

## Written in the Stars

Sara Seager

### Awakening

**F**IRST REALLY *saw* the stars when I was 10 years old and on my first camping trip, in Ontario. I remember awakening late one night, stepping outside the tent, and looking up. I was completely stunned by what I saw. Stars—millions of them it seemed—filled the entire sky. I had never imagined that there was such a vast expanse beyond Earth.

Some critical aspects of my childhood clearly contributed to my later success as an astrobiologist. My father taught me how to think big and how to succeed. He introduced me to many outlandish ideas at a very early age, which helped me learn to take seemingly crazy ideas seriously, even my own. Perhaps unintentionally, my parents' unorthodox child-raising methods also gave me an independent streak and a natural skepticism of authority, traits that led me to challenge ideas I didn't understand or agree with. I wasn't a conventionally good student (I had trouble learning in a classroom setting), but going to school was actually productive, as the many hours I spent daydreaming there laid down the pathways I now random-walk as I conceive and evaluate new ideas.

When, at age 16, I learned one could pursue astronomy as a profession, my guide star was set. I rushed home to tell my father, who immediately harshly lectured that in his view my only option was to seek a career where I could actually get a job and support myself. But it was too late; the independence he taught me had backfired—on him!

### Condensation

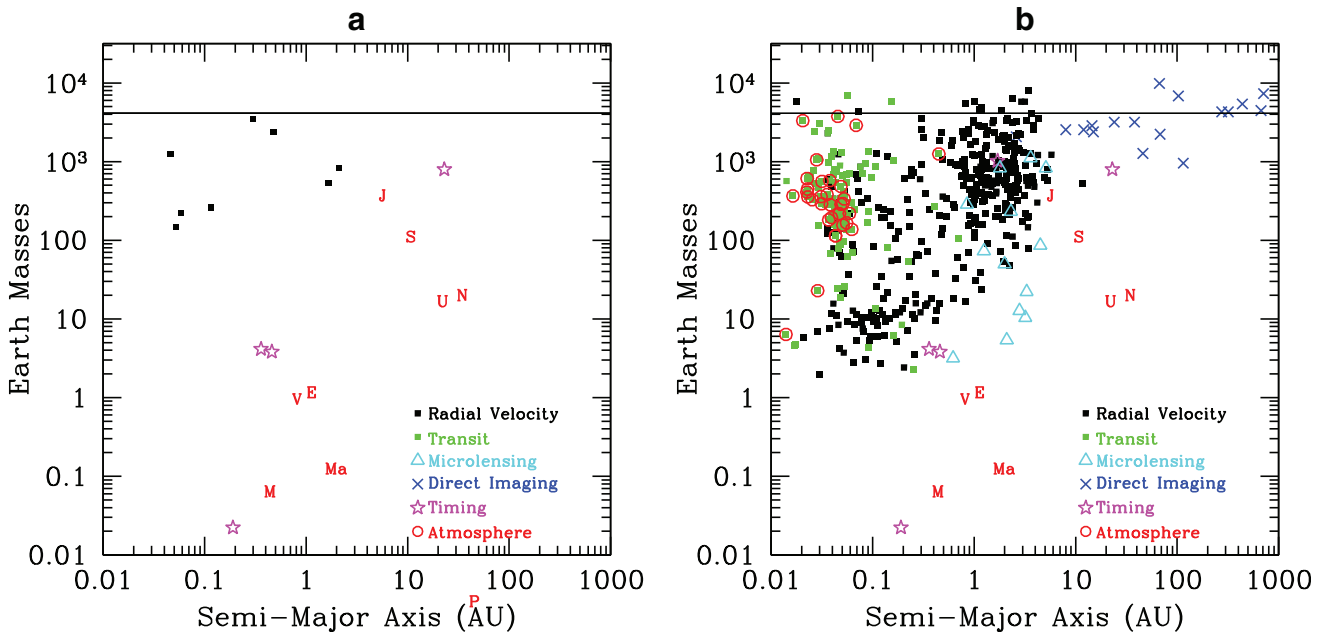
While I was a graduate student at Harvard, in 1995, the first planet orbiting a Sun-like star was announced (Mayor and Queloz, 1995). Seven times closer to its star than Mercury is to our Sun, the Jupiter-mass planet 51 Peg b shattered the paradigm of planet formation. By the summer of 1996—just when I was looking for a Ph.D. thesis topic—a handful of these so-called “hot Jupiters” had been discovered (Fig. 1). My research supervisor, Dimitar Sasselov (now a Harvard professor), encouraged me to take charge of an interesting project he did not have time for: modeling hot-Jupiter atmospheres. With orbital separations of about 0.05 AU or less, these planets are broiled by their stars to temperatures of 1000–2000 K or higher; understanding the details of this extreme heating was a new problem in planetary science. It is risky to give a student such a frontier-breaking project.

Computer modeling of exoplanetary atmospheres was considered risky because at the time most astronomers were skeptical of the new planets, which were products of indirect detection techniques. In theory, the planetary signatures could have been illusions caused by phenomena like stellar variability. It seemed as though every astronomer I told about my thesis topic advised me against it. Even those who believed that exoplanets were real assured me that exoplanetary atmospheres would never be observable. Always a risk-taker—and in no way committed to a career in science—I forged ahead. My Ph.D. thesis was one of the first on exoplanets, and ultimately provided the first predictive descriptions of hot-Jupiters under strong stellar irradiation (Seager and Sasselov, 1998).

### Migrating Planetesimal

By September 1999, I was a newly minted Harvard Ph.D. headed for a postdoctoral fellowship at the Institute for Advanced Study (IAS) in Princeton. Surrounded by cosmologists who had never met an exoplanet researcher before, I was persistently asked, “What’s the next big thing in exoplanets?” The answer was obvious: the discovery of a transiting exoplanet. It was crystal clear to me that any day a transiting planet could be discovered—and that such a discovery would change everything. A planet going in front and behind its star as seen from Earth would open up possibilities for studying the planet’s atmosphere, as well as many other physical properties. Starlight would filter through the transiting planet’s atmosphere, imprinting the spectral signatures of atmospheric gases in its passage. The resulting signal promised to be large enough for measurement with current instruments, so I drafted a paper on the idea of observing exoplanetary atmospheric transmission spectra (Seager and Sasselov, 2000).

All the previous summer, as I was finishing my doctorate at Harvard, I had lobbied astronomer Dave Latham to scrutinize his handful of hot-Jupiter planet candidates for photometric fluctuations that could signify a transit. With each one of his several planet candidates having an estimated 10% probability to transit, I thought Dave might have the first transiting planet in hand without knowing it. Later, at Princeton, I even made a weak attempt to find a telescope to do the follow-up observations myself. Alas, Princeton had no appropriate instrumentation and very few nights per year with suitable observing conditions. Little did I know that because of my persistence, Dave Latham had handed a



**FIG. 1.** Exoplanet census then and now. The solid line is the conventional upper mass limit for the definition of a planet (13 Jupiter masses). (a) Known planets at the end of 1996, the year the author started working in the field of exoplanets. Pulsar planets and a handful of radial velocity–discovered exoplanets were known. (b) Known planets 15 years later (at the time this article was written; September 2011). Several different exoplanet techniques have succeeded in discovery, each with its own regime of mass vs. semimajor axis parameter space. The red circles denote transiting exoplanets with published atmosphere measurements either during transit or secondary eclipse. It is fair to say that the unpopulated part of the diagram is due to contemporary technological limitations. Data taken from <http://exoplanet.eu/>. Color images available online at [www.liebertonline.com/ast](http://www.liebertonline.com/ast)

prime hot-Jupiter planet candidate off to another Harvard Ph.D. student, Dave Charbonneau, who at the end of 1999, along with an independent group, discovered the first transiting exoplanet, HD 209458 b (Charbonneau *et al.*, 2000; Henry *et al.*, 2000).

My theoretical work set the stage for the first observation of an exoplanet’s atmosphere, emphasizing sodium as a detectable gas at visible wavelengths. An observing group that went looking for sodium’s signature in the radiation from the planet soon found it with the Hubble Space Telescope (Charbonneau *et al.*, 2002).

After the sodium-prediction paper, I subsequently developed theoretical models and applied them to descriptive predictions for many additional advanced exoplanet observations (exoplanetary scattered light and polarization signatures, Seager *et al.*, 2000; an estimate of the planet’s rotation rate from a projected oblateness measurement, Seager and Hui, 2002; atmospheric refraction signatures, Hui and Seager, 2002; atmospheric photochemistry, Liang *et al.*, 2003, 2004; and characterizing Earth-like exoplanets by their diurnal photometric variability, Ford *et al.*, 2001). At the time it felt like no one else in astronomy and astrophysics really cared; even experts thought such observations were too far off in the future to warrant much attention. Fortunately, at the IAS, our legendary astrophysics leader, John Bahcall, had a visionary and bold philosophy. He believed as long as the underlying physics is sound and well developed, and the phenomenon conceivably detectable within one’s lifetime, an astrophysics topic is worth pursuing. I am forever grateful for this extraordinary attitude.

## Collision

While at the IAS, I tried and failed on my first attempt at observational astronomy. I befriended Gabriela Mallen-Ornelas, a Princeton Chilean postdoctoral fellow. Her Chilean affiliation entitled her to propose to use the 10% of telescope time allocated to Chilean institutions on the large, sophisticated telescopes the country hosts. I can clearly recall us in front of her computer, browsing websites for the telescopes available to us and brainstorming about what exoplanet projects we might pursue. We ended up designing a transiting exoplanet survey from first principles (Mallen-Ornelas *et al.*, 2003) and coding most of the computer algorithms from scratch. In those days, there were few established techniques for such a survey. I remember John constantly checking up with me to see how the code development was going and how clear each observing night in Chile had been.

Not only did bad weather in the Chilean winter nearly kill the project, but out of the several key steps in the experiment we had made one particularly bad choice. Nonetheless, we thought we had discovered the first transiting exoplanet with the transit survey technique—until we realized it was a pernicious, pervasive false positive we and others later nicknamed a “blend” (Mallen-Ornelas *et al.*, 2003). In his typical sardonic tone John Bahcall simply said, “At least you didn’t publish it.” I often wondered whether he really thought two postdocs could succeed in 6 months with such an ambitious project, or if his enthusiasm was designed to give me positive feedback simply for embarking on new ideas. Overall our transit search was a failure and faded

away into the annals of dead exoplanet experiments. Nonetheless, this failure was the single most important career event for my future success. It taught me to reinforce my impulsive optimism with precautionary steps like carefully selecting collaborators and double-checking every single step in any experiment for potential errors or oversights.

One very happy memory (and highly cited paper) remains from the failed observing effort (Seager and Mallen-Ornelas, 2003). A planet-transit light curve can be described by a fixed set of equations, and I had realized there were just as many unknowns as there were equations for physical parameters of interest. This seemingly prosaic brainstorm led Gabriela and me to some major insights and applications. One hot summer night in 2001, we were working late, feverishly pitting our minds against the equations. Around midnight, we suddenly and simultaneously had a major “Eureka!” moment, shouting in unison “Density!” Then we burst out laughing. We had figured out that a star’s density could uniquely be determined from a transiting planet’s light curve alone. It remains one of my favorite papers ever, and its contributions are now frequently used in the candidate-vetting process for transiting planets. That a theoretical paper is the most significant paper to come out of our observational project has a sweet irony.

It so happened that just at the “Eureka!” moment, John (also working late) walked by and found Gabriela and me laughing hysterically. The next Monday he called me into his office and told me this was the first time he recognized a major disadvantage for female scientists. He said I would never be able to bond with my male colleagues in the same way as I could with Gabriela, thus missing out on many professional connections and opportunities. I have thought of this insight over the years but haven’t found a way around it.

During my time at the IAS (1999–2002), I interviewed for faculty positions at top universities around the country. Several rejected me because of my research interests. Many professors worried not only that exoplanetary atmospheres would never move beyond theory but also that the entire field of exoplanets would soon degrade into “stamp collecting,” where discoveries would be strictly limited to planetary minimum masses and orbits. Few believed that there would ever be enough transiting planets for useful science. Considering Kepler’s 1200 transiting planet candidates (Borucki *et al.*, 2011), the more than 150 transiting exoplanets that have been detected in ground-based surveys, and dozens of exoplanetary atmosphere measurements, these faculty search committees, in retrospect, seem to have been rather short-sighted.

### Consolidation

Meanwhile the Carnegie Institution of Washington, Department of Terrestrial Magnetism, was one of the first places to recognize the potential of the exoplanet field of research. Working on the frontier, and not tied down by the mammoth bureaucracy inherent in universities and government labs, Carnegie hired a group of exoplanet researchers, which I joined in the summer of 2002. I benefited tremendously from the ground-breaking attitude at Carnegie and being in a geophysics-intensive environment. Aided by high-pressure geophysics studies on equations of state, I moved into research on exoplanetary interiors (Seager *et al.*, 2007).

Around this same time, I was heavily involved in the Terrestrial Planet Finder (TPF) science and technology teams, part of NASA’s effort to plan a telescope capable of directly imaging small, Earth-like exoplanets. For a time, our work was generously funded, to the tune of tens of millions of dollars per year, and the team met frequently. Many of us became friends.

My favorite story from the TPF years involves the meeting where a new idea for suppressing starlight, the external occulter, was first introduced to the community. A star can outshine a small accompanying planet by a factor of millions—even billions—so imaging a planet requires somehow nulling all that excess starlight. About 45 astronomers and engineers were at the meeting and in various states of attentiveness. But when the presentation on the external occulter started, the room became completely silent. You could have heard a pin drop. People listened in shock and disbelief at the audacity of what was described: The sunflower-shaped occulter would be about half a football field in diameter and would have to be folded up like a piece of origami to fit into a rocket. It would be deployed more than 30,000 miles from its telescope and would need to be tightly aligned with the very distant telescope. Only then would its deep shadow fall precisely on the telescope’s optics, blocking a target star’s light and allowing accompanying terrestrial planets to be imaged. Though less technologically mature than other light-suppression concepts for TPF, the external occulter concept is flexible enough to even be used with other more generalist observatories, such as NASA’s James Webb Space Telescope (JWST), slated to launch later this decade. This operational flexibility may prove vital, considering that TPF’s high complexity and cost resulted in its indefinite deferral in 2006 (for summaries of TPF concepts see the following: internal coronagraph, Trauger and Traub, 2007; external coronagraph, Cash 2006; infrared nulling interferometer, Peters *et al.*, 2010).

The Carnegie Institution of Washington was an original member of the NASA Astrobiology Institute, and while at Carnegie I recall skeptical discussions in the community on the fledging field of astrobiology. The main issue of contention was whether astrobiology was “business as usual,” meaning individuals continuing their own research under the umbrella of astrobiology funding, or whether astrobiology was really sparking new cross-disciplinary research. In my case it really did spark new research. Lengthy discussions with George Cody and a then-Carnegie Postdoctoral Fellow, Matt Schrenk, led to collaborative work on trying to understand how life in extreme environments, like deep-sea hydrothermal vents or hypersaline lakes, might reveal its presence via gaseous metabolic byproducts released into a planet’s atmosphere. Consideration of such “biosignature gases” helped sow the seeds for some of my present-day research (for the first of a series of articles, see Seager, Schrenk, and Bains, 2012, in this issue of *Astrobiology*).

In the mid-2000s, the Massachusetts Institute of Technology (MIT) and other universities realized the exoplanet theme in astrobiology was not only a solid field of research, it was accelerating. Around 2006, the MIT Department of Earth, Atmospheric, and Planetary Sciences was looking for an exoplanet researcher who would both lead and catalyze exoplanet efforts at the university. The position was eventually

offered to me. I remember my late father pushing me to accept this job, “Opportunity is rare. You would be a fool not to take it.” When I finally decided to accept the MIT offer I told him, excitedly, this was the best I could do at my age, with tenure at age 35. His eyes flashed and, frowning, he responded in the toughest voice he ever used on me, “I never want to hear you say that anything is the ‘best’ you can do. I never want you to be limited by your own negative thinking.” At the time, he was battling cancer; and while it went unsaid, we both suspected this was one of our last conversations. He decided to finish by pushing me to keep thinking big.

### Biosignature Gases and Sibling Planets

I took the faculty job at MIT and began carefully thinking big about where to go next in research. I could keep on with what I was doing, but the foundation was now laid for researchers young and old to study exoplanetary atmospheres and interiors, and that part of the field perhaps didn’t really need me anymore. In fact, the whole attitude toward exoplanets had slowly changed from a simple “too futuristic” to a more dismissive “any crazy, ill-conceived idea could get published.” I wanted to transcend the poor quality of these futuristic papers and the petty fighting prevalent among many researchers (Fig. 2).

I chose two extremely “futuristic” projects to invest in. The first is exoplanetary biosignature gases, specifically whether or not there are prominent biosignature gases different from the ones on Earth. The motivation here is to go beyond all the tediously terracentric biosignature gas work and try to develop a new, exhaustively thorough quantitative framework based on general chemical energy sources and sinks. The real motivation, however, is that the number of bright stars amenable to future atmospheric observations of any accompanying terrestrial planets is very small. Given the diversity

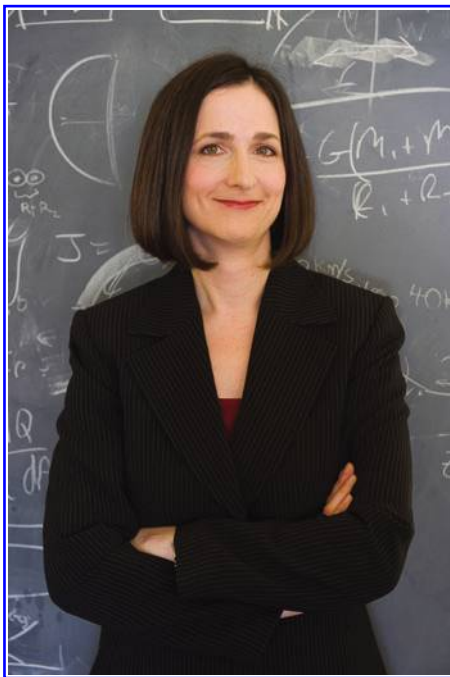


FIG. 2. The author. Photo credit: Justin Knight. Color images available online at [www.liebertonline.com/ast](http://www.liebertonline.com/ast)

of planetary environments, the chances don’t seem to be good that we will find an Earth twin orbiting one of these stars. So if we are to succeed in identifying signs of life, even on a sliding probabilistic scale, we must broaden our perspective on viable biosignature gases.

The second project comes out of the idea to find transiting Earth-sized planets around the brightest Sun-like stars in the sky. I had carried this thought in my head for nearly a decade after I heard Ron Gilliland outline the idea that, if a transiting planet around a Sun-like star was a little bigger than Earth and a bit closer in its orbit, its atmosphere might be detectable via transmission spectroscopy with a future large aperture space-based telescope such as JWST. When NASA’s TPF mission was shelved in 2006, I thought harder about these so-called “super-Earth” planets. When I learned about tiny, off-the-shelf “CubeSats” in 2007, I jumped on my idea of putting a tiny telescope with high pointing precision inside. Such an assembly could find promising transiting super-Earths about Sun-like stars, and at a very low cost.

I knew these were the right projects when colleagues told me the biosignature gases work was too futuristic and my first two related papers were rejected. The ExoplanetSat concept was usually brushed off when I described the required pointing precision—a few arc seconds—two orders of magnitude more precise than conventionally believed possible for such tiny spacecraft. Eventually one group of like-minded engineers at Draper Lab joined my MIT effort, and now we have a potential launch date for the first ExoplanetSat prototype in 2013. If successful, we will follow this with a fleet of ExoplanetSats with different telescope apertures to search the brightest Sun-like stars for transiting Earth-sized exoplanets. We aim to start a new paradigm for space science missions by the graduated growth of a constellation of satellites: small spacecraft can be used as the key detection element within a scalable and realizable satellite constellation to achieve meaningful science.

To mitigate the risk of my futuristic efforts, I have my large group of students and postdocs working on exoplanet data, interpreting atmospheric signatures as well as interior compositions from exoplanet mass-radius measurements. We’ve investigated the first comprehensive models of super-Earth atmospheres (Miller-Ricci *et al.*, 2009) and the first interior models for the first transiting super-Earth/mini-Neptune (Rogers and Seager, 2010). We are leading the way to a new framework for exoplanetary atmosphere and interior interpretation; up until now researchers constructed a few “forward” models to try to match the data as a basis for interpretation. For exoplanetary atmospheres we developed the “million model approach” to explore the entire valid parameter space (Madhusudhan and Seager, 2009). We are extending this philosophy to other areas of exoplanet characterization. My new focus also includes training the students to excel at the crossroads of planetary science and engineering.

### Outlook

To commemorate my 40<sup>th</sup> birthday I convened a symposium at MIT in May 2011 titled, “The Next 40 Years of Exoplanets”<sup>1</sup>. Here I pressed my friends and colleagues to

<sup>1</sup><http://seagerexoplanets.mit.edu/next40years.htm>



**FIG. 3.** Describing a vision for the next 40 years of exoplanets, with renowned planet hunter Geoff Marcy, concurring, in the foreground. Photo credit: Justin Knight. Color images available online at [www.liebertonline.com/ast](http://www.liebertonline.com/ast)

each deliver a personal, provocative message about the future of exoplanets. While the field of exoplanet research is maturing, most researchers believe there is still huge unrealized potential. However, some aspects of the field of exoplanet research are becoming saturated, and the Holy Grail of finding and identifying an Earth analog via a TPF-type mission is looking more and more distant. Out of the many incisive and visionary statements contributed at the MIT symposium, two remain central in my mind. The first is that as a discipline created by rogue pioneers, the field of exoplanets lacks social unity. This has created problems in the present political landscape, particularly in advocating which of the inevitably large and expensive space missions should be pushed forward. The second memorable message is, in contrast, about an emerging, unified vision. In exoplanets we plan to map the nearby stars (with a planet census) to leave a legacy for people 40 years from now and beyond. Hundreds or a thousand years from now, people embarking on interstellar travel will look back and remember us as the society that first found the Earth-like worlds (Fig. 3).

Because of the huge diversity of exoplanets (both for planetary systems and for individual exoplanet mass, size, atmosphere, and orbital parameters) I like to end each day with the thought "In exoplanets anything is possible within the laws of physics and chemistry." I also often think of the success stories of scientists working against the trend, and

often in obscurity, and of John Bahcall's related tenet of taking bold new ideas, supporting them with solid physics, and making them happen. Astrobiology is one of those rare fields of science with the opportunity for big advances and where, indeed, *almost* anything is possible.

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#### Abbreviations

IAS, Institute for Advanced Study; JWST, James Webb Space Telescope; MIT, Massachusetts Institute of Technology; TPF, Terrestrial Planet Finder.

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Address correspondence to:

Sara Seager  
 Massachusetts Institute of Technology  
 Professor of Planetary Science  
 Professor of Physics  
 Class of 1941 Professor  
 54-1718 77 Massachusetts Ave.  
 Cambridge, MA 02139  
 USA

E-mail: seager@mit.edu