The Restriction Digest

G.S.A. Newsletter

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Johns Hopkins University School of Medicine

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GSA Notes

by Krishna Juluri
GSA President

I would like to take this opportunity to update you briefly on some of the GSA happenings during the past months. Most of our events at the beginning of the semester were designed to give a warm welcome to the new graduate students at Hopkins. GSA members provided useful information to the new students during orientation during the graduate student panel discussion and helped give tours around campus to the new students. Later we hosted the Orientation Happy Hour in which students were introduced to their big sibs. Also in September was the Fall Picnic. Unfortunately rain forced us to move indoors and substitute burgers and dodge-ball with Chinese food and board games, however, the turnout was excellent and everyone had fun. Finally, during the last week, we organized a trip to the National Aquarium in Baltimore with free tickets provided for the first years.

Our next event will be the Halloween Masquerade party which will be held on October 31st from 9PM to 1AM

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Nice Guys Finish First

By Dan Gorelick

On October 8, 2003 every graduate student's dream came true for me. My mentor, Peter Agre, won the Nobel Prize in Chemistry. Peter got the call at 5:30AM. I heard the news three hours later on the radio like everybody else.

I called the lab. "Hey, Jen, do we still have lab meeting this morning even though Peter just won the Nobel Prize in Chemistry?"

"No, you idiot. We don't have lab meeting. Now get the hell in here."

I flew into lab and postdocs Kelly, Ramana, and Jen greeted me with applause. We all hugged. We were exuberant.

That morning is now an indelible memory, akin to where I was on the morning of September 11 (sacrificing rats to measure Aquaporin-9 levels in liver) or where my parents were when JFK was assassinated.

As additional lab members arrived they were greeted with applause and hugs. Faculty members dribbled over to festoon the lab with congratulatory signs and decorations. We returned their adulation with applause—in fact, we members of the Agre lab performed nothing to merit the Prize—but we were so full of joy that applause was a natural catharsis.

Steve Desiderio brought us a case of bottled water. Maybe it was intended for Peter, but the lab drank it.

Peter hadn't arrived yet but I had already answered a call from a reporter in Brazil. Trish, Peter's administrative assistant, concocted an impromptu triage system for dealing with phone calls. Most

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Upcoming Events

Wine Tasting Seminar
November
TBA

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Peter Agre
"Aquaporin water channels--from atomic structure to clinical medicine"
November 25, 4PM
West Lecture Hall WBSB

Next submission deadline:
January 16, 2004

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Cloning Response

By Philip O'Herron

In any ethical decision we make as scientists, we cannot go with our gut feeling but must use sound reasoning and acceptable starting principles to determine the appropriate course of action. Nowhere is this more needed than in the discussion surrounding the harvesting of embryonic stem cells for research purposes. As most scientists do not have expertise in philosophy or ethics, we often need to work in collaboration with other thinkers in order to fully grasp the issue. A philosophy that seeks to defend a predetermined course of action rather than discover the truth about reality isn't worthy of the name. And when the question at hand is one of experimentation on human beings, with all of the profound effects that any decision will render, we must especially take care to use sound reasoning and be honest in our deliberations. How our society determines this ethical issue will have some of the most serious effects of any ethical question ever raised. Many of these effects no one will be able to see until years or even decades from now, and so we cannot fully appreciate what they will be. But we must do the best we can with every issue and only a consistent reasoning that makes a sincere attempt to discover the truth will provide a sustainable ethical framework and ultimately a sustainable society.

In the May 2003 issue of this newsletter (http://www.hopkinsmedicine.org/gsa/news.html), Dan Cohen, in his defense of cloning, suggests that we "present a definition of humanity, in terms of physical development and consciousness, that preserves the moral distinctions between organism and tissue and honors our commitment to healing those afflicted by the vagaries of degenerative disease." The problems with this statement are abundant. I suggest we present a definition of humanity that actually defines what a human being is, rather than one that allows us to defend a certain behavior. As I argued at length in my first article, physical development and consciousness cannot be used as objective indicators of the presence of human life (arguments Mr. Cohen simply ignored). And they certainly cannot be used to distinguish organism and tissue (a distinction which has moral relevance but is in fact a scientific distinction). Also, the role of medicine in service to the ill in no way plays in to the definition of humanity. Medicine's role is to aid all of humanity. If it were to define humanity in such a way as to exclude some so that the excluded can be used for the benefit of the included, then it would be failing to fulfill its role.

I believe that the problem with Mr. Cohen's arguments can best be seen if we employ two fundamental philosophical distinctions – the actual versus the potential and the whole versus the part. The whole versus part distinction is the distinction between organism and tissue. An organism is a whole, and it coordinates and arranges all of its parts to perform appropriate functions that allow the organism as a whole to function. The parts are governed by the whole and work for the good of the whole. With the destruction of the whole (death), the organism loses its ability to maintain all of its parts in a functional manner to preserve life. At this point it is ethically permissible to take organs (parts) since the organism (as a whole) no longer exists. The legal system has decided that brain death is the defining moment for when life passes. This is because without brainstem activity the organism cannot function in a coordinated manner. Artificial feeding, breathing, and blood pumping may enable individual cells to stay alive, but the organism as a whole is no longer alive. This quality of organisms that renders them "capable of growing, maturing, maintaining a physiologic balance between various organ systems, adapting to changing circumstances, and repairing injury"[1] is what separates a living organism from a dead one and an organism from tissue. This is simple scientific fact. And it is a fact that embryos starting at the level of a fertilized egg posses this characteristic and therefore are properly to be considered organisms and not tissue.

Mr. Cohen acknowledges that embryos are organisms by the fact that he tries to argue around the chief requisite in organ donating situations – namely prior death of the individual. Commendably he does not claim it is just tissue in a dish, which most supporters would do. Instead, he attempts to combine two supposedly acceptable behaviors to defend the research. The first is that it is okay to kill a person who will die anyway (the deformed twin) and who if not killed another's death will result (the healthy, conjoined at the head twin). (I disagree however that it is acceptable to kill this twin. Allowing the twin to die naturally after separation surgery, assuming all attempts to save his life have been exhausted, is ethical. But directly killing the twin is not.) And the second is that it is okay to create a person (the big-brother's-spare-parts baby) to use parts of them to help another live (the ill, organ-needy, older sibling). And so it is therefore okay to create a person who will eventually die (if we leave them as they are), and kill them to help another person live who might die without the parts that the death of the first person gives.

However, this combination of permissible behaviors is not an acceptable way of addressing an ethical question. From Mr. Cohen's reasoning, it would be perfectly acceptable to genetically alter your clone so that the baby would die one year after birth. With enough willing women, armies of these babies could be created and their organs could be harvested at one year for needy transplant patients. The problem is that necessary descriptive details of the two proposed acceptable behaviors have been left out. For instance, it may or may not be acceptable to create new life for the purposes of organ transplantation. But I think few would find it acceptable to create new life to be killed for vital organ transplantation. Likewise, people may sympathize with parents who elect to kill one twin to save another. But if we had been told that the parents had strategically arranged things so that the current situation would arise, we would think them criminals. There is a critical missing link in the creation-for-use-for-saving/destruction-for-saving chain. Even if both

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segments of this chain are ethical, it doesn't mean we can overlap them as we wish and make creation-for-destruction-for-use-for-saving ethical.

Another problem with this argument is the misuse of our second philosophical distinction – the actual versus the potential. A person who is alive is actually alive whether or not they will die eventually (as a matter of fact, we all will). There is no such thing as a potential life in this sense. The only way in which something currently alive can be called a potential life is if it is a part of a living thing (such as an egg cell) and has the capability of becoming an entire distinct living thing (such as a new organism). It is an actual new life when it becomes its own organism but it is also actually alive as a part when it is a part of the parent organism. However, a handicapped child who will die from deformities is actually alive and not a potential life. So also is a living embryo, even if it will die if not placed in a womb. The only change from potentiality to actuality is in the sense of becoming a new distinct life separate from the parent. If we can eventually tinker with fibroblasts to make them oocytes, then when an actual oocyte is created we have a living organism. But as long as the cell is anything else, it is still only a part, and therefore destroying it does not destroy a living human organism. This is why opponents of harvesting embryonic stem cells do not object to experiments on egg or sperm cells that do not involve the creation of a new embryo.

Lastly, the claim that human behavior is a measuring stick for ethical behavior is absolutely untenable. All one would need to do to determine whether a certain action is ethically permissible would be simply to do it, and thereby set the precedent for such behavior. Nothing then would be unethical. Likewise, court decisions are not infallible guides. My arguments stand or fall on the merits of logical reasoning, and the courts are only referred to as evidence of their accepting the argument; not as an authority whose decisions we should accept prima facie without checking their rationality.

It is so important in the delicate field of ethics to be completely open about the situations we deal with, and to strive honestly to discover the truth. Only then should our behavior be determined in accord with the truth. Although it may not always get us the result we want in the immediate, it is the only sustainable ethical framework that will ensure that every generation of medical and scientific researchers properly fulfills its role of service to humanity.

[1] http://www.firstthings.com/ftissues/ft0305/articles/condic.html

For more information see:
http://www.bioethics.gov/reports/cloningreport/research.html
http://www.nrlc.org/killing_embryos/
Weldoncloningfacts 022603.html

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Crossing the Developmental Divide: A Case for Embryonic Research

By Daniel Cohen

One of the things that has always drawn me to science and mathematics is their ability to solve puzzles and paradoxes for which philosophy fails to provide satisfactory answers. Take, for example, Zeno's paradox, which stymied thinkers for 2500 years. The basic format of the paradox is as follows: Achilles is in a race with a tortoise. Being a fair sportsman, Achilles gives the tortoise a 10-meter lead. To catch up with the tortoise, Achilles must cover the 10-meter gap, during which time the tortoise moves 1 meter ahead. In the next time interval, Achilles must run the additional meter; all the while the tortoise advances a little further ahead. If the tortoise is always advancing during the time Achilles is closing the distance between them, he can never surpass the tortoise and will lose the race. Or put another way, if the space between the tortoise and Achilles is infinitely divisible, how can Achilles cover it in a finite amount of time? The resolution of the paradox lies in the understanding of infinite series, as described by Gregor Cantor in the last quarter of the 19th century. Namely, certain classes of infinite series converge on a finite number- thus at some point in the race, the sum of the gap closing times for Achilles reaches an measurable time point at which the two competitors are tied for distance, and the faster one can advance ahead.

While the fallacy of Zeno's paradox may seem intuitively obvious to modern thinkers, philosophers are still endorsing a modified form of Zeno's logic to defend scientifically untenable viewpoints. Nowhere is this more apparent than in the debate over the definition of human life as it is currently playing out over fetal tissue transplantation and somatic cell nuclear transfer (SCNT). Essentially, if we consider a path between two states, one a fertilized egg and the other a human life, there are an infinite number of developmental stages linking the two. How then can we transition between the two states, advancing from embryo to human? In viewing development as an infinite progression, we are often left with the absurd logic of midpoints- that first the embryo must become half-human, then three-quarters human, and so on. Thus arises the *reducio ad adsurdum* argument that the embryo can never attain full human potential

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(analogous to Zeno's paradox), and therefore any developmentally-based definition of life either expressly denies any moral status to an embryo OR necessitates an infinite number of meta-human states, which are ethically useless. Invoking this argument, anti-cloning proponents conclude that the human status of an embryo can only be resolved one way, namely that a developmental transition between embryonic and human life is impossible or without ethical distinction, and therefore, a fertilized egg must intrinsically contain all the value of human life. But nothing could be further from a scientific truth, as small changes in embryonic development can converge on the physically finite, and as such, discrete stages of development can provide objective distinctions between tissue and organism, and organism and human being.

One of the principle objections to this viewpoint is that establishing a boundary between human life and potential life based on stages of development supplants objective, universal definitions for "life" and "humanity" and thus presents a slippery slope with ever-shifting boundaries that reflect limits of scientific knowledge and cultural biases. And it is a situation which should give us pause. But the mere fact that a definition of human life in terms of developmental character cannot be placed in terms of absolutes in no way invalidates the viewpoint that there *exists* a defined threshold, post-fertilization, at which the sum of a developmental series transitions the embryo from potential to human life. To this end, it is worth noting that controversial definitions and boundaries are not unique to the topic of cloning, but are the stuff of long-standing debates in the biological sciences. Take, for example, the definition of "species" in biology- the reproductive species principle is violated by all bacteria, transposons which jump genes through all levels of taxonomy, animals which can interbreed successfully, plants that duplicate their genomes but fail to speciate, and organisms known only from fossil remains. While no single unifying definition of species applies to all organisms, the "species concept", nonetheless remains a cornerstone of ecological and evolutionary studies. Similarly, we can apply a scientific concept of a human developmental threshold to the embryonic research and cloning debate, conceding that its usage will fail to cover every conceivable aspect of human existence (e.g., infants with life-threatening physical defects such as anencephaly).

Few scientists would dissent that embryogenesis can, in principle, be classified into a series of incremental changes- first, constraining the infinite potential of stem cells to specific fates, and then, though coordinated signalling pathways and morphogenetic events, further restricting developing tissue to create a specific organism, (e.g., a human fetus). The challenge, then, is in the classification of these developmental events into discrete stages that correlate with key measures of biological and ethical value, namely viability and individuality/humanness. Simply put, what thresholds of viability (such as embryo implantation, establishment of placental exchange, completion of organogenesis, and viability ex-utero) pertain? And what determinants of humanness do we principally consider? If indivisibility and uniqueness are the key to human nature, then we need simply look to symmetry breaking events in embryogenesis, such as development of the primitive streak (the natural barrier to twinning or cloning), as the hallmark of the transition between embryonic and human life. Or do physical features that differentiate the embryo from non-human primates (noting that people consider cloning experiments on these animals to be ethical) such as brain development also weigh into our definition? While I do not pretend that I can resolve all these questions here, I do think that the outline of this debate convincingly demonstrates that a developmental definition of human life can be built upon objective criteria, and such criteria are not needlessly divorced from ethical considerations. Moreover, the physical characters specified above challenge philosophers to distill their ideas about human life into concrete formulae.

Interestingly enough, we can see this played out in Mr. O'Herron's essay, which addresses scenarios by which embryonic and human life may be distinguished. In the first case, brainstem/CNS activity, which is necessary to coordinate the function of multiple organ systems, is given as a determinant that defines the difference between a living human organism versus tissue. A second definition notes that an organism must be "capable of growing, maturing, and maintaining a physiologic balance between organ systems." For reasons that are difficult to elucidate, however,

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these criteria are then abandoned when evaluating the status of an embryo used in therapeutic cloning. Given that an embryo in culture lacks not only CNS function, but also any distinct organ systems, and cannot grow and mature without artificial life support (e.g. tissue culture media and incubator), it can hardly be equated with a human life, as measured by the *least* controversial standards.

Does it follow, then, that a human embryo is morally equitable with mere tissue or tissue culture cells? Indeed, I believe that adopting such a polarizing perspective is futile, and needlessly dismissive. As addressed in a previous column, it is valid to consider the embryo as potential life (or if one prefers, non-human life), and as such, entitled to a measure of respect and moral standing. But this moral standing is not inconsistent with research, which allows for the embryo's destruction (SCNT) provided that 1) we can establish that the moral treatment of embryos is ethically separable from that of humans and 2) such destruction is not wanton but rather serves a specific moral purpose. While I do not wish to recapitulate the debate, which establishes these two points, I do want to take a moment to detail how we may manifest respect for embryos used in SCNT research. And here I would refer the interested reader to an excellent article from the Hastings Center, "Respecting What We Destroy" by Meyer and Nelson. In essence, Meyer and Nelson conclude that we can preserve the ethical value of embryonic life by imposing a specific threshold of development beyond which embryos cannot be used, using the minimum number of embryos required, and restricting their use to research whose goals can not be attained by other methods. These constraints are easily accommodated by the scientific community and closely parallel the ethical guidelines forged from the debate over animal research. As such, it is possible to envision an ethical framework in which research on SCNT can take place in a principled fashion, placing value both in the sacrifice of the developing embryo, and the lives of those afflicted by disease that depend on stem cell therapies. However, such a framework is only possible if we reject the standards of moral absolutism, and allow scientific knowledge to guide us in establishing biological characteristics that define a threshold for human life.

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calls came to Peter's office, but some came to us in the lab. I began using the greeting 'Nobel Prize-winning Agre lab.' We were all delighted—for Peter, for each other, for ourselves. It didn't take long for somebody to remark on how positive this was for all of our careers.

Peter finally arrived around 10:45AM, after spending the morning fielding calls and meeting reporters at his home. His neighbor is a reporter for the *Baltimore Sun* and dispatched himself to cover the story.

We waited in the hallway for Peter. Members of the neighboring Pederson and Caterina labs and members of our lab applauded when Peter arrived. Peter hugged the women and then greeted all the men. Photographers and reporters recorded everything. Somebody asked Peter to describe that morning's call.

"I got a call from the Swedish Chef and he didn't want to talk about Big Bird."

Peter was unusually well dressed, wearing a button down shirt and tie. As he ran the gauntlet of admirers and approached the lab he turned to proudly display his tie. Barbara Smith, now working in the Pederson lab, was the technician in Peter's lab during the aquaporin discovery and initial characterization (1988-1992). Peter pointed to Barb and motioned to his tie. 'You see this tie? You remember you gave me this, Barb? I've been waiting for a special occasion to wear this.' That tie, a gift from Barb, depicted the periodic table of elements.

Peter slipped into his office and we returned to lab. Mike Caterina caught my attention. "Enjoy this," he said, "it's incredible."

We all agreed we weren't going to do much work that day.

Peter finally appeared in lab and several photographers took his picture—pictures of Peter talking to us, group pictures of us hugging Peter—that group picture was an experience. We all surrounded Peter while the photographer straddled two lab benches so he could shoot us from above. He snapped picture after picture, giving us instructions to move here or there, but never ceasing to take pictures and never moving his eye away from the camera. This lasted for several minutes and is the closest I will ever come to a professional photoshoot.

Denise Montell came into the lab to congratulate Peter. It wasn't obvious whether she was going to give Peter a handshake or a hug. Peter preempted her. "This never happens. I get to hug Denise."

We went up to the conference room. The department splurged for a breakfast platter. About 11:15 Peter arrived and was greeted by almost a minute of applause. Then we ate.

At noon Trish ushered us to Tilghman Auditorium in the Turner Building for a press conference. I sat with most of the lab in the 2nd row. Print and television journalists, photographers, and cameramen sat in the rows behind us. About a dozen basic science faculty and a handful of graduate students were in the audience.

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Chi Dang and Bill Brody sat alongside Peter. They made some brief comments and then Peter took the podium to answer questions. The media peppered Peter with interesting queries ("Does this mean people should drink more water?"). The reporter from the *Washington Post* was particularly incisive. His question demonstrated knowledge of osmosis and diffusion. His article in the *Post* best described the scientific aspects of Peter's discovery.

When Brody brought the conference to a close the photographers stood up, darted over to Peter and snapped a barrage of photographs inches from his face. This must be what happens to the President, I thought. We remained in our seats and reporters hovered around us, asking us to comment on what Peter is like to work with. Cynthia Wolberger came down and told my end of our row that several TV crews were out in the lobby and they wanted to talk to members of the Agre lab so we should go to the lobby. I went up and Landon, a faculty member associated with the lab, was talking to a camera crew. Another crew nabbed me, and then we switched.

We returned to the departmental conference room where lunch—a real lunch, the kind faculty get at meetings, not the CMM/BCMB pizza-stromboli feast delivered from the Northeast Market—was waiting. En route the Hopkins public affairs people guided us back to the lab. The media wanted shots of us working. Graduate students David and George put on a display of pouring bacterial media from one flask to another, while Connie, another graduate student, pushed and pulled some syringe plungers on a stopped-flow machine.

For the second time in as many hours we greeted Peter with applause in the department conference room. Peter made some humble, self-effacing remarks (basically a repeat of the press conference) and we continued to eat. Students and postdocs from other labs congratulated us in spite of our protests that we did nothing to merit Peter's award. I was euphoric anyway.

We returned to the lab later that afternoon. I got onto the internet and found everything I could about today's Nobel—the official Nobel announcement, the wire service reports, a video of the head of the Nobel committee describing why Peter and Roderick Mackinnon won. Other lab members huddled around the computer watching and reading.

Debbie Andrew came by and congratulated us and we were sheepish—too numb, by then, to applaud, we didn't know how to respond.

Ramana, Jen, Kelly and I sat in the lab conference room mulling over the day's events. I began to think about how this would affect me.

I'd always imagined a Nobel prizewinner's lab to be large, wealthy, and efficiently run. It would come as a shock to nobody if Sol Snyder or Bert Vogelstein won. Both men dominate their fields, both publish several high profile publications per year, both have large, incredibly wealthy labs with millions of dollars worth of the latest equipment, both have an administrative team that ensures lab members are able to focus on science and not have to worry about anything else. Not so the Agre lab.

It shouldn't have taken Peter winning for me to realize that Nobel worthy discoveries can happen in any laboratory. Peter Mitchell was ostracized from the mainstream scientific community and made his Nobel contribution in an academically unaffiliated lab which he funded using his personal wealth. He had one assistant. Erich Wieschaus and Christiane Nusslein-Volhard made their Nobel contribution when they were junior faculty members. They are the only two authors of their Prize-winning 1980 *Nature* paper. The list goes on.

None of these scientists had large labs or gobs of government funding when they made their breakthroughs.

What might be unusual about Peter? Well before winning the Nobel Prize many of these scientists used their discoveries to establish large, wealthy laboratories. By the time they won the Prize, they were in charge of a small empire with a level of funding most scientists will never achieve.

After discovering the first aquaporin Peter did not expand his lab into an empire. He did not ask the NIH for money so he could systematically knockout out every aquaporin in a mouse model—a very expensive endeavor, but one that Peter probably had the clout to pull off. (One of his competitors did, and has successfully generated half a dozen aquaporin knockout mice.) Instead Peter made substantial contributions to aquaporin structure and function—studies that require much less money than studies involving transgenic animals. This avenue is also closer to Peter's scientific roots. He trained with biochemists, not physiologists.

So how will the Nobel change the lab? I don't know if the lab will expand to epic proportions, but I bet that Peter will take more risks. He is always telling us to focus on experiments that nobody else is doing, and to not worry about experiments that anybody else can do. We are always encouraged to stay away from the mundane.

Certainly the Nobel Prize adds credibility to the lab. We all thought studying the aquaporins was important; now the rest of the scientific community knows this as well. Suddenly the basic properties of these proteins—how they traffic, whether they are gated, how they are affected by post-translational modifications—will be of more general interest. We still do not know the role of aquaporins in non-secretory tissues.

The most important consequence is that Peter winning the Nobel Prize invalidates the maxim that nice guys finish last. He's the type of guy you'd want to lead your son's boy scout troop. A man inculcated with a traditional protestant work ethic harboring a generous dose of liberal freethinking—more than enough to be a registered Democrat.

I chose a mentor whose scientific achievements I admired. I hoped that I would grow to admire his scientific style as well. Peter is not cutthroat and avoids direct scientific competition. He will only enter a race if he knows he can win. Every time I would complain about a competitor doing something we should have done, he would admonish me with advice from one of his mentors, Steve McKnight: "Don't worry about it. He's doing our work for us."

One of our competitors generated several aquaporin

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knockout mice and categorically refuses to share them, in blatant violation of NIH guidelines. I, heady young graduate student, complained to Peter that we should do something—even if a formal complaint takes longer to wend its way through the NIH bureaucracy that if we made the mice ourselves, I argued that there was a principle involved. Peter agreed that this scientist was behaving selfishly, but what good is it to stand up and publicly call Hitler a jerk? It's something everybody knows, and it won't change his behavior.

His kind euphemisms are legendary around the lab. A mundane scientific paper more descriptive than ingenious, one more suited to *JBC* than to *Science* or *Nature*, is a "scholarly work." And when my results are not groundbreaking, and it seems that I am likely to collect my negative results into a "scholarly work," Peter is fond of reminding me that it will be well cited by people in the field.

Sometimes he is almost too nice. Often he is away from the lab, lecturing at other institutions, too nice to refuse an invitation to inspire young people outside of Hopkins.

We graduate students tend to be insecure. The Nobel Assembly in Stockholm gave me reassurance that will last a lifetime. They've reassured me that Peter's style of science is not only sound, but exemplary. Perhaps I would have come to this realization myself, after graduating. As I entered the 3rd year doldrums of graduate school I began to have doubts—about the success of my research, about the scientific advice Peter would give to me based on his experience.

Peter's Nobel instantaneously matured me. I imagine advising my students the same way Peter advised me: "Science is a marathon, not a sprint."

One thing is certain. If they ever install a fountain in the Restriction Courtyard, you can bet it will be named in honor of the Aquaporins.

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at the Rennaisaance Harborplace Hotel. This will be an exciting event cosponsored by the Medical Student Society, School of Nursing, and School of Public Health. Dress up in your favorite costume and come and meet fellow students from the other schools on campus and enjoy the open bar and buffet. Tickets are on sale now (email us at gsa-g@jhmi.edu to purchase). In addition to the Masquerade, look for the return of our popular wine tasting seminar in November.

Besides the fun and games, we have some more academic events planned. We will be having Dr. Julie Gerberding as our Alicia Showalter-Reynolds Memorial Lecturer during the spring semester. Dr. Gerberding is the head of the Centers for Disease Control and Prevention (CDC). Those of you who follow the news will undoubtedly recognize her from her many appearances in the press during the SARS scare last year. Her work includes roles in the CDC's response to bioterrorism and her research interests include infection prevention, antimicrobial resistance, and medical errors in healthcare settings. I am also pleased to inform you that Dr. James Rothman, the head of the Laboratory of Cellular Biochemistry at Memorial Sloan Kettering will be speaking as part of our Pioneers in Science Lecture series. Dr. Rothman has pioneered the field of vesicular transport and pioneered the use of reconstituted Golgi to study vesicular transport. He also discovered the SNARE proteins and proposed the SNARE mechanism for vesicular targeting and fusion to the plasma membrane. For his contributions in these areas, Dr. Rothman shared the 2002 Lasker award.

I would like to thank you all again for your participation in our events. As I alluded to previously, turnout in this year's events has increased dramatically. Please join us at future events and at our meetings (3PM in room 2-108A in the 1830 Building, the third Tuesday of every month) and feel free to contact us by email at gsa-g@jhmi.edu with any of your questions or concerns.

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Balancing career and life: Experiences from SOM faculty
By Lai Hock Tay (*ltay@bme.jhu.edu*)

Have fun. Don't be afraid to ask really tough questions. Take advantage of your mentors and post-docs. Anxiety is normal. Work hard, and you will succeed.

These are words of advice from a panel of four distinguished research scientists from our very own School of Medicine, who were invited by the Professional Development Office to share their personal experiences with students on how they balance the demands of life and a scientific career. Panel members were Susan Michaelis, Professor of Cell Biology; Jeff Corden, Professor of Molecular Biology and Genetics; Carol Greider, Director of the Molecular Biology and Genetics Department; and Robert Silicano, Professor of Medicine and Director of the MD-PhD program. Wendy Sanders, Director of the Professional Development Office, posed questions to the panelists and invited follow-up questions from the audience, making it a lively and interactive session.

Wendy's first question: "What were the most challenging aspects of graduate school and how did you deal with them?" Susan felt that the first six months had been difficult; she decided she would think about the rest if she made it through to the six month point. The very end of graduate school was also difficult for her, as she had to consider the next step. Jeff was married while he was in graduate school and lived in a house with no heat. The lowest point of his graduate school days was during a winter so cold that all of the water in his sink froze. Jeff also pointed out that students would realize most of the successful experiments for their thesis would be done in their last year.

"How did people in your life help you during that period?" a student asked. Carol felt she had been supported by her friends in graduate school; these friends are now her best friends in life. Bob met his wife here at Hopkins on the 7th floor of the Wood Basic Science Building, and they went through graduate school together. "Isn't it more difficult to go through graduate school when you are married?" Wendy wondered. "As long as your ups and downs are not in 'sync', it is ok. One would have more time here and less time there, and your partner, who had been through the same challenges, would understand," said Carol.

"How do you integrate a career in science with your family and friends?" Wendy posed the question to Carol. "I focus to the point of putting on blinders. I work until about 5 PM and head home to the kids. I focus on one job at a time. Also, I rely on supportive people in all areas; I depend on my husband to chauffer the kids and people in the lab to get things done." Susan in turn responded by telling the audience that, because she has no children, she does not need to leave work for home at a specific time and is able to channel her after-work hours into sports. Jeff, who spent 13 years coaching his son's baseball team, pointed out that a big draw for him to academic science is the flexible schedule. "I can set up an experiment to run whenever I want." Bob decided right after his PhD that he wanted to put family first. He acknowledged that to be successful in science is extremely difficult — it is very competitive, and one practically has to give up everything else. Bob, who has two children in their teens, focuses on his family and his students. "There isn't a lot of time for other stuff." On the issue of having kids, Carol, who has two children, commented that a career is equally difficult to manage with or without kids. "It is not necessarily harder with kids; you just divide up your time differently if you want to have kids." "When is it a good time to have kids?" asked an audience member. "There is no good time; it's equally hard at any stage of your career. You want to do it before you are 40. It's biology; we can learn that from other classes," said Carol, accompanied by a good laugh from the audience.

The panelists also revealed some of the strategies that work for them to take care of their children. Carol said she tried different options – daycare, having a nanny, and, currently, an au pair, which she considered least expensive of all if you have an extra room for him or her in your house. Jeff admitted that he brought his kids to the lab on Saturdays, and they learned to pipette. Now, his daughter is in law school! Bob attributed success in this area to his wife who is a genius at finding the right person to help.

Wendy moved onto the next question. "What is most stressful working in this competitive environment?" Susan felt that writing grants is stressful and added that, to her, running a lab is like running a business, in that one has to continue to work to get funding. She also pointed out that taking on more responsibilities to a point where one cannot get anything right is undesirable. Jeff felt that receiving reviews for a grant proposal is like getting a final exam grade. Furthermore, he stated that he doesn't like to be deprived of the time to do bench-work and feels that it is difficult to direct his students if he doesn't have the chance to do the experiments himself. Carol reiterated the theme of focusing on one thing at a time. She agrees that grant writing is stressful, but once that is done, she puts it away and goes onto other things. Bob had similar comments as Jeff; he likes to have the time to do what he wants to do, and he gave an example of what he dislikes — the regulatory details he has to face when working with human subjects.

This event was the first of a series of symposiums for graduate students and post-docs designed to discuss issues that are not normally talked about in the classroom. It was sponsored by the Student Assistance Program, in collaboration with the Professional Development Office.

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