it. The global agribusiness company Syngenta, which was set up in 2000 in the merger of the agricultural divisions of Novartis and AstraZeneca, also supports the project.

Principal investigator Maarten Van Helden from the National School for Agricultural Engineers at Bordeaux has been running the project for around two years with his research staff and Syngenta employees, but this year, volunteers from the general public recruited by Earthwatch will be participating for the first time.

Viticulture in places like the Bordeaux region is ideally suited for the development and study of sustainable agriculture, as its primary focus is not on quantity but on quality and character. The natural environment around vineyards, known as the 'terroir' helps to create the specific character of a wine. Therefore, wine growers have a strong interest in preserving or even enhancing the terroir, which could be defined by elements such as hedgerows, small forests, or rivers and lakes. Van Helden's project aims at a better understanding of the biodiversity in such environments and providing farmers with new ideas as to how they might enhance it.

The hope is that natural biodiversity, possibly including species that were lost and then reintroduced, will contribute to a natural regulation of vine pests and will also help to prevent erosion and land degradation, and avoid run-off of fertilisers and pesticides. While some "farmscaping" measures have already been tried in some areas and are recommended in farming guidelines, their success needs to be monitored in the long term, such that future measures can be based on solid scientific evidence.

When reconsidering their production methods, wine growers will also be able to benefit from the recent publication of the complete genome sequence of the Pinot Noir grapevine. Anne-Françoise Adam-Blondon, who was involved in the sequencing work, commented on the biodiversity project: "I think that it is a very interesting objective; in parallel it could be interesting to introduce more diversity in terms of grapevine cultivars."

Michael Gross is a science writer based at Oxford. He can be contacted via his web page at www.michaelgross.co.uk

Q & A

Jeffrey C. Hall

Jeff Hall was born in Brooklyn, New York, but spent most of his childhood years in the suburbs of Washington, DC. He was an undergraduate at Amherst College in Massachusetts, where he began to study genetic phenomena in Drosophila*. He continued in this vein as a graduate student at the University of Washington (Seattle). When he subsequently became a postdoctoral researcher at the California Institute of Technology (Pasadena, California), he made a lateral move into neurogenetic studies of* Drosophila *(just as a duck might make a lateral move* à l'orange*). After his CalTech stint, he returned to Massachusetts to become a faculty member at Brandeis University (Waltham), where he has spent almost all his time from 1974 to the present. He has also faked some work at the University of Maine (Orono), off and on during the current century. Throughout most of Hall's career, studies performed by him and co-workers have involved genetic, molecular and neurobiological analyses of reproductive behavior in* Drosophila*, as well as analogous investigations of the fruit-fly's biological rhythms — along with ways that those two phenomena overlap by virtue of certain gene actions.*

Who had important positive effects on your formative years? A key early influence was Philip Ives, my first mentor. When I was an undergraduate, Ives was a sub-faculty researcher. He went deaf when he was a student of A.H. Sturtevant in the 1930s, making it difficult for him to secure a standard job. So he was given an ostensible 'charity' position at the little college in question (even though he was one of the few serious biological researchers there). I was assigned to Ives as a research assistant. He was a superbly dedicated trainer, from whom I learned many nuts and bolts about *Drosophila* genetics. Just as important, Dr. Ives encouraged me to 'love the little flies' — implying one must feel that way to get the most out of one's organism. This view resonated with me, for I realized that I did love working with them in general and during a given moment. Many years later, I noticed that some of those who started studying *Drosophila* seemed at best indifferent to the little flies and even seemed to fight them, which conceivably undermined the quality of their research.

Another important mentor was, oddly, a chairman: Herschel Roman, founder and longtime chair of a seminal Genetics department, in which I was fortunate to be a graduate student. He made that research group into a tribal entity, fostering relentlessly positive interactions amongst its members (beyond mere camaraderie and carousing). Roman also took many of the individual trainees in his department under his wing and routinely informed himself about what we were doing experimentally. He provided superb guidance for our activities as nascent professionals and even made specific research recommendations from time to time. I was not, and should not have been, singled out for Roman's attentions. But I appreciated his awareness of me, which included that Herschel catalyzed scenarios by which I received offers for both postdoctoral and faculty positions. Years later, my admiration of him grew, as I realized how special he was as a biologist in general and a leader in particular. Prof. Roman was the opposite of 'chairman as empty suit.' Among those operating at that high level whom I have encountered since, few have offered his kind of close support for the people they're supposed to lead.

Do you have any other 'heroes,' as it were, among researchers in your field? Yes, two stand out; like Ives and Roman, they are pretty-much unheard-of by now. One is Douglas Kankel, a co-post doc of mine. Doug and I worked side-by-side on two neurogenetic projects using *Drosophila*. He was one of the most insightful critics I've known; but he displayed this attitude with deep knowledge and sharp wit, never by way of pure naysaying. Kankel opened my eyes to the potential of the little flies. I had been laboring in the micro-arena that could be called 'genetic genetics,'

as opposed to 'bio-genetics' where one takes the approach just implied to analyze a biological phenomenon. Doug stressed the importance of adopting a self-demanding attitude, in which one must assimilate many kinds of biological knowledge to apply to the problem at hand, no matter how formidable the disparate pieces of expertise may be.

The second idol I'll mention is David Suzuki. He got into genetics at the same college where I did, although we did not overlap. I was fortunate to meet Suzuki as a graduate student, when he was at the University of British Columbia. His work was crucial to the resurrection of *Drosophila* from the ash heap of biological research. Back then, most of the work in the fly was genetic genetics, addressing ever more narrow and esoteric questions. Herschel Roman would come into the lab where I toiled as a graduate student and (jocularly) pronounce that 'you folks are working in a dying system.' Suzuki contributed mightily to turning this around. He began systematically to screen for developmental mutants in *Drosophila* that were temperature-sensitive. The genetic world reacted to Suzuki's approach and accomplishments as if they were genuinely sensational. I heard a hard-bitten local geneticist (not Roman) exclaim that 'he's taking microbial principles into a metazoan!' More broadly, Suzuki was delivering seminar after seminar; and everyone wanted to hire him away from UBC. But Dave did not bite. During the late '70s he left university science and went into television in his native Canada (*Suzuki on Science*; Canadian Broadcasting Corporation), where he has sustained this activity and analogous ones ever since.

David Suzuki's contribution to resurrecting *Drosophila* is not well enough remembered or recognized. His work came to the fore slightly in advance of the genetic screens performed by Seymour Benzer and other drosophilists. Suzuki's conditional mutants began to appear in print during the same year that Benzer's seminal search for behavioral mutants was reported, but the former's accomplishments seemed more significant at the time (perhaps because the development of animals was taken more seriously than their behavior). And these activities occurred well before the Nüsslein-Volhard/Wieschaus screen for their famed developmental body-plan mutants. They shared the Nobel Prize with Edward Lewis. Before this, only fly people seemed aware of Ed's activities. He once said to me, in his mensh-like manner: 'Jeff, I don't publish because of what people say, that I'm a perfectionist or something; actually I wasn't getting anything done until I started looking at embryos expressing *bithorax* variants.' These of Lewis's studies were picking up steam when I was at CalTech, ending in the early '70s, and they culminated later that decade in his famous *Nature* article. (I include this apparently stray point as a preview about problematical features associated with 'a number' of other articles that appear in rags of this ilk.)

You just alluded to being a post-doc with Seymour Benzer. Is he not another of your heroes? Not really, despite all his achievements during a long career, ending only with his death at the end of last year (an event that saddened me greatly). It is rather awkward to say this about someone so recently deceased, but I feel that Seymour was not a leader in the same ways as those I've pointed out, neither to his group nor to the field of neurogenetics at large. Indeed, he actively disclaimed any leadership role. I regret that he did not promote the field more strongly. (He might have done more than give 'big talks.') He otherwise shied away from using his fame, in that he demurred from going to the mat on behalf of certain former co-workers when they found themselves at crucial tipping points in their careers.

You sound especially grumpy about scientific luminaries: why? I can't help feel that some of these 'stars' have not really earned their status. I wonder whether certain such anointees are 'famous because they're famous.' So what? Here's what: they receive massive amounts of support for their research, absorbing funds that might be better used by others. As an example, one would-be star boasted to me that he'd never send a paper from his lab to anywhere but *Nature*, *Cell*, or *Science*. These submissions always get a foot in the door, at least. And they are nearly always published in one of those magazines — where, when you see something you know about, you realize that it's not always so great.

Celebrity 'PI's,' who are no longer Professors, have too much in the way of lavished resources — by which I mean too much money to do good work! They can and do hire very large numbers of workers, but it is at-best difficult closely to interact with and properly to supervise these bloated numbers of personnel. Such Actual Investigators (AIs) cannot easily gain their boss's attention; and the latter is unable to provide the required close, ongoing scrutiny of their research. There is huge pressure on the overworked, anxious AI to bring something 'great' to the boss, who wants everything to go to a vanity journal. One outcome of these antics is that some bizarre stuff is salted throughout this overly conspicuous subset of the literature.

The implication is that you are not graced with the ability to function in this manner? Is this true, and is it why you are about to leave science early? Yes and yes. I admit that I resent running out of research money. If Seymour and the like are (or were) the Dave Singleman's of our business, I am amongst the Willy Loman's. This means, for instance, that recent applications from our lab have had their lungs ripped out, often accompanied by sneering, personal denunciations — perhaps reflecting the fact that this old-timer has lost his touch. But I still love the little flies and claim that my colleagues and I could continue to interact with them productively.

Whether or not a researcher of a certain notoriety deserves that the 'support system' keep him going, there is a far more general problem: What props up biological research, at least in the vaunted US of A, involves a situation so deeply imbued with entitlement mentality that it has sunk into institutional corruption. A principal symptom of this state of affairs involves the following: People are hired after they have undergone long stints of training; and a potential hiree must present a large body of documented accomplishments. In my day you could get a faculty job with zero post-doc papers, as in the case

of yours truly; but now the CV of a successful applicant looks like that of a newly minted full Professor from olden times. Notwithstanding these demands, and the associated high quality of a fledgling faculty-level type, the job starts with some 'set-up' money for equipping the lab; but next to no means are provided to initiate that 'research program' and to sustain it during the years to come. US institutions (possibly also those in other countries) behave as though they and their PIs are entitled to research funding, which will magically materialize from elsewhere: 'Get a grant, serf! If you can't do it quickly, or have trouble for some years — or if your funding doesn't get renewed, despite continuing productivity — forget it!' But what if there are so many applicants (as there are nowadays) that even a meritorious proposal gets the supplicant nowhere or causes a research group to grind prematurely to a halt? What if the situation is worsened when the government at hand is anti-science and otherwise squanders its resources on international adventurism?

Unlike most professionally based endeavors, the business of biology gives you only the spatial wherewithal to do the work for which you were hired. You get nothing for the personnel required to function, let alone for all the items needed to get through a given week of work. Might an institution imagine that it should devote part of its 'capital fundraising' toward endowing the ongoing research of its employees — at least so that no such effort would abruptly sink to the null point? The answer is 'nice try: we will raise funds, but we'll put them all into building buildings — in order to fill them with additional hires, who will be as haplessly on-their-own as is ill-fated you.' Having said all this, I acknowledge that 'I got mine' from the government over the course of many years. Thus, as I say 'so long,' one component of my last-gasp disquiet stems from pompously worrying about biologists who are starting out or are in mid-career.

Department of Biology, Brandeis University, Waltham, Massachusetts 02454, USA; School of Biology and Ecology, University of Maine, Orono, Maine 04469, USA.
E-mail: hall@brandeis.edu, Jeffrey_Hall@umit.maine.edu

Quick guide

Yolk

Paul Jorgensen

What is yolk? Yolk is the food deposited in the eggs of animals that will provide the energy and building blocks required for development and growth. Yolk comes in different forms, but the term 'yolk' probably originally referred to the familiar amniote eggs that give rise to reptiles, birds and omelettes.

How is yolk 'eaten'? In amniotes, the embryo forms from cells that sit on top of the denser, more opaque yolk. As the embryo develops, it forms a yolk sac, which acts as an extraembryonic gut, taking up substances from the yolk, digesting them and distributing nutrients to the embryo proper via the circulation. Many animals (e.g. many insects, octopuses, fish, reptiles, marsupial mammals) use yolk sacs to feed the embryo (Figure 1A). But there are also a number of animal groups (e.g. nematodes, sea urchins and almost all amphibians) that do not develop a yolk sac. In such organisms, the yolk is less conspicuous and is perhaps best defined as the nutritional reserves provided by the mother, including yolk platelets, fat droplets and glycogen. These reserves are inherited from the egg cytoplasm by all of the embryonic cells and are consumed intracellularly during development (Figure 1B,C). The patterns mentioned above need not be mutually exclusive. During later stages of amphibian embryogenesis, for instance, the developing gut appears to feed the rest of the embryo by digesting its intracellular yolk, thus functioning much like a yolk sac. Conversely, in embryos with yolk sacs, a small amount of yolk might infiltrate the embryonic cells.

What are yolk platelets? Yolk platelets are usually the dominant components of yolk. In fact, yolk platelets are often the dominant components of eggs. In the African clawed frog, *Xenopus laevis*, yolk platelets make up 50% of the egg volume and 80% of the egg protein. Yolk platelets come in a number of

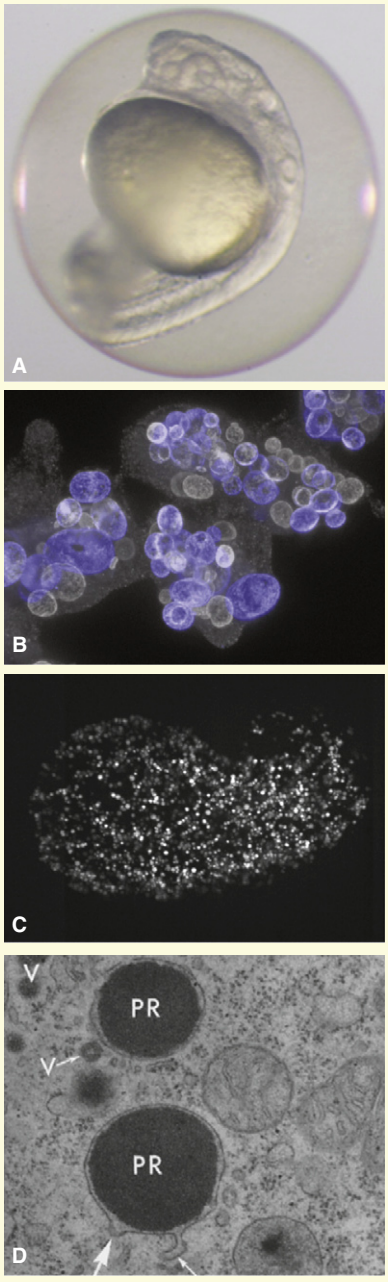

Figure 1. Diversity of yolk.

(A) A big ball of yolk underlies the zebrafish embryo and is being consumed by a yolk sac (Photograph by Steven J. Baskauf). (B) In *Xenopus* embryos, yolk platelets (white and/or blue spheres) are prominent in all cells early in development. (C) Yolk platelets (white spheres) are scattered throughout the developing *C. elegans* embryo. (D) Mosquito oocytes contain yolk platelets with central crystals of Vitellogenin derivatives (PR). (Reproduced with permission from Roth, T.F. and Porter, K.R. (1964). Yolk protein uptake in the oocyte of the mosquito *Aedes aegyptii*. J. Cell Biol. *20*, 313–322. Rockefeller University Press.)