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Manifesto of a Space Scientist

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Key Points:

- Postdoctoral mentors are critical for the career of a scientist.
- It is easier to get forgiveness than permission.
- Do not be afraid to hire very talented people.

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Abstract
In my five decades as a space scientist I experienced many successes, failures, struggles and joys of solving important (and not so important) problems. I documented my story in my book "Phoenix" (T. I. Gombosi, 2013) and up-to-date information about my work can be found at my personal website (T. I. Gombosi, 2022). In this article I am summarizing some of the lessons I learned. I hope you will find my "manifesto" useful and entertaining. Each point has real life experience to support it. Together, they pretty much summarize my philosophy of scientific research, project management and leadership.

1 Mentors and Opportunities

Mentoring is perhaps the most important task of an established senior scientist. In my experience the best predictor for future success of a young scientist is the status of his/her postdoctoral mentor. Education, talent, communication skills, and other factors also play an important role, but mentoring is the most important.

I was very fortunate with my mentors. I had a very atypical career path (at least by US standards) since I earned all my degrees in Hungary and came to the USA in my thirties (see my autobiography/family history, T. I. Gombosi, 2013). As I sometimes describe it, "I earned my PhD before I had a drivers license."

Career changing opportunities only happen once in a while. It is up to us to recognize them and take full advantage of them. I, just like most other scientists, have had several such opportunities, and I took advantage (willingly or not) of most of them.

1.1 Kicking and Screaming to Space Science

My first "opportunity" came right after I graduated with a Master's Degree in physics. My MS thesis was in theoretical high-energy (or elementary particle) physics, a very popular subject at that time. My dream was to join the PhD program in the Department of Theoretical Physics at my alma mater. However, I was not admitted. There were very few available positions at highly coveted research institutions. Only the top students got jobs at these places, the majority of the graduating class ended up in non-research type jobs.

Intercosmos was a Soviet-block space program, designed to give nations on friendly terms with the Soviet Union access to space missions. The organization was created in 1967, but it took Hungary a few years to create an organizational structure ready to participate in the program. As part of the Hungarian Intercosmos program, the Cosmic Ray Division of the Central Research Institute for Physics (KFKI) was given two positions to start *in situ* (satellite based) observations of outer space. I was offered one of these positions.

I was devastated that I could not become a theoretical particle physicist. I was also worried that I would become the first full time space physicist in Hungary with only a Master's degree and not even a single course in space physics. But, I had no choice. This was the only offer I had and I had to take it. In retrospect, it was the best thing that happened to me in my professional life. I learned my lesson and became much more open minded (adventurous?) when offered opportunities I was not ready for. Eventually I got my PhD in 1974 while working on the data analysis of the Intercosmos-3 spacecraft. I was mainly guided by my Soviet colleagues, Sergei Kuznetsov and Pyotr Vakulov, but I defended it in Hungary. My subject was the study of energetic particles below the radiation belts.

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1.2 Data Processing and Elyasberg

My first task was to work on the data analysis of the Intercosmos-3 spacecraft built by Soviet and Czechoslovak scientists to study the Earth's radiation belts (Van Allen belts). Since Hungary did not participate in the design and manufacturing of the instruments, it got the unenviable task of converting telemetry signals to useful instrument measurements. It was technically challenging and required long hours of repetitive work. Eventually, I found a rather elegant solution to the problem, which caught the eye of a very influential Soviet scientist, Pavel Efimovich Elyasberg. During World War II Elyasberg had been a Soviet artillery officer and had participated in the liberation of Hungary. He was a brilliant mathematician and quickly rose through the ranks of the officer corps; by the mid-1950s he had become a colonel in the Red Army. In the 1950s he became a leader in the Soviet ballistic missile program and eventually became responsible for the trajectory determination of ballistic missiles. When the Soviet space program started in the late 1950s he also became responsible for the orbit calculations of Earth orbiting and deep space spacecraft. Elyasberg's military career suddenly ended when the first Soviet mission to the Moon (Lunik-1) missed its target (due to an incorrectly timed upper stage burn) and flew by the Moon. Since he was in charge of the spacecraft's orbit, Elyasberg was immediately discharged from the army and assigned to the civilian space program where he was put in charge of spacecraft communications, orbit determinations and scientific computing. Though a very powerful job in civilian space exploration, it was a huge step down from his former military position.

After I presented my satellite data reconstruction method at a data processing meeting, Elyasberg took me under his wing. I was introduced to members of Elyasberg's own research group at the Space Research Institute (IKI) and Elyasberg started to spread the word at IKI that he had discovered a talented Hungarian. Elyasberg's mentorship gave me the opening I needed to break into the international scene. Elyasberg was a strong personality with quick (and usually correct) judgement and firm opinions. He could not stand fools and mediocre people. He ruthlessly humiliated those he considered unworthy of being in space research. At IKI everyone showed great respect and deference to Elyasberg and his opinions were not dismissed easily.

When an opportunity arose to be the first foreign postdoc at IKI, Elyasberg decided that it was better for my future to work with a space science group and not with his applied mathematics group. At that time I didn't understand the difference, but in retrospect it is very clear that Elyasberg was right (as usual). He recommended me to Konstantin Iosifovich Gringauz, the head of a space plasma instrumentation group at IKI. Gringauz accepted me and sometime in late November of 1975 I arrived to Moscow.

1.3 Venus and Gringauz

When I arrived to IKI to work with Konstantin Gringauz as a postdoc I was expecting to be assigned to a project investigating the Earth's space environment, since this was the area in which I had some experience. I was shocked when Gringauz asked if I wanted to work on the analysis of the Venus orbiter results. For an aspiring space scientist from Hungary, with no space program of its own, the opportunity to work on the hottest project of the times was like winning the lottery. I was pretty self-confident (should I say cocky?), and the fact that I knew nothing about Venus or planetary space environments did not even make me pause for a second. I immediately agreed, and jumped into a new adventure.

During World War II Gringauz worked on the design of small, rugged, sensitive radio transmitters and receivers for tanks. After the end of World War II he started to study radio-wave propagation in the ionosphere. In 1948 he participated in the

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launching of a V-2 rocket that carried a radio sounder to study the ionosphere. In 1956 he was assigned to design the transmitter-antenna system for what became Sputnik-1. His idea that this satellite should use a decameter transmitter was intensely debated and finally accepted, partly because the Soviets wished Sputnik-1 to be heard around the globe. On October 3, 1957, he climbed the rocket at Tyuratam to check out the Sputnik-1 antennae and transmitter. He was the last person to touch the satellite. Following the launch of the world's first artificial satellite, Sputnik-1, on October 4, 1957, the 'beep, beep' of the transmitter was heard by politicians as well as by amateurs and scientists around the globe. From 1958 onward his research concentrated on *in situ* measurements of ionized gases surrounding the Earth and the planets Venus and Mars, where he is credited with numerous scientific discoveries and "firsts." He received the Lenin Prize (the highest civilian award in the Soviet Union) in 1960 in recognition of his pioneering work in these fields.

Gringauz was a very strong personality, a true fighter. His nickname was "bulldozer" because he plowed ahead with his ideas no matter the opposition. He was highly respected but not liked by his peers and managers. At the same time he always treated his people fairly and was willing to fight for them at any time. His group was very loyal to him and he was very loyal to his group.

The Venera-9 and Venera-10 spacecraft were launched in June 1975. In late October they successfully landed on Venus and operated in the extremely hostile environment for about an hour. The main spacecraft were captured by the gravitational field of the planet and they operated for two months, providing a goldmine of information about our sister planet. These were the first orbiters around a planet other than Earth and they revolutionized our knowledge of Venus. When I arrived in IKI the Venera orbiters were at the center of space research not only in the Soviet Union but in the USA as well. Since at this time the Venus plasma environment was the main focus at IKI, I joined this effort and spent my postdoc time working on the analysis and interpretation of plasma observations by the Soviet Venus orbiters.

Konstantin Gringauz became my mentor and friend. He treated me as a son for the rest of his life. He introduced me to the US space science community and invited me to give the main Venera plasma observation talk at the 1977 IAGA conference held in Seattle. The year 1977 was an interesting time for the US space program too. The first NASA planetary orbiter, the Pioneer Venus Orbiter (or PVO), was launched in May 1978 and arrived at Venus in December 1978. It was one of the most successful planetary missions NASA ever had, orbiting Venus for nearly 14 years before it entered the atmosphere and burned up. It was well known in 1977 that NASA would launch its Venus orbiter the next year. At this time, however, the Venera-9 and -10 observations were the only data available about Venus's space environment. This made the Soviet Venus observations particularly valuable to US scientists; access to them could provide their last opportunity to make changes in the instruments and mission profile of PVO. In short, Venera-9 and -10 results were of great interest for the world's planetary research community and any presentation at a major international meeting about these results was certain to attract a lot of attention.

1.4 USA and Andy Nagy

I arrived in Seattle at the end of August 1977. The presentation went very well. I managed to stay within my allocated time and was able to answer all questions. This was not trivial; at this time I had difficulty understanding American English (and British English as well). But, Gringauz looked like a proud father, so I was pretty certain that things had gone well. During the break following my presentation, several American scientists approached me asking follow-up questions. They were very curious about the Hungarian kid (I was just 30 years old) giving one of the major Soviet talks.

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This was very, very unusual. Among these Americans was a bearded guy in his forties. To my great surprise, he started to speak in Hungarian. It turned out that his name was Andrew Nagy (everyone called him Andy). He was a professor at the University of Michigan and an Interdisciplinary Scientist of the Pioneer Venus Orbiter program. This meeting changed my life forever.

During the rest of the conference Andy and I had several more discussions, and at the end Andy asked me if I was interested in joining his Pioneer Venus research group at the University of Michigan. I responded that yes, I would be very interested, but it was not easy to get permission in Hungary to work in a Western country for an extended period of time. In the end, we agreed to stay in touch. I received the invitation in the spring of 1978 to join Andy Nagy's group as a postdoc. I would be working on the data analysis of NASA's Pioneer Venus spacecraft. This position was a big demotion – in Hungary I was already a full Research Scientist with a growing international reputation. On the other hand, this invitation offered an opportunity to break into the US science scene and to work with the next big planetary mission.

Getting permission to work in the USA, or in Western Europe, was quite difficult. While Hungary was undoubtedly the most liberal among the Soviet block countries at this time, it was still strictly controlled by the Communist Party. The joke at the time was that "Hungary was the most joyful tent in the socialist camp." Hungary was eager to show a moderate face to the outside world and the regime particularly favored high-visibility activities. In particular, Olympic sports, performing arts (especially classical music) and "hard sciences" had privileged status: these people could travel more than the general population.

It was against this backdrop that I submitted my request for a one-year leave to go to the University of Michigan and work on NASA's Pioneer Venus mission. Everybody was surprised when my request was not only approved, but I was allowed to take my family with me to the USA. We arrived to Ann Arbor in early February 1979.

During our visit I continued numerical modeling of Venus's plasma environment. Because Andy Nagy was not only an active member of the Pioneer Venus project but also a "mover and shaker," I immediately gained access to the inner circles of the US planetary science community. This was a huge opportunity, since the US science community at this time was still operating like an "old boys" network. Most decisions were made over drinks and dinner, and being a good drinking partner was almost as important as being a good scientist. By the end of my visit I was considered by the inner circles of the US planetary science community as one of the very promising young space scientists in the world.

1.5 Halley's Comet and the Importance of Public Engagement

Less than a month after my return to Hungary from the USA the Committee on Space Research (COSPAR) held its 23rd annual general assembly in Budapest. During the Cold War, COSPAR represented one of the main venues in which US and Soviet scientists could meet and exchange results and ideas. Having the COSPAR meeting in Budapest was a very big deal for Hungary, offering an opportunity to showcase Hungarian involvement in space research. For me this was a special event since I had a chance to host my Soviet and American colleagues.

It was customary for the local scientists to organize small receptions in their homes during large international meetings. My wife and I invited about 30 colleagues to our small condo for a wine and cheese reception. Among the invitees were Roald Sagdeev, the Director of IKI, some of my American friends and several Western European colleagues. One of these was Jaques Blamont, a colorful French space scientist

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who was a driving force behind the successful Franco-Soviet cooperation in space research. At the time France and the Soviet Union were negotiating French involvement in a Venus mission that would deploy long-lived scientific balloons in Venus's atmosphere to study its properties. Sagdeev and Blamont were the leaders of this planned mission. Shortly before the COSPAR meeting engineers at IKI realized that the trajectory of the planned Franco-Soviet Venus mission (called Venera) could be modified so that it would intercept Halley's comet in March 1986. On the balcony of our small apartment Sagdeev suggested to Blamont that the mission be modified and, in addition to delivering scientific balloons to Venus, it should also be instrumented to investigate the vicinity of this very famous comet. As a result of the change in mission, the French balloon payload had to be downsized and the two Venera spacecraft would no longer be placed into orbit to support them. Blamont liked the idea, but in the end the French decided to walk away from the balloon program, leaving the Soviets to build their own balloon payload instead. The French, however, became major participants in the Halley observations. In short, the Venus-Halley (VEGA) program was born on our balcony.

A few days later Sagdeev invited me to participate in the new VEGA mission. The fact that at this time I did not know much about comets was not an obstacle, since around 1980 cometary science was in its infancy. Everyone had to learn the little we knew about comets, and eventually a new area of space research emerged from the Halley missions. By luck, I was at the forefront of this emerging field and in a few years became an expert of the physics of comets.

By the fall of 1980 the international Halley armada had taken shape: the Soviets would launch two VEGA spacecraft, ESA would launch Giotto and Japan their two probes. Coordination efforts between ESA, JAXA (Japan Aerospace Exploration Agency) and IKI started in late 1980 and gradually accelerated as time went on. Even though NASA did not have a dedicated Halley mission they did not want to be left out of the international cooperation and joined the informal coordinating group.

Almost by chance, I found myself in the middle of international activities associated with the planned Halley armada. The Cold War was still going on, even though some cooperation was taking place between the superpowers. The multinational Halley coordinating group offered a good opportunity to have some behind the scenes contacts between American and Soviet scientific leaders. This, however, could not be done overtly: they needed an intermediary to organize contacts at a somewhat neutral venue. Roald Sagdeev was a major driving force of this scientific opening. He was a personal friend of Mikhail Gorbachev, who would become the leader of the Soviet Union five years later and who already had tremendous influence on Soviet policy. Sagdeev's main partner in this effort was the Science Director of the European Space Agency, Ernst Trendelenburg. He was a strong supporter of East-West cooperation and the driving force behind ESA's Giotto mission. Sagdeev and Trendelenburg had a special personal relationship based on mutual respect and shared scientific and political interests.

Sagdeev introduced me to Trendelenburg sometime in late 1980. We developed an instant affinity for each other: my cynicism and irreverence was a great fit with Trendelenburg's style. By 1981 I had become an intermediary between Sagdeev and Trendelenburg. Within a year, I was quite well known in space science circles in Eastern and Western Europe, the Soviet Union and the USA.

In late 1982 I was given a unique opportunity to give a high profile public lecture. The first successful planetary probe, Mariner-2, encountered Venus on December 14, 1962. The Planetary Society, a US nonprofit organization founded by Carl Sagan to promote the exploration of the solar system, organized a major event to commemorate the 20th anniversary of the Mariner-2 flyby and to advocate for further exploration of

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Venus. The event was attended by politicians, NASA officials and many luminaries. In the afternoon there was a symposium in one of the largest auditoriums in Washington, DC, and it was followed by a large fundraising dinner. For the symposium, Sagan scheduled three presentations: one by himself talking about the inspiration of planetary exploration, one by the famous science fiction writer Isaac Asimov who talked about his vision for humanity moving beyond Earth, and the last one by Roald Sagdeev, who was supposed to talk about the VEGA mission. Even though the VEGA project was well under way, there had never been a public lecture about it. The Soviets were notorious for keeping their space missions under wraps until they were successfully launched. Sagan was eager to break this practice and wanted Sagdeev to talk publicly about the upcoming VEGA mission.

Sagdeev very much liked Sagan's idea and agreed that a public lecture about VEGA at a high profile event would be very useful. For some reason, however, he did not want to give this lecture himself or any of his Soviet colleagues, and he suggested me instead. People at the Planetary Society had never heard of me before and they were quite surprised by this suggestion. The suggestion aroused both Sagan's and Asimov's curiosity and they gave me a royal reception. There was a press conference with the three speakers before the public lectures, and the speakers posed with the President of the National Academies, Frank Press, at dinner.

My lecture was a success. At this time I did not fully appreciate the importance of celebrities in American life, and I was unintimidated by the fact that I was following two famous speakers. I even joked that Sagan and Asimov had just given the introduction and now I would give the "real" lecture. In some respects this was true, since an attraction of the event was the introduction of the VEGA project to the American public.

After this event Carl Sagan stayed in touch with me and we occasionally got together until his untimely death. I had the highest respect for Sagan who accomplished something that very few scientists do: he made people interested and excited about basic science, especially about the exploration of the solar system.

1.6 Global MHD

Around 1990 I was already a tenured full Professor at the University of Michigan. At that time my research focus was the supersonic outflow of thermal plasma from the high latitude ionosphere, the so-called *polar wind*. My numerical simulations were based on the *finite volume* formulation of the transport equations and I used a simple (first order) *Godunov scheme* that I learned from the plasma simulation literature.

Almost by accident, I attended a small reception for a seminar speaker of Dutch origin at the home of one of my faculty colleagues. At the reception I met a recently hired professor of Aerospace Engineering who had recently arrived from the Netherlands. As we were chatting about our scientific interests, I happened to mention that I was successfully using Godunov's method and I was very pleased with the robustness and accuracy of the method. My conversation partner casually responded that this was interesting and that he was the world's leading expert in Godunov schemes. It turned out that I was talking to Bram van Leer, the father of high-order Godunov schemes. Soon afterwards we started to collaborate, and we were eventually joined by two other newly hired faculty members from the Aero department: Kenneth Powell and Philip Roe. It never occurred to me that I was out of my league in this collaboration and we immediately found common language and challenging problems to solve. By the late 1990s we succeeded in generalizing the most advanced computational methods of computational fluid dynamics (CFD) to space plasma magnetohydrodynamics (MHD) and invented a new computational approach to solving the MHD equations (Powell et

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al., 1999). This method eventually led to the development of our very successful global MHD code (BATS-R-US) and the Space Weather Modeling Framework (SWMF).

2 It is Easier to Get Forgiveness than Permission

The International Astronautical Federation (IAF) was created in 1951 with the aim of encouraging the advancement of knowledge about space and the development and application of space assets for the benefit of humanity. Its annual International Astronautical Congress (IAC) usually focused on space technology and space travel. It was a tradition for both the Soviet and the American human space flight programs to showcase their astronaut corps at the IAC. In the fall of 1983 the IAC was held in Budapest and because I was fluent both in Russian and English, I was put in charge of the special programs the IAC provided for astronauts and cosmonauts.

The first American female astronaut, Sally Ride, had completed her space flight aboard the Space Shuttle earlier in 1983. She was the star of the US delegation participating in the activities of the IAC in Budapest. The Soviets did not want to fall behind in the publicity competition. They sent the second Soviet female cosmonaut, Svetlana Savitskaya (who flew 19 years after Valentina Tereshkova became the first woman to have flown in space), to the conference. Since the IAC was about peaceful cooperation in space, the two women were supposed to make several joint appearances and they both were very much looking forward to meeting each other.

World events, however, can interfere even with the best laid plans. On September 1, 1983, just a few days before the start of the IAC, Korean Air Lines Flight 007 was shot down by a Soviet interceptor west of Sakhalin Island, in the Sea of Japan. All 269 passengers and crew aboard were killed. The aircraft was en route from New York City to Seoul via Anchorage when it flew through prohibited Soviet airspace around the time of a US reconnaissance mission. The Soviet Union claimed that the aircraft was on a spy mission and that it was a deliberate provocation by the United States to test the Soviet Union's air defenses. The political climate during the IAC was very tense and the US delegation cancelled all joint appearances of American and Soviet astronauts and cosmonauts.

Astronauts and cosmonauts are selected from very large groups of strong individuals good at overcoming obstacles. Sally Ride quickly realized that I did not care much about rules and regulations. She approached me and told me about her desire to meet with Svetlana Savitskaya in spite of the official position that there be no meeting between them. I had 24 hours to arrange a "secret" meeting because of the tight schedule of the astronauts.

I enlisted the help of the Hungarian cosmonaut, Bertalan Farkas. Farkas approached the Soviet delegation, who were actually quite pleased by the idea of a private meeting between the two women. They, however, insisted that Svetlana should not go alone but be accompanied by the commander of the mission she flew on.

The next evening there was a reception at the U.S. embassy, after which Sally Ride sneaked out of her hotel room and was picked up by me in a private car that took us to the apartment of Bertalan Farkas. Svetlana Savitskaya and her chaperon arrived about the same time. A group of about ten people, including spouses, gathered, and the two women chatted for six or seven hours, until the early morning. I translated for them and by the end was quite exhausted. Not the women. They were as lively at five in the morning as they had been at the beginning of their meeting (see Fig. 1). It is interesting to note that Sally Ride remained forever grateful to me for organizing this meeting. She regularly kept in touch with me and we occasionally got together at various meetings until her untimely death in 2012.

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Fig. 1. Top row: My mentors. Left to right are Pavel Elyasberg, Konstantin Gringauz and Andrew Nagy. Middle row: Guests of honor at the Planetary Society's dinner to celebrate the 20th anniversary of the Mariner-2 encounter with Venus. Left to right are Frank Press, Tamas Gombosi, Isaac Asimov and Carl Sagan. Bottom row: Left to right are Sally Ride, Bertalan Farkas, Svetlana Savitskaya and Tamas Gombosi and Sally Ride and Svetlana Savitskaya.

I am sure that the leadership of the US delegation was informed about this "secret" meeting. They, however, most likely decided to look the other way. This

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event reinforced my instinct that you should not ask a question if you dont want to hear no for the answer.

3 Stick to your Guns

I love westerns. They are uncomplicated stories that almost always have predictable endings. I use this analogy to summarize one of the most important lessons of my professional life: Respect and never underestimate your competitors, and, if you are convinced that you are right, stick to your guns.

3.1 BATS-R-US

In the early 1990s the federal government initiated the national High Performance Computing and Communications (HPCC) program to adapt the Nation's Research and Development effort to the parallel computing revolution. Both NASA and the NSF created HPCC Grand Challenges programs to transition large legacy simulation codes from vector machines to massively parallel architectures.

Shortly after my collaboration with the world leading CFD faculty (van Leer, Roe and Powell) started, we responded to both the NSF and NASA HPCC initiatives and proposed to develop a massively parallel space plasma simulation code with adaptive mesh refinement (AMR) and modern shock capturing methods (high-order Godunov schemes). The proposals were declined with devastating reviews. The most memorable comment was "why does everyone want to jump on the Godunov scheme bandwagon." To which Phil Roe responded "because we built it, dammit!"

Eventually we were able to get some seed funding from a joint NSF-NASA-AFOSR grant and developed a prototype implementation. Three years later the second round of NASA's HPCC Grand Challenges competition was opened and we proposed to develop a 3D heliosphere model with Godunov schemes and AMR. We called the proposed code Block-Adaptive Tree Solar-wind Roe-type Upwind Scheme (BATS-R-US). The proposal was funded, but some time later I was told by a NASA insider that the selection was controversial, because some people wanted to save us from the "embarrassment of failure."

By the early 2000s BATS-R-US was ready for prime time (T. I. Gombosi et al., 2000).

3.2 The Space Weather Modeling Framework

Computational frameworks have emerged as a response to the increasing complexity of models and computing systems. A comprehensive space environment model encompasses multiple, interacting physics domains. The software representing these domains is often developed by different groups, and must be coupled together to form composite applications. The applications are computationally intensive and the execution time directly affects the ability of the models to deliver useful results. Frameworks are intended to promote ease of model development, integration, extension, modification, and use.

The need for domain model coupling arouse in heliophysics in the early 2000s. Two efforts were selected for implementation, one by NASA and one by the NSF. NSF selected a Science and Technology Center called "Center for Integrated Space-weather Modeling" (CISM), while the NASA Computational Technologies program selected our "Space Weather Modeling Framework" (SWMF). CISM was a large consortium of research groups led by Boston University, while the SWMF was a single institution effort by the University of Michigan.

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Due to its geographically distributed nature, CISM created a distributed system that coupled legacy models as separate executables (Goodrich et al., 2004), and produced a Sun-to-Earth model chain that allowed scientific investigations and post-event analysis (Hughes & Hudson, 2004). However, this approach turned out to be cumbersome and after the end of CISM funding their distributed framework faded away.

From its inception, the SWMF (Toth et al., 2005; T. Gombosi et al., 2021) was developed with high parallel performance and portability in mind. It provides the software environment and tools to couple the various models with each other. The SWMF provides a capability to simulate the space-weather environment from the solar photosphere to the Earth's upper atmosphere and/or the outer heliosphere. Currently there are sixteen physics domains in the SWMF, but in an actual simulation one can use any meaningful subset of the components.

From the early days there was a (mostly) civilized competition between the CISM and SWMF teams. There were occasional arguments at various meetings and the two groups did not shy away from criticizing each other. Over time the single executable approach of the SWMF turned out to be more attractive than the distributed approach adapted by the CISM team. However, the domain models used by the two teams are still in use and are being further developed. It turns out that it is very useful to have competing models for the same domain, since no model is perfect and different models have different strengths and weaknesses. In addition, multiple models enable better ensemble forecasting, an approach that is increasingly important in space weather research.

4 Hire People who are Better than You

Working with talented people could be challenging. A colleague of mine from an East Coast elite university once told me that he stopped working with graduate students, because "if they are mediocre, they need too much investment, and if they are really good they want all the credit." I, however, learned my approach from my mentor, Andy Nagy, who would proudly tell anyone who would listen (or would not) that he only had five postdocs: Ralph Cicerone, Bill Chameides, Rich Stolarski, Tom Cravens and Tamas Gombosi. They all became AGU Fellows and Cicerone and Chameides were elected to the National Academies. Andy's approach was to set the general direction (defined by the grant that paid the salaries) and let the postdocs lead. Andy's approach worked extremely well in all cases. A fundamentally different approach is described in the whimsical Parkinson's laws (originally published in the 1930s, the latest edition is Parkinson, 2019). In the first Chapter, called "The rising pyramid," (Parkinson, 2019) describes this approach as "an official wants to multiply subordinates, not rivals." I have seen many scientists to use this approach when they had a chance to expand their "empire." This approach works for a "Dear Leader," but talented people run away from the group as soon as they can. Sooner, rather than later, the group becomes a one person show, and eventually, it fades away when the leader retires or moves on.

Once I had a chance to hire younger people I wholeheartedly adapted Andy Nagy's approach. This adaptation was based on my own transition from "I can do everything better" to the recognition that "I cannot do everything myself." The question is how much control are you willing to give up in return for better and faster results. Mediocre people will attack the problem your way and will follow your suggestions. Outstanding people, on the other hand, will do it their way and pretty much ignore your suggestions. It takes some humility and a lot of self-confidence to let them do this.

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Around the turn of the millennium I was successful and won two large awards. One was a Department of Defense (DoD) Multidisciplinary University Research Initiative (MURI) grant to address "Space Weather Effects on DoD Operations," and the other a NASA HPCC grant for "Increasing Interoperability and Performance of Grand Challenge Applications in the Earth, Space, Life, and Microgravity Sciences." The aim of the two projects was the development of the SWMF (NASA HPCC) and its application to the Sun-Earth space weather system (DoD MURI). The two projects provided enough to hire about 10 students and early career scientists. While most of the people I hired became very successful, I want to talk about the two people who became instrumental in the success of the Michigan space weather modeling enterprise, Igor Sokolov and Gábor Tóth.

Igor graduated from the Moscow Institute of Physics and Technology (FizTech) in Moscow under the guidance of Nobel Prize winner Vitalii Ginzburg. He earned his PhD in fusion plasma physics. He is undoubtedly one of the best physicists I know, on par with Galeev, Sagdeev and Shapiro. In Russia he was mentored by two Nobel Prize winning physicists, and at Michigan he also worked with a third one (laser physicist Gérard Morou). Gábor graduated from the best STEM high school in Hungary (Fazekas Mihály Gimnazium). By the time he finished high school he had a first-place finish in the International Mathematical Olympiad and a second-place finish in the International Physics Olympiad. After finishing his Masters Degree in Physics in Budapest, he joined Princeton University for his PhD (Oxford University came across too late with financial support). As a postdoc in the Netherlands he single-handedly designed and developed the Versatile Advective Code (VAC, jokingly also referred to as the Very Ambitious Code), a highly flexible modern MHD code that is still widely used. Igor and Gábor are among the very best computational physicist I have ever met, on par with Roe and van Leer.

Great scientists usually have great egos, and Igor and Gábor are no exceptions. They are like wild west gunslingers in feel-good westerns: they only shoot at worthy adversaries who are in their league. This means that they treat us ordinary scientists with benign contempt, not even expecting us to fully comprehend their ideas and solutions to difficult problems. With each other, however, they have no mercy and they always have time and energy to point out the smallest mistake the other makes. Over the two decades they worked on the same problems they were constantly inspiring and challenging each other. In my view this gunslinger mentality has been the main driver behind our successes in the 21st Century.

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