A Word from a Black Female Relativistic Astrophysicist: Setting the Record Straight on Black Holes

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ABSTRACT

This Letter is written to clear up a situation, and hopefully we will learn something from it: scientifically and morally. Herein is presented a true “historical” scenario of events, that led to my being the first person (Williams 1991) to successfully work out the Penrose mechanism in four-dimensions (three-space momenta and energy). Before working out a solution to the Penrose mechanism: to extract energy from a rotating black hole, the Penrose mechanism (since first proposed by Roger Penrose in 1969) had been attempted by scientists over the world for nearly two decades, with little success, although making some progress. In the Penrose analysis of Williams (1991, 1995) details of the behavior of efficient Penrose relativistic scattering processes in the ergosphere are described. The reason a solution eluded other scientists before me is that there was very little known about orbits inside the ergosphere, where space and time are no longer separable, as measured by an observer at infinity (i.e., far away from the Kerr black hole). In this Letter, I describe how analytic derivations of the conserved energy and azimuthal angular momentum of particle orbits not confined to the equatorial plane allowed me to succeed where others, whose “shoulders” I stood on, had failed. Also, I will mention some well known scientists in the astrophysics community by name, as I discuss their involvement, and outline the facts behind an author feeling the need to set the record straight.
I. INTRODUCTION

The problem is the following. There were, at least after May 1997 up to January 1999, two popular trains of thought associated with energy extraction and the production of jets in black holes: one is that the jets are inherent properties of geodesic trajectories in the Kerr metric of a rotating black hole, and thus, can be described by Einstein’s General Theory of Relativity (Williams 1995; de Felice & Carlotto 1997); and the other is that the accretion disk and its magnetic field through magnetohydrodynamics (MHD) are producing the jets, with the field being anchored to the rotating back hole or the accretion disk, like models of Blandford & Znajek (1977) and Blandford & Payne (1982). My suggestion (Williams 2002) is that it could be a combination of the two, with gravity controlling the flow near the event horizon, and MHD controlling the flow at distances farther away. The observations of the jet of M87 suggest this may be the case (Junor, Biretta, & Livio 1999; Perlman et al. 2001).

But, whatever the case maybe, the supporters of the Blandford-Znajek (BZ) model of today, Blandford, in particular, and his collaborators, continue to promote this model, even with the aged-old problem of converting from electromagnetic energy to relativistic particle energy, not to mention the general relativistic problems (see Williams 1999b and references therein). Now, I find nothing wrong with their continued support of BZ-type models. It is only when these supporters publish researched work from my paper (Williams 1995) in which I describe in details gravitational physics near the event horizon; yet, they do not appropriately reference my manuscript: this is where the problem lies. It seems that they are suppressing my successful, competing model—that can extract energy from a black hole—by masking my model, while using black hole physics originated from my manuscript, attempting to make BZ-type models work. Such behavior, of not references my work, has resulted in other authors taking credit for black hole physics devised by me.

The latest attempt is Komissarov’s recent paper (astro-ph/0402403, accepted for publication in MNRAS). This author claims that the driving force of the BZ model is the ergosphere, just like in the Penrose mechanism, if one assumes that the magnetic field lines are inertial frame dragged, generating an electric field that accelerates (i.e., “heats”) electrons that subsequently inverse Compton scatter soft background photon from the accretion disk. Here is where I must speak out to set the record straight! I do not know exactly where Komissarov, who acknowledges “Roger Blandford for his useful comments on this subject,” is going with this investigation. Since Komissarov has already established that the magnetic field need not anchor to the event horizon, but only be inside the ergosphere, which mildly suggests that the “classical” BZ model is not important, his direction is likely obvious and inevitable. This author goes further, implicating, as stated above, that the generated electric field heats the electrons that inverse Compton scatter soft background photon in the ergosphere. Now, based on past experience, it seems that Komissarov is being led, advised, or directed by Blandford, into eventually using my calculations (Williams 1995), i.e., first my Penrose Compton scattering in the ergosphere, then perhaps the rest: Penrose pair production ($\gamma\gamma \rightarrow e^-e^+$), Penrose pair production ($\gamma p \rightarrow e^-e^+p$); and, of course, with no intentions of referencing me as the originator, as it is with some other authors associated with Blandford.

Well, at this point, I must stop Komissarov and make him quite aware, as well as the scientific community as a whole, i.e., any that may not know, a complete four-dimensional spacetime analysis of Penrose scattering processes (inverse Compton scattering and pair production as mentioned above) has already being thoroughly investigated by Williams (1995), inside the ergosphere of a supermassive Kerr (rotating) black hole. In this analysis (1995; see also Williams 1999a, 1999b, 2001, 2002), general relativistic model calculations, surprisingly, reveal that the observed high energies and luminosities of quasars and other active galactic nuclei, the collimated jets about the polar axis, and the asymmetrical jets (which can be enhanced by relativistic Doppler beaming effects) all are inherent properties of rotating black holes. From this analysis, it is shown that the Penrose scattered escaping relativistic particles exhibit tightly wound coil-like cone distributions (highly collimated vortical jet distributions) about the polar axis, with helical polar angles of escape.
varying from 0° to 30° for the highest energy particles. It is also shown that the gravitomagnetic (GM) field, which causes the dragging of inertial frames, exerts a force acting on the momentum vectors of the incident and scattered particles, causing the particle emission to be asymmetrical above and below the equatorial plane, thus appearing to break the equatorial reflection symmetry of the Kerr metric. When the accretion disk is assumed to be a two-temperature bistable thin disk/ion corona (or torus ≡ advection-dominated accretion flow), energies as high as 54 GeV can be attained by these Penrose processes alone; and when relativistic beaming is included, energies in the TeV range can be achieved, agreeing with observations of some BL Lac objects (Williams 1999b).

These calculations of the Penrose mechanism (Williams 1995) have already revealed in details how energy is extracted from a black hole, suggesting a complete theory, (1) without the need of a large-scale accretion-disk magnetic field, which, according to general relativity, will not effectively exist, upon nearing the event horizon; and (2) without the associated problems arising when attempting to use the disk magnetic field in direct extraction of energy from a black hole.

So, what use is there for the BZ-type model proposed by Komissarov? The Kerr rotating black hole gravitationally blueshifts particles to higher energies, so there is no need for this to be done by the disk magnetic field, assuming it could effectively exist in the ergosphere, as proposed by Komissarov, but contrary to recent findings that the flux of such a field will be expelled or redshifted away (Bičák 2000; Bičák & Ledvinka 2000), relative to the observer at infinity as well as locally.

Moreover, it does not surprise me that an author, that has collaborated with Blandford, on some level, would state that the magnetic field is dragged around by spacetime in the ergospheric region. Notice the similarity to my proposing (Williams 2001, 1999a), for the first time ever, that the field lines of the GM field (i.e., the gravitational analog or resemblance of a magnetic field) inside the ergosphere are inertial frame dragged, causing symmetrical and asymmetrical jet forces in the polar direction, which is based on detailed analytic calculations and computer simulated results. Note, I recently (April 2004) found in the literature that Bičák & Janiš (1985) were the first to propose magnetic field lines are frame dragged into rotation by the geometry, inducing an electric field; why did Komissarov fail to reference this paper?

This in general has happened a number of times, concerning my work. It appears that Blandford, down through the years, since our first contact in 1992 in which he became familiar with my work (as will be described in this Letter), has constantly advised his collaborators and graduate students to do black hole physics worked out and/or proposed by me, yet not appropriately referencing the origin.

Peer review processes, associated with manuscript submissions to The Astrophysical Journal (ApJ) and Physical Review D (PRD), and research proposals submitted to granting programs of NASA and the National Science Foundation (NSF), if dishonesty is involved, as it rarely is, provide the perfect means for an author to gain access to another author’s unpublished works. Those of you who have served as referees or grant proposal panelists know that it is all about the integrity of the individual, that keeps one from wrongdoing.

Note, as an originator of much of the “modern” black hole physics of today, based on my 41 page paper published in PRD (Williams 1995), scientists at NASA’s Astrophysics Theory Program, NSF, and ApJ, have spoken highly of my work: She has contributed significant to the field (NASA 1997). She has impressive experience in analyzing rotating black holes, and has developed important insight into the details of processes needed to liberate energy in the form of jets (NSF 1999). She is an expert on this kind of physics and her three-dimensional analysis of the energy extraction process has contributed to our understanding of the Penrose mechanism (NSF 1999). Her work on the Penrose mechanism, and how one might use it in a quantitative manner to extract energy from within the ergosphere of a spinning black hole using the solutions that she developed in the early 1990’s, is first-rate (ApJ 2004).

So, now you are probably wondering, if Williams’ model does all that she claims, then why have we not heard more of her work? In the paragraphs below, I will outline the chain of events, explaining how up until 1999, I was beginning to get international recognition from the astrophysics
community for the black hole physics introduced by me in Williams (1995), and then it was abruptly taken away from me by a group of scientists (collaborators, graduate students, friends, etc. of Blandford). These scientists began applying my devised black hole physics to BZ-type models, but, as mentioned above, were not appropriately referencing my paper. This resulted in these scientists being given credit for my work. I am appealing to the scientific community to help me set the record straight.

I always believed that I would be respected as a Black person for my knowledge. This was one of the reasons I was determined to get my PhD, and with me being the first Black female astrophysicist in our nation, it was not easy, yet I pressed through. Now it seems that my knowledge is being unethically used against me, by some authors, for selfish and economical gains. That is, by not appropriately referencing me as the originator of researched black hole physics presented in such a classic, important paper (Williams 1995) has resulted in certain authors taking credit for my research, and thus, causing my career to suffer. Is it because I am Black, or because I am a woman, or both?

In the following sections, the events leading to the problem stated above and people involved will be presented, alone with documentation when needed for credence and/or clarity.

**II. GRADUATE SCHOOL**

While studying physics as an undergraduate, I realized early on that my two favorite forces are gravity and electromagnetism. So, while in graduate school, every chance I got, I did a paper, gave a talk, or worked with some faculty person on a research project having to do with one of these forces, whether it was studying the magnetic field structure of disk galaxies, magnetic fields in jets, magnetic fields of neutron stars and white dwarfs, or just working problems in electromagnetism for a summer research project—I was always eager to learn more so that I could apply the knowledge to solving problems of the universe related to these forces. Since, however, done of the faculty members at Indiana University, Bloomington, were doing relativistic astrophysics research, I taught myself General and Special Relativity at an advanced level, from many books, including Weinberg (1972) and scientific journal papers.

Because I was not working on any of my advisor’s grants, I had to work and save money to attend conferences on my own. I attended the 1988 Texas Symposium on Relativistic Astrophysics, hoping to meet Penrose and some of the other conferential scientists, including those whose work I had become familiar with through their journal papers and correspondence, such as Williams Fowler, Blandford, Martin Rees, Stephen Hawking. My mission was to find out why, it seemed, no one was working on the Penrose mechanism. Yet, I would still see it mentioned in text books, as if people still believed in its potential. But I could not find anywhere in the literature of it being worked out.

Below, I give a brief chronological account of my research projects in graduate school, as a Research Associate (1984-1991), before getting my PhD:

**1984-1985:** This marks the beginning of my dissertation research. My desire was to created a model to explain the core energy source of quasars and other active galactic nuclei (AGNs). Next, I desired to use this energy source to generate the jets associated with radio strong AGNs. To begin with, I duplicated William Fowler’s supermassive star ($\sim 10^8 M_\odot$) model computer simulation of a $n = 3$ polytrope as a possible energy source of quasars. A problem encountered in using these objects is that the life expectancy of supermassive stars shining on nuclear energy is only $\sim 10^6$ years. Quasars are thought to have ages up to $10^9$ years: supermassive black hole models with accretion disks can supply the necessary energy for at least $10^8$ years; however, supermassive stars could very well be progenitors of such black holes (Fowler 1985; by correspondence).
1986-1987: I computed a model simulation of a thin disk/ion corona accretion, surrounding a Kerr black hole, with accretion rate $\dot{M} < \dot{M}_{\text{Eddington}}$. This model combined the thin accretion disk of Novikov and Thorne (1972) with the two temperature ion corona (or thick disk) of Eardley and Lightman (1975); Eilek (1980); Eilek and Kafatos (1983). This computer generated model readily gives parameters of the disk, such as number density, pressure, temperature, height, etc., at a given radius, before and after instability sets in, i.e., the secular instability that makes the thin disk swell into a thin disk/ion corona bistable geometrical structure. The problem one encounters here is that the accretion disk can only give rise to particle energies up to $\sim 100$ MeV (Eilek 1980). Yet quasars are observed to radiate energies up to at least $\sim$ GeV. So the only place I had to turn to, hoping to get the high energy needed to duplicate the observed spectra of quasars, was the black hole.

1987-1990: I computed a Monte Carlo simulation of Compton scattering in the ergosphere of a supermassive Kerr (rotating) black hole, using the Penrose mechanism to extract rotational energy. A similar calculation was previously done by Piran and Shaham in 1976; however, they applied the equatorial escape conditions, i.e., for photons confined to escape along the equatorial plane, thus, ignoring the escape conditions for scattered photons above and below the plane, assuming that the equatorial escape conditions were not much different from the nonequatorial escape conditions. Yet, I found that the escape conditions were very different, thus, making their assumption invalid. In my model calculation, I apply the nonequatorial escape conditions to the scattered photons (i.e., escape conditions that take into account motion above and below the equatorial plane). However to do this, I had to derive analytical solutions for the conserved energy and angular momentum of nonequatorially confined orbits of massless and material particles; such solutions had never been derived before. These derived solutions allowed me to apply the nonequatorial escape conditions, from which I obtained higher energies and more photons escaping than obtained by Piran and Shaham. The results of my model calculation show that the Penrose Compton scattering can generate hard X-ray/soft $\gamma$-ray spectra in agreement with observations of AGNs. But more so, this black hole model calculation can incorporate other scattering processes that could generate even higher energies. The results of the above investigation was presented in my thesis [Williams, R. K., Master’s Thesis, Indiana University, Bloomington (1990)].

1991: For my dissertation (December 1991), I presented Monte Carlo computer simulations of Penrose Compton scattering (PCS) and Penrose pair production production (PPP) processes, $\gamma p \rightarrow e^- e^+ p$ and $\gamma \gamma \rightarrow e^- e^+$, in the ergosphere of a supermassive ($10^8 M_\odot$) rotating black hole. This mechanism, as applied in these calculations, can extract hard X-ray/$\gamma$-ray photons from the inverse Compton scatterings of initially low energy UV/soft X-ray photons by target orbiting electrons in the ergosphere. Such low energy infalling photons are consistent with photons emitted by that of a thin disk/ion corona accretion. These model calculations also allow relativistic electron-positron ($e^- e^+$) pairs to escape with energies as high as $\sim 4$ GeV; these pairs are produced by infalling low energy photons interacting with target photons in bound orbits inside the ergosphere, at the photon orbit. This process may be the origin of the relativistic electrons inferred from observations to emerge from the cores of AGNs. Also discovered in these model calculations were that the Penrose scattering processes naturally produces jets of relativistic particles aimed toward the polar axes, and in most cases the jets are somewhat one-sided, agreeing with observations of AGNs. Overall, these Penrose processes can apply to any mass black hole, more or less, depending on the characteristics of the accretion disk, and suggest a complete theory for the extraction of energy from a black hole. [Note that, the above investigation
III. POSTDOCTORAL ERA

As I applied for postdoctoral fellowships, I sent my Proposed Plan of Research (1992) to various institutions. On one occasion, I sent, along with the Proposed Plan of Research, a copy of my dissertation. This was sent to Kip Thorne at California Institute of Technology (Caltech). This occurred after applying for a postdoctoral position at the Space Telescope Science Institute, and being told by Meg Urry in November 1992 that she would be glad to give me a position, but it would be observing. She said that I needed to be in a position where I could do theory. She then suggested that I contact Martin Rees or Kip Thorne. I was quite honored, and replied that I never thought about contacting these scientists of such great status. She then replied, “but you are good.” Her remarks were based on comments told to her by Jean Eilek, her friend, who was also on my dissertation committee.

Since, I had already made plans to attend the 1992 Texas Symposium on Relativistic Astrophysics, held in Berkeley, CA, to present my research and to meet with Penrose, giving him a copy of my dissertation, telling him that “it worked,” and since Kip Thorne was on the conference committee, figuring that I could talk with him at the meeting, I sent him a copy of my dissertation and my Proposed Plan of Research, inquiring about doing a Ford Foundation Postdoctoral Fellow at Caltech.

In the Proposed Plan of Research, I proposed to investigate Penrose processes in the electromagnetic field of the Kerr-Newman (rotating and charged) black hole (see also Williams 1995, p. 5423), repeating the calculations of the Penrose scattering processes, that were initially done in the field of a rotating Kerr black hole. Since NASA’s Gamma Ray Observatory (GRO) had recently confirmed that quasars (at least 3C 273 and 3C 279) radiated up to $\sim 10$ GeV, and my model at the time, based on limited knowledge of the accretion disk, extended only up to $\sim 4$ GeV, but could extend higher in an appropriate magnetic field, plus I needed a way to generate the electromagnetic jets: to accelerate and collimate the Penrose escaping particles, led me to propose using the Kerr-Newman black hole. That is, I had found a way to extract high energy particles; now I was ready to create an electrodynamic model to generate the jets (proceeding on with my initial plan made in graduate school). I had proposed to let the magnetic field of the Kerr-Newman black hole interact with the Penrose escaping electron-positron ($e^- e^+$) pairs. From a purely electrodynamic point of view, I proposed to have the Keplerian conducting accretion disk move through the dipolar-like magnetic field of the black hole, inducing an electric field, similar to that of Lovelace (1976) and Blandford (1976), but in my case the high energy particles would already be available, and not have to be created in some complicated improbable way in the field, as proposed in the above 1976 models. Also, since my disk would be treaded by the magnetic field of the black hole ($B \sim 10^{10}$ gauss), as opposed to the field of the accretion disk (up to $B \sim 10^8$ gauss), a more powerful jet would be expected. Finally, I proposed to compare my results with some of the “opposing” models that proposed using the electromagnetic field of the accretion disk (Blandford 1976, Blandford & Znajek 1977; Blandford & Payne 1982; Lovelace 1976, Scott & Lovelace 1982; Burns & Lovelace 1982; Punsly 1991)—as opposed to using the electromagnetic field of the Kerr-Newman black hole—to generate the astrophysical jets of AGNs.

Note, in the Proposed Plan of Research, I added, stating, “From this project, I will receive the necessary background knowledge needed in order to begin a successful research career, studying general relativistic astrophysics and electromagnetic fields of massive extragalactic bodies. I will also apply this knowledge to phenomena in cosmology—especially to phenomena pertaining to the early universe.”

I arrived in California at the 1992 Texas Symposium on Relativistic Astrophysics meeting on a Sunday, and would present a poster paper on the following Wednesday. I found Penrose right away. I had planned to meet Thorne and Blandford also. Now, Thorne was okay, i.e., as far as his research goes. But Blandford was the scientist of whose work I had investigated in details, in my
quest to find the “perfect” quasar model.

Deviating briefly, Blandford was one of the astrophysicists, I had concluded, whose model came close to getting it right, he and Lovelace. While in graduate school, I studied both their models, for the generation of jets by electrodynamics: The Lovelace type models (Lovelace 1976, Scott & Lovelace 1982; Burns & Lovelace 1982) assume $e^- e^+$ cascades are accelerated by the electric field, generated from a magnetized rotating disk. The Blandford type models (Blandford 1976, Blandford & Znajek 1977; Blandford & Payne 1982) assume the disk magnetic field anchors to the event horizon, i.e., “surface” of the black hole, extracting electromagnetic energy due to magnetic braking, or anchors to an accretion, such that centrifugal forces launch or lift accretion disk particles to relativistic speeds. But the main problem with both these jet models was converting from Poynting flux of electromagnetic energy-momentum to relativistic particle flux energy-momentum. In both cases, the magnetic field had to be unphysically large or the particle density had to be unphysically small. In essence, Blandford and Lovelace needed a way to get the high energy particles, that could subsequently interact with their proposed electromagnetic field, to generate the jets. So, standing on the “shoulders” of Blandford and Lovelace, I figured I could use their way to accelerate and collimate the particles—of course, appropriately referencing their work; but first, I had to get the relativistic particles needed. My main focus became, at that point, to find a way to get the $e^- e^+$ pairs.

After searching the literature, I was led to the Penrose mechanism by some authors (Piran, Shaham, & Katz 1975; Leiter & Kafatos 1978; Kafatos & Leiter 1979; Kafatos 1980; Piran & Shaham 1977a, 1977b; Eilek & Kafatos 1983) who came close to working it out. But different from them, however, standing on their “shoulders,” I was able to extract high energy photons, and most importantly, to extract relativistic $e^- e^+$ pairs in the form of jets, thus, working out the Penrose mechanism.

Note, my solving the orbital equations for the conserved energy $E$ and azimuthal angular momentum $L$ (Williams 1991, 1995), of nonequatorially confined particle trajectories, above and below the equatorial plane, allowed me to do the PPP ($\gamma\gamma \rightarrow e^- e^+$) at the photon orbit. This is because the bound photon can only exist in nonequatorially confined orbits in the Kerr metric.

So, as mentioned earlier, for my postdoctoral fellowship, I proposed to use the magnetic field of the Kerr-Newman black hole, comparing it to that of the accretion disk, to further accelerate and collimate the Penrose produced escaping particle, expectedly, out to the observed distances. It did not matter to me which would be more important, the field of the black hole or that of the accretion disk: I just wanted to get to the scientific truth. When I wrote to Thorne, to inquire about coming to Caltech as a postdoctoral fellow, I thought he too wanted to know the truth, for I knew that I had part of it [i.e., the relativistic $e^- e^+$ pairs from PPP ($\gamma\gamma \rightarrow e^- e^+$) at the photon orbit].

Below I present excerpts from an email letter sent to me from Thorne in response to my inquiry concerning the Ford Postdoctoral Fellowship. But first I must return to mentioning the occurrences at the 1992 Texas Symposium surrounding my meeting Thorne. After working out the Penrose mechanism in 1991, showing that the energy observed astronomically can be extracted from rotating black holes, and thus, placing them at the cores of quasars, in 1992 NASA’s GRO began confirming my model. But, when Carl Fichtel of NASA gave a talk that Tuesday on the new high energy GRO results, towards the end of his talk, he made the statement, along with showing a schematic drawing, that recently Kip Thorne and his group (with my name not mentioned) suggested to use the magnetic field of the black hole! This was my proposal that I had sent to Thorne a few weeks earlier.

I was literally crushed. On my way out of the auditorium, almost in tears, I met Janna Levin, a then graduate student at MIT, and expressed to her what had taken place. She try consoling me because the evening before, at dinner, we, and some others, including her research advisor, had discussed such unethical behavior. I then looked for Thorne, but was told that he would not be at the meeting until Friday.

During my poster presentation the following day, I told everyone that I could what happened (i.e., the fact that my model had been mentioned by Fichtel), in my desire, now, to find just
what type of person was Thorne, for I had never met him personally. The feedback was not very encouraging. I even told it to Meg Urry, who also attended the meeting. She thought it was a good thing, and that I could work with Thorne on the project. I also heard that Thorne was at odds with the astronomy community because of the large funds he was taking from the granting agencies to complete the LIGO project. I figured, okay, if he just wanted to “borrow” my idea, just so that the community would think that Kip Thorne is still in the business of new ideas, then fine. However as you will see from the letter exerts below, Thorne’s intentions were quite contrary to what Urry and I thought.

Finally, when Thorne arrived at the meeting on Friday, someone came over and pointed him out to me. I went up to him and introduced myself. He responded that he would talk with me after doing lunch with Stephen Hawkin.

After his lunch, while I was talking with Janna, Joseph Silk, and another scientist, Thorne came up to me stating he was ready to talk. He then took me around the corner away from the crowd. I immediately asked him what did he think of my proposing to use the magnetic field of the Kerr-Newman black hole. His response was that he had not gotten to that, but, he was just so interested in my cover letter. I said, “But you had to, because Carl Fichtel mentioned that Kip Thorne and his group suggested to use the magnetic field of the black hole.” He responded that he was not there, and therefore, he did not know what Fichtel said. When I saw Thorne appearing to play the “I don’t know role,” I backed off. I surely did not want a man of his status to come down on me (a person just beginning my career). So, I changed the subject, asking what I would need in regards to my application package for a postdoctoral position at Caltech. We talked more about the application, and he gave me his email address.

When I got back home to Indiana University, I wrote Thorne a letter, telling him the story of my long efforts, i.e., my long hours spent and years of research to find a model that explained quasars. I expressed to him that I was almost there: I had the particles needed to interact with a surrounding electromagnetic field, that could possibly generate the jets out to Kpc distances. I wanted to convey to him of my being a real person, and not just something you can, or would even want to, take or steal from.

I continued to complete my Ford Foundation Postdoctoral Fellowship application, with Caltech as the host university. After completing my application all but the host letter from Thorne, and the deadline having passed, although the office was willing to make an exception, I contact Thorne to remind him. Below are exerts from the email of February 25, 1993 Thorne sent to me:

Dear Reva [Kay],

I am replying...to your application to come to Caltech on a temporary research position, with your own salary support. The bottom line is positive, but with a caveat.

Roger Blandford, Sterl Phinney, and I have all read the letters of recommendation that you arranged to be sent, and your research proposal and portions of your thesis. On these basis, we would be happy to have you as a member of our research group next year, and would look forward to interacting with you. however, none of us is enthusiastic about your proposed continued research on the Penrose process as an energy source in AGNs. We (particularly Blandford and Phinney, who are much better experts on this than I) believe this process is NOT very promising. We also, quite generally, believe that postdoctoral researchers should branch out in new directions, different from their thesis work; this is important if they are to be strong scientists.

Accordingly, we would be happy to accept you into our group if you will agree that you will spend the majority of your research and writing time on new research directions, different from the Penrose process, directions to be selected in consultation with Blandford, Phinney, and me. (I would encourage you, however, to finish writing up your past Penrose process research in parallel with launching one or more new research directions.)

Your new research directions could concentrate on relativity... I would be especially interested in seeing you get involved with issues in the generation of gravitational waves by
astrophysical systems (e.g. coalescing binaries) in connection with the LIGO Project. . .

Please let me know your reaction to this. If it seems acceptable to you, then I will write to the . . . Fellowship program . . .

Best wishes,

Kip

When I showed this letter to one of my dissertation advisors, he did not understand why they would advise me not to finish my initial proposed project (which consisted of two parts) as a postdoctoral fellow, in addition to finish writing up “Part 1,” especially since I had had so much success with Part 1. (Part 1 was to extract the relativistic particles, particularly the $e^-e^+$ pairs; and “Part 2” was to use the surrounding magnetic field, interacting with these particles, to generate the jets. My advisors thought that the completion of Part 1 was enough to get my PhD, calling it “a very fine piece of work and I am sure she will continue to expand fruitfully upon it”; it is “beyond the ability of many of her contemporaries”; “a first-class piece of work.”) Other than that, one might think that the letter above is not so bad. However, considering what had taken place at the Texas Symposium, a few weeks earlier, i.e., how Fichtel announced that Kip Thorne and his group suggested using the magnetic field of the black hole, one can not help but conclude that something is amiss. If Thorne and his group at Caltech were proposing to use the magnetic field of the black hole, independent of my proposal, then why would they not want me to work on the same model with them, since I had devised a theoretical and numerical model to extract the relativistic particles that would be needed?

The reason why, it seems, is that Thorne and his group, including Blandford and Phinney, wanted me to stop working on the Penrose mechanism, so that they could use my researched black hole physics, and results, to resurrect their models of the 1970’s and early 1980’s, attempting to make them work at least as good as mine, and if that failed, just take mine—sooner or later. The literature reveals that this “plan” went into full motion in 1999.

What, however, probably delayed putting this so-called plan into full motion until 1999, other than the lack of opportunity, is my continued quest to use the Penrose produced particles and electrodynamics to generate the observed jets. I decided to attend University of Florida (UF) instead of Caltech because (1) I initially wanted to do my postdoctoral work at UF, working with the relativistic astrophysics group there; (2) I could continue my research on the Penrose mechanism as an energy source for AGNs, with the support of my postdoctoral advisor, Henry Kandrup: for there was still much to be revealed about the four-momentum trajectories of the escaping Penrose particles in the Kerr metric, that needed to be investigated. Now, as far as my understanding of the trajectories, I was making considerable progress, finding that many of the features I expected to be produced by electrodynamics were actually being intrinsically produced by the Kerr black hole. But each year, from 1995, after my 41 paper (Williams 1995) was published in PRD, over and over, I submitted follow-up papers to ApJ and/or PRD, to share my new findings, and to improve my credentials, as well as submitted proposals to NSF and NASA’s grant programs, to support my research as first a postdoctoral associate, then as assistant scientist at UF, only to be rejected or denied time after time. So with no new refereed papers published, no funds, I was forced to accept positions that were not conducive to my research, working basically in a “vacuum”: a visiting assistant professorship at North Carolina A & T (1997-1998) and an associate professorship at Bennett College (1998-2002) before returning to UF, to effectively continue by research, since both these past positions required a lot of teaching, leaving little time for research projects. Yet, I still managed to present my research at some international conferences and workshops [e.g., Texas Symposium on Relativistic Astrophysics (1996, 2000), Marcel Grossman Meeting on General Relativity (1997), American Physical Society (1998) American Astronomical Society (2000), Aspen Center for Physics (2000)], to promote my model of AGNs, and to get some papers published in proceedings. But, I had no luck in getting my papers published in ApJ nor PRD; it seemed that both these journals, I was told, had difficulty in finding referees that would give a report: For example, a certain referee would agree, then many months later state that they cannot give a
report. The reason, I was told by editors of ApJ and PRD, is that the subject matter is not a familiar one. Finally, when a referee would agree to give a report, it would be extremely negative. This happened over and over again; with each revised improved submission, the reports got more and more negative. Even when I submitted Williams (1999a) to General Relativity and Gravitation (GRG) in 1997 (after the 1996 submission was rejected by PRD), one of the two required referees recommended publication, but unfortunately the second referee was the same referee of my PRD submission, and his same negative comments (even though the manuscript had been revised), as well as his status in the astrophysics community, ruled; a third referee suggested that the paper be submitted to MNRAS or ApJ. Eventually, the editor of GRG suggested I resubmit the manuscript to PRD—since this is where my previous manuscript on the subject had been published, and so I was right back where I started. Yet, I was determined not to give up on my AGN model, particularly, since new observations were becoming more and more consistent with my results. I knew it would only be a matter of time before my hard work and longsuffering would pay off. But then something happened in 1999.

Right around the time the panel at NSF met, in early January 1999, to review the fifth proposal I had submitted, another one I was not awarded, although this year it was recommended to “award” me, “this worthy Principal Investigator” (see also the above comments in Section I), an ApJ Letters paper by Krolik (1999) was received January 13, 1999, apparently using black hole physics presented in my manuscript (Williams 1995), of the orbits inside the marginal stable orbit (or the so-called plunging region), properties and conditions I devised, to work out the Penrose mechanism. Krolik applies this physics to the BZ-type models. His conclusion of possible magnetic torque inside the plunging region was, with little doubt, based on my results, which showed that particle orbits could exist in this region, inside the marginally stable orbit, contrary to what was thought previously by accretion disk theorists. Krolik even states “there is the potential here for a realization of the Penrose process,” yet, does not reference my work. Moreover, in the Acknowledgements, he thanks Mitch Begelman, Roger Blandford, and Scientific Editor Ethan Vishniac of ApJ. Note, in my proposal to NSF (1999), I requested funding to investigate a three-dimensional time-dependent evolution of the jets intrinsically produced by the Penrose process: without and with the accretion disk magnetic field, to see if the jets extend to their observed distances.

In addition, similarly, after the panel met that January, an ApJ Letters paper by Gammie (1999), received April 6, 1999, claimed to have worked out a model developed by Takahashi et al. 1990, the so-called MHD Penrose process, stating that energy is being extracted in this model, from within the ergosphere. Not only did it appear that Gammie used the particle orbits derived in Williams (1991, 1995; with the results presented again in Williams 1999a), enabling him to get rid of a degree of freedom by expressing \( E = E(L) \), where \( E \) and \( L \) are the conserved energy and azimuthal angular momentum as measured by an observer at infinity, but it seems he also used a modified version of my initial conditions, and final analytical expression that transforms from the local nonrotating frame to the Boyer-Linquist frame (i.e., the frame of the observer at infinity), which shows the Penrose energy extraction process. The “guiding center approximation” stating that the single particle approach must be used when a fluid, as in MHD, is in a significantly strong gravitational field, allowed him to use this final expression (for further details see Appendix of Williams 1999b). Thus, it is no wonder that he was able to extract some energy, but little—only because of the physical way he set the problem up. Nevertheless, it clearly shows that my equations work in a general sense. Note, however, due to the complexity of the problem, it is literally impossible for Gammie to have known how to use these relations except having prior knowledge of them being used in my detailed calculations (compare Williams 1995). Now, with Gammie performing a one-dimensional space Penrose analysis: within a magnetic field, would not it be appropriate for him to reference my three-dimensional space Penrose analysis: without a magnetic field? at least to compare the results. Yet, he does not, which is not surprising, for Gammie (1999) acknowledges that he is grateful to both Blandford and Krolik.

Concerning the above, I am not sure who served on the NSF 1999 panel, and may never know, or if it had anything to do with the timing, but after January 1999 a large flux of related papers began being published in ApJ and PRD, altogether tied with Blandford and/or Thorne...
and Phinney, with each using the particle orbits devised in Williams (1995), applying them to the BZ-type models, more or less, claiming to have worked out the Penrose mechanism; or using these orbits in gravitational wave phenomena (e.g., coalescing or merging binaries), claiming to have solved the three-dimensional Kerr trajectories for $E$ and $L$, independently of my solutions or numerically (see e.g., Hughes & Blandford 2003)—anything but to appropriately reference by paper. Strangely, out of all the recently proposed black hole models for AGNs, by authors that I can identify as using my papers, or my proposals, are closely affiliated with Blandford, including the work of Meier & Koide et al. (2000, 2002). This has occurred too frequently to be a coincidence. Even if these authors were not aware of my work, one would expect Blandford to at least bring it to their attention, so that they could appropriately reference my papers. Similarly, Begelman, who also knows of my work (Williams 1995), a collaborator and friend of Blandford, a co-author of the classic paper by Wilms et al. (2001), which reports observations of the X-ray illumination from within the central region of the accretion disk, suggesting that energy is being extracted from a rotating black hole, mentions in this paper the BZ model, but then associates it with the Penrose mechanism. Yet, there is no mentioning of my model, which shows in details how Penrose Compton scattering within the marginally stable orbit produces escaping X-rays (Williams 1995, 1999a, 2002) that would naturally illuminate the accretion disk, giving rise to a steep emissivity profile, being consistent with these observations (Williams 2004).

Now, the Proposed Plan of Research mentioned in Section I, to use the Kerr-Newman black hole, was before finding out about the vortical escaping Kerr orbits, in 1997, from Fernando de Felice at the Marcel Grossmann meeting in Israel. After my Parallel Session presentation (Williams 1999c), concerning the influence of the gravitomagnetic field on the trajectories of escaping Penrose scattered particles, he wanted to know if the particles escape along vortical orbits. We talked and he directed me to a paper (de Felice and Carlotto 1997) describing gravity induced vortical trajectories. At the time, however, I knew that my particles escape with large angular and fairly large polar coordinate momenta, with relativistic energies, but that was all, which is why I proposed to do a time-dependent evolution of the escaping trajectories. Yet, it was the paper by de Felice and Carlotto (1997) that led me to investigate, finding that the Penrose scattered particles escape along vortical trajectories collimated about the polar axis in the form of jets. Such vortical trajectories are a general relativistic effect due to the frame dragging and the transferring of orbital angular momentum to the scattered particles, as well as the GM force acting on the momentum of the particles (Williams 1999a).

IV. PRESENT ERA

It now appears that I have everything needed to generate the jets, even an intrinsically induced dynamo magnetic field due to the escaping plasma (i.e., charged particles), that could magnetically aid in confining the particles. Such a magnetic field, being similar to that of a solenoid, would be carried along with the $e^-e^+$ plasma jet. This however needs further investigation.

Importantly, in these Penrose processes we do not need the magnetic field of the accretion disk to “communicate” between the accretion disk and the black hole. Therefore, there is no need for the BZ type models (and their many associated problems) in the direct role of energy extraction from a spinning black hole. But the presence of BZ-type models appears to be need once particles are on escaping orbits, serving the same effects they do in the jets of protostars, i.e., seeming to have a dominant role on a large scale, within the weak field limit, at distances outside the strong effects of general relativity.

As for producing the observed synchrotron radiation, indicating the present of a magnetic field near the core region, such radiation could very well be produced by the expected intrinsically self-induced magnetic field due to the dynamo-like action of the escaping Penrose produced $e^-e^+$ pairs, escaping on vortical, coil-like helical trajectories concentric the polar axis, in the form of a swirling “current” plasma. This, therefore, adds more to the unimportance of the accretion
disk magnetic field near the event horizon. Now, whether or not the helical jet structure of the escaping Penrose particles and the intrinsically induced magnetic field are related to recent VLBA polarization measurements (see Homan 2004 and references therein) remains to be seen; at least, however, the helical trajectories of the escaping plasma are consistent.

V. CONCLUSIONS

In this Letter, I have described events that led to my needing to speak out to set the record straight on modern black hole physics. In closing, I want to apologize to the innocent, i.e., the ones that were unaware that it was my work being suggested to do by Blandford, Thorne, Phinney, Krolik, and/or Gammie. For example, former Caltech graduate student Hughes, who was apparently advised by Thorne, Blandford, and Phinney to “rederive” $E$ and $L$, presents his derivations in a series of papers published in PRD (Hughes 2000, 2001a, 2001b), yet did not reference my manuscript(s), even though it is quite clear that he used analytic equations unique to my derivation presented in Williams (1991, 1995, 1999a). For completion and comparison below, I derived functions $E(a, r, Q)$ and $L(a, r, Q)$; Hughes derives $L(a, r, E)$; $Q(a, r, E)$ and $E(a, r, L)$; $Q(a, r, L)$, where $Q$ is the so-called Carter constant, and $a$ is the angular momentum per unit mass parameter. But I do not fault this former graduate student: for he was only doing what he was advised to do. Yet, I do fault Editor Nordstrom of PRD for permitting it to happen, who was well aware of my work, and of my being Black, from my repeated manuscript submissions and rejections, while papers of models by graduate students of Thorne, Blandford, and/or Phinney, using black hole physics presented in my manuscripts, were allowed to be published. Moreover, in a recent ApJ Letters paper by authors Hughes and Blandford (2003), entitled “Black Hole Mass and Spin Coevolution by Mergers” in which the nonequatorial orbital conserved energy $E(a, r, Q)$ and $L(a, r, Q)$ are needed, just the very functions derived in Williams (1991, 1995), and the simplicity of taking the derivative of Equation (A13) of Williams (1991, 1995), solving for $Q(a, r, E, L)$, yielding $Q(a, r)$, one cannot refrain from questioning the truthfulness in the statement made by these authors that the conserved quantities: $E$, $L$, $Q$ were solved for numerically, particularly when we know that Blandford had knowledge and access of analytical solutions from my work.

Now, I know that there were a few other scientists involved, but the senior scientists named (particularly, Blandford, Thorne, Phinney, Krolik, Gammie, Vishnai, Nordstrom), based on a chain of events, played the most prominent roles. So, overall, I think I pretty much have the scenario right, i.e., as to how I believe that my intellectual property: the knowledge (or research) of the black hole physics conveyed in my papers, and in my various grant proposals, was “stolen” from me, and given to some other scientists to use in their models, where in many cases, those scientists have gain recognition for that very knowledge used. Thou shalt not steal.

I realize that one can not reference every paper one obtains knowledge from, particularly if the material is not applied directly. But having knowledge that I successfully worked out the Penrose mechanism (Williams 1991, 1995) and to mention the Penrose mechanism in your manuscript, and willfully not mention by work, is straight-out scientifically morally wrong!

Scientists out there that know me, know that I am a nice hard working person, who loves science, particularly relativistic astrophysics, and I do not deserve what I have been put through these past years. It has been distracting and has kept me from the science I am capable of doing; therefore, it must stop. This Letter, I am praying, shall do the job.

Finally, we study science for the beauty of it. It is interesting how we are steadily, it seems, being revealed more and more of the mysteries of the Universe and the physics of how things of nature work. These revelations are being revealed, however, to scientists that work hard to seek answers in a righteous way. This goes beyond the color of ones skin and the texture of ones hair. Every scientist wants recognition for his contribution to science, just like the athlete wants to win, yet they both enjoy “playing the game.” Ones ethnic race should not exclude a scientist from being recognized for their contributions. So I am appealing to the scientific community, give to me the
recognition that I am due for my contribution to the study of black hole physics.

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