar that occults the hot core from view at low angles. In the case of SgrA*, the strong, blue- and red-shifted Brγ and Brα lines visible in Fig. 7 of my 1990 [18] review, at distances of $\lesssim 1$ 1year from the center, are witnesses of a hot, screened interior.

Whilst talking about SgrA*, the nearest among all supermassive CEs in the Universe and therefore the best supermassive BHC of all, I should repeat what I already reported in my 2009 Portobello lecture notes: its non-BH character is now manifest to me in five independent ways. They are: (1) The 16 year-Kepler ellipse of star S2 around it has been reported by Frank Eisenhauer (in November 2007) not to close, by 3^0. (2) The mass estimate of SgrA* has grown throughout the past years—during its approach—from $10^{6.41}$ to $10^{6.58} M_{\odot}$. (3) The distance estimate of SgrA* implied by the monitored test-star orbits has reached 8.33 Kpc, whilst other distance estimates claim $\lesssim 8.0$ Kpc. (4) The gigantic wind from SgrA*, mapped by the blown-off windzone tails from $\gtrsim 8$ nearby stars, at distances of $\lesssim 1$ 1year, and in the light of the redshifted Brα and blueshifted Brγ line, of mass rate $10^{-2.5} M_{\odot}$/year, and speed $\lesssim 10^3$ Km/s, exceeds what can be achieved by a collection of surrounding stars, and much more so what can be achieved by a starving BH, and (5) The mysterious brightening (by 0.5 mag), and angular offset of S2 near periastron, in 2002, asks for a dense local environment [7].

With this ends my short history of Black Holes, as of 2009. I once helped studying them, and advertised them in 1972, and in my two semi-popular reports in 1973 [15] and 1976 [16]. During that latter year, first doubts in their reality emerged, and made me wonder if not more conservative sources could yield better descriptions of the proposed candidates. And by 1979 [17], I was no longer sure of their presence, anywhere among the vast zoo of astrophysical sources. With this I am not saying that black holes were unphysical: they may have formed somewhere, or may form somewhere in the future, when a lot of hydrogen has been burnt into iron. Without nuclear power, BH formation is difficult to prevent. But we live in a young Universe, and I expect BH sources to be rather inconspicuous compared with non-BH sources: with low power, low variability, and soft spectra (below γ-rays). Our life on Earth should be hardly influenced by them. Does this sound like bad news? I do not think so: our planet Earth offers an overabundant richness of physics problems, even without BHs. Let us enjoy all of them!

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