In the pink: The seeds of the corn-cockle (*Agrostemma githago*, flower shown here) have no obvious adaptation to a particular mode of dispersal but show an intricate surface pattern.

so that the creativity of millions of years of plant evolution is not irreplaceably lost during our own short tenure on this planet."

Given the urgency of current environmental concerns, engaging the public in the conservation of plants has to be a priority for any scientist interested in the variety of plant life, he says. It is therefore central to the work of the Millennium Seed Bank and the Royal Botanic Gardens Kew. "The creative partnership between artist and scientist in this book stimulates us to want to know more about seeds and how they work," says Crane. "The creative partnership between artist and scientist in this both stimulates us to want to know more about seeds and how they work," he says. "I hope that it encourages us to see the glory of Nature and to do what we can to ensure its survival in the future", he says.

Seeds: Time Capsules of Life by Rob Kesseler and Wolfgang Stuppy. Published by Papadakis in collaboration with the Royal Botanic Gardens Kew. ISBN: 1 901092 66 6.

Q & A

Eve Marder

Eve Marder is the Victor and Gwendolyn Professor of Neuroscience and Chair of the Biology Department at Brandeis University. She has worked on the modulation and dynamics of small neuronal networks and mechanisms of neuronal homeostasis. She is Chief Editor of the Journal of Neurophysiology *and the Incoming President-Elect of the Society for Neuroscience.*

What was it like being a woman in science at the beginning of your career? I entered graduate school at a transition point for American science. Prior to 1969, the year I started graduate school, it was commonly known that many graduate programs had quotas for women students, and almost all of them offered admission only to a few each year. Just about the time I was applying to graduate school the draft lottery was instituted for the Vietnam War. With the lottery, many draft deferrals for graduate school disappeared, and men of my generation could no longer attend PhD programs to stay out of the army. (I believe that MD/PhD programs still were draft-deferable, and this greatly increased their desirability among men interested in research.)

The result of the change in draft laws was that the pool of men available for graduate study dropped, and programs found themselves offering more places to women. My class, consisting of 13 women out of 30, created a hubbub when we arrived at UCSD in the fall of 1969 (the year before there were two women out of 30). The furor lasted a few weeks, and by the end of our first year, both male and female students had settled into labs, and graduate admissions at UCSD (and virtually all other programs around the country) went from being almost exclusively male to approximately 50:50 within a period of 2–3 years.

If you had asked me at the time, whether I had ever been discriminated against for being female, I would have answered no. I had a PhD thesis advisor who provided everything I needed to do my work, including the freedom to do what I wished. Other faculty members, both at UCSD and elsewhere gave me substantive technical advice and encouragement. That said, many years later, when I look back at those years with the optics of present politics, I recall numerous occurrences that today would be considered totally unacceptable, but at the time didn't faze me. For example, one of my first year rotation advisors made a habit of coming over to where I was working and telling me I should quit graduate school and get married and have six babies. After the 20th or so lecture of this type, I told him I was spending the rest of my rotation in the library, because it was clear he didn't want me in his laboratory. But it never crossed my mind to take his comments seriously; I just thought he was a jerk.

Many of us who started during those years have similar stories, and we maneuvered around the 'jerks'. I was lucky, because UCSD was then a young institution, and the faculty who were attracted by the challenge of building a new institution were open to change. It has now been more than 35 years since I started graduate school in a gender-balanced class and I have lost patience with the persistence of underrepresentation of women. I do not find meeting rosters with all-male speakers acceptable, regardless of how well-meaning and clueless the organizers might be. I don't like doing so, but I force myself to make a fuss with meeting organizers, reminding them that all-male speaker lists send a terrible message to the young women and men in the audience.

What drew you to seek collaborations with theorists and to the field of computational neuroscience? As a graduate student working on small circuits, I noticed that I couldn't think about what was happening to the individual neurons in the circuit except in groups of two or three cells at a time. Then I realized that when others talked about circuits they too would deal with subcircuits of two or three cells, as if we all had to chunk their activity in order to think about them. Years later, I realized more formally that most of us cannot think about the interactions of more than a few non-linear processes at the same time, and that if one wants to understand dynamics that depend on more than one or two nonlinear processes, it is useful to employ computational and theoretical measures to understand how multiple nonlinear elements interact to produce system outcomes. Many biologists have developed what I call ‘word models’ that describe how they think things work. One of the best uses of theory is to translate those word models into more formal mathematical and computational languages so that they can be explicitly explored. Sometimes it becomes clear that things cannot work as described in the word models, and more often the process of trying to formalize a word model reveals that some essential piece of data or analysis is missing.

What advice would you give to theorists looking for new collaborations with experimentalists and to experimentalists looking for theory colleagues? I would advise theorists coming from physics and math to ask neuroscientists and other biologists what really puzzles them. Beginning collaborations sometimes founder because theorists entering biology may not be attracted by the same questions that interest biologists. I would advise biologists to insist that their nascent collaborators be able to describe in words exactly how their proposed models work, and what they can learn from them.

What has governed your choice of scientific problem? I realized early in my career that some scientists are drawn to study the consensus problems of the era, while others seem to search for their own vision. There are tremendous advantages of working on a consensus problem: a large number of people care about the results of the experiments, and one doesn’t have to justify one’s scientific existence. I realized as a graduate student that I didn’t want to do experiments that would be done by others if I didn’t do them, but I wanted to contribute through my own voice and vision. There are big risks associated with avoiding consensus problems. Most notably, without a large community working on the same or similar problems, one can end up doing a lot of drudge work that can make it difficult to get to ground-breaking experiments. And then, even if you find something interesting, it may be more difficult to convince others that they should pay attention. But, if one is lucky, one can point to new ways of thinking about a scientific problem, and have the satisfaction that you have reshaped the way we think about something, not just won a race to be first.

You have worked with a ‘simple’ invertebrate system for your entire scientific career: have there been any special challenges associated with that? When I started in neuroscience, there were a great many preparations being studied, among them invertebrates and lower vertebrates. These were selected because they were ideally suited to addressing a particular experimental question. Thirty years ago, it was difficult to do intracellular recording in the vertebrate central nervous system, so there were many problems in cellular electrophysiology for which invertebrate preparations were required. Today, for many problems in neuroscience, it makes more sense to study them in vertebrate cell culture or slice preparations than it does in invertebrate nervous systems, and it is not an accident that the community using invertebrate systems — outside of *Caenorhabditis elegans* and *Drosophila* — has become a smaller fraction of the neuroscience community over the years (although some might argue it still contributes conceptually far more than would be expected on numbers alone). Many times during my career, well-meaning colleagues have urged me to move to a vertebrate preparation. My answer is that as long as I can formulate an important biological question that is better addressed with crabs and lobsters than with rats, I will stick with crabs and lobsters.

What advice did you get early in your career that was particularly helpful or important? I have been extremely fortunate in having had numerous friends and mentors, including my graduate and postdoctoral supervisors who were supportive and helpful on a continuous basis over many years. Sometimes, however, particular things someone says just hit a chord. For me there were two such comments, one made by Eric Kandel, which I am sure he has long forgotten, and one made by my colleague Jeff Hall, which he may remember because he has never forgotten anything.

During my years as a postdoc I was standing in the Society for Neuroscience poster session with a group of people, and everyone drifted away leaving me standing alone with Kandel. He looked around, and then said to me, “Do you know what the secret to success is?” I of course said no. He then said, “Just keep working”. This is probably the most important advice anyone could give to a scientist of any age.

During my first month as an assistant professor, Jeff Hall walked into my then empty lab. He told me a story about a beginning faculty member who had just sent off the first paper consisting entirely of work from her lab. I understood from Jeff’s not totally straightforward account that the first paper from a new lab is important, not for what it says, but by virtue of its existence, as it demonstrates that the lab is ‘up and running’. To this day, I tell all beginning assistant professors that the first paper from their lab must be respectable but can be mundane. Many beginning faculty members want their first papers to be 'home runs'. But, 'home runs' come after the lab has momentum, and the way to gain that momentum is to start producing as quickly as possible.

Do you think there is an increased trend towards translational research?
Throughout the history of modern science there have been times when political or societal needs have set the agenda for scientific research. For example, World War II accelerated research in a number of areas related to communications and weapon systems. So the present push towards finding cures for the major illnesses that plague humankind today is understandable. That said, I find the extent of the present translational rhetoric a bit troubling for two reasons. First, there is a phenomenal amount of human suffering that today could be alleviated with the knowledge, medicines and technology that we already have, if the political will were there. Second, I believe that much of what drives discovery by scientists is sheer curiosity, and the desire to solve puzzles. It is a mistake to forget that the creation of new knowledge, for its own sake, is an important part of what makes us human. At the same time, science is increasingly expensive and technologically demanding, and our fellow citizens pay for it. Therefore, each of us has the responsibility to honestly, to the best of our ability, attempt to create new knowledge. Some of this new knowledge will be directly relevant to curing human disease or other important society issues in the short-term. Some of the new knowledge may change conceptual frameworks in ways that are unpredictable, with unpredictable consequences, on both short-term and long-term timescales.

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Quick guide

Prosopagnosia

Alison M. Harris and Geoffrey K. Aguirre

What is prosopagnosia?
Prosopagnosia — from the Greek prosop for 'face' and agnosia for 'ignorance' — also known as face-blindness, is an impairment in the ability to recognize other individuals by their faces, sometimes even those of parents, siblings and spouses (Figure 1). Despite its potentially distressing social consequences for sufferers, this disorder has proved a boon to cognitive neuropsychology research, providing some of the earliest and strongest evidence for the existence of 'face-selective' processing in the human visual system. Although first systematically observed only 60 years ago by Joachim Bodamer, prosopagnosia is now the subject of an extensive literature. Despite what you may have heard, prosopagnosics seldom mistake their wives for hats.

What causes prosopagnosia?
Prosopagnosia can be 'acquired' as a result of brain damage, specifically from lesions to the occipito-temporal region. In recent years, there has also been growing interest in quantifying lifelong impairments in face recognition, known as developmental or congenital prosopagnosia. In such cases, the etiology is often unknown, but evidence of familial inheritance suggests a genetic component in at least some individuals. Recent research has also examined whether links exist between congenital impairments in face perception and social developmental disorders like autism.

What cognitive systems are affected in prosopagnosia?
Impairment of any number of cognitive systems, from perception to memory, could result in a failure to recognize familiar faces. Yet, although prosopagnosia is often accompanied by mild to moderate difficulties in object recognition, prosopagnosics may learn to rely on non-face visual cues, such as hairstyle or gait, for recognition, as well as information from other modalities, such as voice. This allows many prosopagnosics to discern facial characteristics such as gender, age, and emotion. Prosopagnosia is therefore commonly conceptualized as reflecting damage to a cognitive system specific to visual processing of facial identity. Supporting this idea, Moscovitch and colleagues (1997) described a patient, CK, with a severe deficit in general object perception whose face recognition was nonetheless intact. This 'double dissociation' between prosopagnosics and object agnosics like CK supports the existence of two separate visual processing streams for faces and other objects.

Figure 1. Prosopagnosia.
In Giuseppe Arcimboldo's The Vegetable Gardener (Natura), prosopagnosics can see the vegetables, but not the face. Object agnosic CK, on the other hand, easily perceives the face but not its unusual composition. (Sistema Museale della Città di Cremona — Museo Civico "Ala Ponzone".)

What neural systems are affected in prosopagnosia?
Acquired prosopagnosia is frequently associated with bilateral (occasionally unilateral, right) damage to extrastriate visual cortex, particularly the