TheScientist MAGAZINE OF THE LIFE SCIENCES wednesday EVERY DAY, ONLINE

| Reprints | Issue Contents

20 comments
Comment on this article

A Fluctuating Reality

Accused of fraud, Anders Pape Møller has traveled from superstar evolutionary biologist to pariah.

By Brendan Borrell

ARTICLE EXTRAS

[Origin of a Controversy]

[Timeline: From Superstar to Pariah]

One day in the early spring of 1993, Richard Palmer received a paper by a Danish ornithologist, Anders Pape Møller. Palmer, an associate editor at *Evolution*, was impressed by the paper, but he was troubled by one of Møller's key statistics.

Although he had met Møller only once, Palmer was familiar with his work. Both were fascinated by the promise of fluctuating asymmetry, the subject of the paper in question. "If you measure the right and left paper in question. "If you measure the right and left sides of the body very precisely, they're never exact mirror images," explains Palmer, an evolutionary biologist at the University of Alberta in Edmonton. "Those differences are random, and what they tell you is the inability of the right side of the body to produce an exact mirror of the left."

Møller's paper claimed that asymmetry in the tail feathers of the barn swallow was passed from fathers to their sons; in other words, it was heritable. But Palmer pointed out in a three-page letter to Møller that the statistical significance of his findings hinged upon a single outlying data point, and therefore "it would be more prudent to present the data, indicate the sensitivity of the statistical result to a single point, and conclude that it is not possible to say much about the heritability of asymmetry with the present data." Instead of addressing Palmer's concerns and those of the two reviewers, however, Palmer says he felt that Møller was just trying to make those concerns go away. Møller ultimately softened the language of the paper and Palmer accepted it for publication, although he says, "I was left with the sense that it was more important for him to get the paper published than to be correct."

Palmer wasn't alone. Evolutionary biologist Bob Montgomerie of Queens College says it's no secret that Møller bickers with editors and referees. As a frequent reviewer of Møller's papers, Montgomerie found himself endlessly pointing out mistakes, but "the stuff was getting published anyway."

PHOTO BY TIM MOUSSEAU
Anders Pape Møller measures birds at a field site in the Ukraine in 2005. Møller and Tim Mousseau have been working on a project to investigate the effect of the Chernobyl disaster on biodiversity.

Meanwhile, a handful of Møller's colleagues had begun distancing themselves from him. His collegial relationship with evolutionary biologist Andrew Pomiankowski of University College London deteriorated after a dispute over one of their papers. Adrian Thomas, an ornithologist at the University of Oxford, stopped replying to Møller's E-mails regarding a proposed collaboration. Rumors began circulating about the ecologist, including one back-of-the-envelope calculation that retraced his putative bicycle route at his field research site using the sampling methodology described in concurrent studies. The velocities required an athlete of Olympic caliber.

These suspicions would move into the pages of journals, and eventually into a full-fledged investigation that cast serious doubt on one of Møller's papers. In 2005, Møller's bird-banding permit was revoked, effectively ending his 34-year study of barn swallows. "I've slept badly for five years, now," says Møller via the phone from his lab at the Pierre and Marie Curie University in Paris. "I don't think I have done anything wrong." He says his students have been harassed, his collaborators have been discouraged from working with him, and his family has suffered. His friend, Tim Mousseau, a biologist at the University of South Carolina, has seen firsthand how the investigations have affected him. "I think it was very investigations have affected him. "I think it was very hurtful for somebody who has dedicated their entire life to the pursuit of knowledge," he says, adding: "The only recognition he wants is for his science."

A FARMER'S BOY

Møller was born in the town of Nørresundby on the day after Christmas in 1953. Nørresundby lies in the peatlands of Denmark's sparsely populated Jutland peninsula and is the site of Lindholm Høje, a major Viking burial ground dating back more than a thousand years. While his ancestors took to the sea, Møller took to the land: "I was a farmer's boy."

During his youth, he tended to his father's cows, sheep, and chickens, and, when he had the chance, he watched birds. In the fall of 1969, 15-year-old Møller visited Thorkil Duch, an electrician and an amateur naturalist in the area, who advised him to keep a notebook of his observations. Duch also taught him to capture birds and wrap identifying bands around their legs so that Møller could keep track not just of species but also individuals. Møller returned home and started banding the barn swallow, a slight, nimble bird that would launch his scientific career.

Four years and untold notebooks later, Møller published his first scientific article on barn swallows in a Danish bird journal. He continued to publish throughout high school but was advised not to pursue a career in biology. "I was told there were so few positions that it would never pay off," he says. He went into biology anyway, and was accepted to a doctoral program at the University of Arhus. There, he quickly distinguished himself as a skilled ornithologist and a diligent worker. He wrote modest papers, focusing on mundane but telling details on the lives of common birds: when crows forage, how magpies die, and where blackbirds lay their eggs.

"I was left with the sense that it was more important for him to get the paper published than to be correct."
- Richard Palmer

Shortly after receiving his doctorate in 1985, he was publishing 20 to 30 papers a year in international journals: *Behavioral Ecology and Sociobiology*, *Animal Behavior*, *Evolution*, and *Oikos*, and in 2002 was selected as an ISI Highly Cited researcher in the field of ecology and the environment. He has now published nearly 600 papers. Dolph Schluter, another former nearly 600 papers. Dolph Schluter, another former editor of *Evolution*, says, "He's not just prolific. He's good. He's drawn comparisons [and] pointed to relationships that people will be digging through for years."

Part of Møller's success stemmed from his ability to forge productive collaborations. A list of his coauthors is a who's who in the field of behavioral ecology, and he was as likely to collaborate with a top scientist as with a provincial one.

When asked about his tremendous output, Møller laughs nervously and attributes it to his life on the farm: "You had to work hard to earn your dinner."

A FIELD TAKES OFF

Palmer, who published a review of asymmetry in *Science* in 2004, describes the early pioneers in the field with reverence: Lee Van Valen was "brilliant" and Kenneth Mather wrote "wonderful" papers. Articles on the topic had been trickling in since the 1940s, but the field really took off in the early 1990s thanks to Møller. "Without a doubt," Palmer says, "you can trace the spectacular popularity in this whole subject area to one paper Møller wrote on barn swallows."

Møller had previously shown that longer tails exhibited greater symmetry than shorter tails, a finding which led him to postulate that symmetry could be an indicator of "good genes." Møller's talent, Pomiankowski says, "is taking theoretical ideas and seeing ways they can be tested with data." So Møller promptly modified the length and asymmetry of the birds' tail feathers and found that females preferred the most symmetrical males (see sidebar). A paper, "Female Swallow Preference for Symmetrical Male Sexual Ornaments," was published in *Nature* in 1992 and was immediately touted by media outlets around the world: symmetry equals attractiveness.

Scientists were skeptical. "The results were too amazing to believe at face value, which was partly what made us look so closely at the paper," says evolutionary biologist Gerald Wilkinson at the University of Maryland, who criticized the study in a published note to *Nature*. He and ornithologist Gerald Borgia had noticed inconsistencies with error bars on graphs and doubted the paper's conclusions. Møller published a response to their criticisms, but as Wilkinson recalls, "The only way we could reconcile what he said is if his figures had been in error, if they had been crafted figures had been in error, if they had been crafted improperly."

HERITABLE ASYMMETRY?

In 1993, despite the doubts, evolutionary geneticist Therese Markow invited Palmer and Møller to a conference she organized at the Mission Palms Hotel in Tempe, Ariz. During the conference, Møller first suggested that asymmetry was heritable. This idea is a precondition for his "good genes" theory of sexual selection to apply to his barn swallows: If symmetric tails were not heritable, then they could not have evolved under sexual selection. "A rule of thumb is that everything is heritable," says Møller. "Some things have high heritability and some have a low heritability. This is one of the traits that has a low heritability, but it's very interesting."

Møller mentioned several important studies that demonstrated heritability, but the other attendants insisted that there were none. (Palmer agrees that some evidence exists for the heritability of asymmetry, but he says that it is one of the "squishier" connections.) Møller and Randy Thornhill, who was also at the meeting, set out to prove them wrong by performing a meta-analysis of the relationship between asymmetry and heritability. Thornhill, a professor at the University of New Mexico, Albuquerque, and coauthor of the controversial book *Natural History of Rape*, had been accused of sloppy science in the past. Palmer says he puts the two "in the same basket."

"I've slept badly for five years, now. I don't think I have done anything wrong."
- *Anders Møller*

Palmer rejected the manuscript at *Evolution* after receiving two "vitriolic" reviews that raised serious questions about its quality. Møller and Thornhill stood by their conclusions, and eventually the paper landed at a less prominent journal, *Journal of Evolutionary Biology*. The editor there sensed the brewing controversy and, in an unorthodox move, invited seven commentaries to be published alongside the original article in 1997.

The overall tone of these responses ranged from accusations of sloppiness to hyperbole to outright dishonesty. One set of authors suggested that Møller and Thornhill had a hidden agenda in analyzing their data: supporting their "good genes" model of sexual selection. Pomiankowski, who wrote a gentler response selection. Pomiankowski, who wrote a gentler response to the paper, says, "I was privy to earlier versions of his analysis, and the numbers kept on changing." In their reply, Møller and Thornhill deny a hidden agenda, adding that "there is a real danger to a scientific field when established workers in the field view their colleagues as competitors and use innuendos and direct claims of malpractice to try to get an edge." If Møller and Thornhill really thought they were fooling anyone, they were only fooling themselves.

THE FINAL STRAW

Then, in 1998, Møller published his 33rd paper in the Danish ecological journal *Oikos*, describing a relationship between asymmetry in oak leaves and damage caused by plant-eating insects. A year later, *Oikos* editor-in-chief Nils Malmer received an E-mail from Jorgen Rabøl, a former professor in Møller's lab at the University of Copenhagen, who suggested that the data had been fabricated. Møller was shocked. "I had saved all these bloody leaves from these trees," he recalls. "I thought perhaps there was something wrong with these measurements." He went back to his crackling leaf samples and remeasured them. He soon realized that the new data failed to support the conclusions in the *Oikos* paper. He felt humiliated and did what he and Malmer agreed was the only honorable response: He published a retraction.

That could have been the end of it. But to Møller's dismay, Rabøl brought the case before the Danish Committee on Scientific Dishonesty in 2001. Rabøl presented the committee with files he had obtained from Møller's technician, and the committee then requested Møller's own data files. Møller delayed for months, insisting that the raw data had been stolen along with his laptop in 1996. Instead, he sent the committee a transformed data set that served as the basis for the paper's three tables. The committee noted inconsistencies in even these files and ruled in 2003: "Neither the raw data kept at the University of Copenhagen nor the data forwarded by the defendant could have generated the results that emerged from the article."

Møller insists that the investigation did not prove his guilt but was instead a character assassination. Indeed, Rabøl had been fired after Møller complained of his lack of productivity, and Møller maintains that the accusations were part of Rabøl's revenge. Møller notes accusations were part of Rabøl's revenge. Møller notes that a second investigation, conducted by his home institution, the National Center for Scientific Research (CNRS) in Paris, did not find him guilty of intentionally committing fraud. But even that verdict states that the committee was "lacking the material evidence necessary to establish innocence."

THE AFTERMATH

Møller still publishes at a healthy pace, although he says his manuscripts are rejected twice as frequently as before the investigation. "He's under the microscope," says former *Evolution* editor Schluter at the University of British Columbia. Yet a look at his recent papers shows that while he is keen on citing his own work, he rarely cites opposing views, perhaps hoping, as Palmer remarks, that they'll just "go away."

Perhaps in response, scientists remain critical and even unkind to Møller. In 2000, Palmer published an unusual essay in the newsletter for the *International Society of Behavioral Ecology*. It was a fable concerning the fictitious Traumweber brothers, Andy and Randy, expert tailors in the "remote kingdom of Glücklichtal, nestled high in the European Alps." Palmer wrote that the maestro of Glücklichtal's symphony noticed that audiences "seemed pleased with performances conducted in the Traumweber tuxedo, but dissatisfied when he performed in his imported tuxedo." After careful investigation, "Andy Traumweber discovered the imported jacket was less precisely made, most particularly in the tails: one was distinctly longer than the other." The title of the piece, "The Emperor's Codpiece," came from its final coup:

According to a palace informer, the Emperor was particularly anxious about his imperial private parts, which he felt were so asymmetrical that they deviated too far from the norm. Fortunately, the Traumweber brothers were able to allay his fears with a profound revelation: In certain very special cases, increased expression of a predictable asymmetry actually signals increased fitness, and one of those cases is testicles (Møller 1994), at least if men are like birds. That's why they subsequently fashioned the Emperor's codpiece to enhance his already conspicuous asymmetry.

The president of the society, Nick Davies, issued an ambivalent apology in the subsequent newsletter.

"Results were too amazing to believe at face value, which was partly what made us look so closely at the paper."
- Gerald Wilkinson

In a devastating book review of *Asymmetry*, *DevelopmentalStability*, and *Evolution*, evolutionary geneticist David Houle at Florida State University, wrote that Møller and his coauthor John Swaddle at the College of William and Mary "repeat the original conclusions of Møller and Thornhill's (1997) meta-analysis of the heritability of asymmetry down to the wildly inflated estimate of average heritability. Although they do address some of the criticisms of others, these are, in effect, dismissed as technical points that do not affect the overall conclusions."

In closing his review, Houle widens his scope to include the gullible souls who jumped aboard the fluctuating asymmetry bandwagon in the 1990s as well as all scientists who succumb too easily to the enthusiasm accompanying new ideas. "We have little choice," writes Houle, "but to seek inspiration from gurus of the newest ideas; sometimes they turn out to be partially right. However, we should never believe them without a struggle. If an idea seems too good to be true, it is probably not true."

These days, Møller's most vocal defender seems to be Mousseau. "I like Rich [Palmer] a lot," Mousseau says, "He's a friend of mine, but he's quite emotional and somewhat irrational in his stance: he just doesn't like Møller." Palmer privately wrote Mousseau and cautioned him not to be so cavalier in defending his colleague. Mousseau, in turn, wrote letters to both *Nature* and *Science* with more than 20 coauthors, defended Møller on discussion boards, and started a petition to give Møller back his bird-banding permit.

Pomiankowski says Møller is in the "limbo land" in which many scientists investigated for fraud find themselves. "I find it an unsatisfactory situation to be in, but that's where we are. I would much prefer that he was properly absolved for what happened or found properly guilty." He'd rather know the truth now. "It's very hard to understand what motivates another person," says Pomiankowski. "You can concoct an explanation about why things go wrong, but who knows?"

Comment on this article

---

comment:
cover story:A fluctuating reality
by David Herz

[Comment posted 2007-01-09 18:42:52]
This well written piece is ample evidence, not that we need it, that even the highly educated, the dedicated, the above all suspicion can behave like vindictive children...except that they are adults, and I am not referring to Moller...In such a vindictive and emotionally wrought climate it is highly unlikely that the so called objective scientific spirit can wend its way to something approaching equity...the myth of the two tailors is evidence of great immaturity of spirit, misuse of power, and simple disrespect.
thank you for allowing me to comment.

---

comment:
Inflated Egos
by Charles Cortes

[Comment posted 2007-01-10 04:25:32]
I agree with Herz, the spirit of the article is just that "payback", unfortunately science is herd like. However, great minds excude ubiquitious pride which when fosterd in the minds of men are very often inflated along with egos.

---

comment:
swallow Tales
by Hugh Fletcher

[Comment posted 2007-01-10 12:28:37]
Scientific publications are full of results that literally do not add up, either through fraud or incompetence. In physical sciences where experiments are repeatable, this is usually discovered if it matters. Otherwise it just gets left, and the word goes around about being careful if you work with X. Can anyone honestly produce a worthwhile scientific paper every 18 days? I have been collecting papers containing meaningless rersults for decades. My favourite swallow tale suggested that cutting 20mm of the males' tails would increase their velocity from about 6 or 7m per second to around 15m/s. This is aerodynamically ridiculous, either the tip of the tale accounts for 75% of the drag, or the swallows were now expending 4 times the energy flying (the square of velocity). Do males normaly fly half as fast as females? The explanation was that they were not before and after tail cutting of each bird, they were different swallows flying in different places, and one or two of the fastest birds happened to be included in a particular group of 4. This, together with the surprising conclusion that the extra speed was bad for the birds, was published by Evans in Nature (394 p 233) and elicited a response from Anders Moller as to the exact selection causing these effects. Perhaps the enthusiasm of rank and file evolutionary biologists is not often matched by their numerical skills.

comment:
Hard to Swallow...?
by Larry Pinkerton

[Comment posted 2007-01-12 02:40:53]
As a layman I am comforted when scientists are held under the glaring light of peer review --it is why I love and support science --warts and all.

comment:
cover story:A fluctuating reality
by Michael Morris

[Comment posted 2007-01-18 13:24:51]
Palmer, as associate editor of the respected journal Evolution, seems to have discharged his editorial duties appropriately with respect to Moller's papers. However, the writing of his mythical tale mocking Moller's work was immature and unprofessional. This is no way to deal with the serious issue of potential fraud - and Palmer should have known that.

This was an excellent article.

comment:
A fluctuating reality
by Michael Morris

[Comment posted 2007-01-18 13:24:24]
I might add that Palmer's tale serves to deepen, rather than shed intelligent light, on the issue of scientific integrity in all its forms.

Specifically:
(i) Can Palmer any longer be trusted as a peer reviewer of scientific articles if he resorts to scarcely veiled 'comedic' ridicule of other scientists in his field?
(ii) What does this say about the Editor(s) of the International Society of Behavioral Ecology, who alowed Palmer's story to be published? A half-baked apology by the President, if that's what it was, seems insufficient.

More broadly, these incidents yet again bring into question the processes of peer review and editorship and cast another shadow (as highlighted, for example, by recent articles in The Scientist) over editorial integrity and professionalism.

comment:
the underlying mechanisms of fraud
by Björn Brembs, PhD (brembs.net)

[Comment posted 2007-01-18 13:23:20]
With the many discussions and publications on the recent high-profile cases of scientific fraud, one starts to wonder if what we are witnessing is a new era of dishonesty or an accidental bout of fluctuating dishonesty.
My guess is that the incidences of scientific fraud will increase in the coming decade(s). While each case is founded in its own

the coming decade(s). While each case is founded in its own peculiar circumstances, the overall situation of raised stakes in science will contribute statistically to an overall increase in scientific fraud.
This ties in nicely with the debate on the number of scientists being trained:

Are we training too many scientists? (The Scientist)
Are There Too Many Postdocs? (Science)
Too Many or Too Few? The Postdoc Production Policy Debate (Science)

With increased numbers of scientists and decreased funding, competition rises. Currently, every single scientist on this planet feels the pressure that he/she needs to become a science superstar in order to survive and obtain a position which will pay the bills. A superstar will only be born in a fashionable topic and thus these topics (largely controlled by a few science journals) are overrun, increasing competition further. Obviously, only very few will become science superstars. Consequently, the incentives of behaving fraudulently have never been larger than today. The number of fraud cases will inevitably follow this trend. Because of the huge incentives (getting a job and fame or landing on the streets in disgrace) I'm doubtful that any control measures can stop these parallel developments.

However, reducing the incentives on the high end (less fame and prestige, less spin-off companies, patents and luxurious meetings sponsored by drug companies) and cushioning the low end (e.g. by capping grant size to increase overall grant number) will also decrease the number of fraud cases in science.

If you are a PostDoc with a family, your contract runs out in three months and every faculty position has 300 applicants, you really feel the temptation to fiddle a little with this one graph which will get you the publication you need to beat the other 299 in order to feed your family. Asking for honesty is probably rather ineffective in such a situation.
That's the much more common low-profile fraud which is probably not increasing but exploding as I'm typing this.
The reasons are clear. Are we going to do something about it?

---

comment:

Underlying mechanisms and the right result

by John L. Morton

[Comment posted 2007-01-18 13:22:13]
I feel that I must agree with Björn Brembs. In such a competitive environment these things are perhaps inevitable. The points made above about publication rate and reputation are also important.

There was a suggestion about a firing for a lack of productivity. In my cynical moments I've sometimes felt that a major part of the problem are some senior investigators who just want results, and as long as they are the right results never seem to question about where they came from, or how rapidly they had have been generated. As Dr Brembs pointed out, if feeding your family depends on this the temptation could be irresistable.

A very good article, and very thought-provoking.

comment:
Nature on breeding cheats
by Björn Brembs, PhD (brembs.net)

[Comment posted 2007-01-18 17:07:23]
Perhaps not surprisingly, a news article just out in the journal Nature supports the "pressure cooker" hypothesis of Breeding Cheats (part of a feature on scientific misconduct).
So most likely, if the current funding situation continues we will see more and more cases of fraud.

---

comment:
plagiarism incident
by Dave Peters

[Comment posted 2007-01-19 00:38:15]
The article notes Houle's scathing review of the Moller and Swaddle book. However, no mention is made of the accusation of plagiarism made by Houle in that review.

Moller apparently plagiarized from a manuscript that he was sent to review, without making attribution. A passage of 200 words was supposedly taken word for word from the then-unpublished work. An apology by Moller, who was sent the manuscript for peer review, was subsequently tendered on the book's web site. While this itself was not data fraud, it certainly amounts to misrepresentation of ideas and thus seems relevant to the discussion.

I'm curious because I've read several accounts of the Moller fraud case and none of them refer to this incident. One also wonders what co-author Swaddle made of all this.

---

comment:
Anders Pape Møller fraud
by Jørgen Rabøl

[Comment posted 2007-01-24 18:07:06]
I was the person accusing Anders Pape Møller for data fabrications in the Oikos paper from 1998 and also raised the case to the Danish Committees on Scientific Dishonesty.

After about seven years of dispute with Møller I know for sure, that he will never admit that he did anything wrong. He seems unable to realize even himself, that he fabricates data whenever necessary and possible. If you are interested in the Danish decision and more details about the Møller-case, please refer to my web site: www.jorgenrabol.dk

---

comment:
laboratory technician
by Jette Andersen

[Comment posted 2007-01-26 20:31:11]

May I use this occasion to correct a few things?
Møller got off scot free in the OIKOS case by blaming the technician (me) and retracting the article on the grounds of "bad measurements". This collided with his thanking me profusely ("heroic task") - even twice, both in the acknowledgments and in the body of the article. Møller's act was the reason Rabøl brought the paper to the attention of the Danish Committees for Scientific Dishonesty. That was too low and cheap, not to mention untrue, for Rabøl to stomac. The animosity between the two arose from that fact, not from sour grapes, nor envy or vengefulness. Rabøl was simply discusted as is the author of this paper. Later Møller saw fit to publicly accuse me of alcoholism and substance abuse (again absolutely untrue), so the low level of arguments at least in our case was set very early and stemmed from Møller. Rabøl left Copenhagen University much later than Møller did. He was sixtyone at the time and simply retired. He is still affiliated to the department.

---

comment:

Moller and Thornhill, Palmer and Houle

by Robert Trivers

[Comment posted 2007-02-05 09:45:37]
Richard Palmer says he puts Randy Thornhill and Anders Moller "in the same basket", meaning equally unreliable. This over his statistical disagreements with a review paper they jointly co-authored.

Moller and Thornhill are very different organisms but I would place them in the following basket. They are both brilliant biologists who have taught me a great deal of evolutionary biology. I can not say that about any of their detractors.

Often overlooked in the so-called Moller scandal is the fact that Anders is—barring perhaps his work on oak leaves—always right. If he is inventing his data, he knows exactly how to invent it. Take his discovery that swallow tail asymmetry in males affects female choice in nature. Did he lead us astray—did a discipline gallop off in a bad direction. Not at all. His discovery has been confirmed in a wide range of birds, and experimentally (using leg bands) in two species. Did he lead us astray when he claimed that bumblebees prefer symmetrical flowers, which in turn are richer in nectar rewards? Not at all, there is a flourishing little discipline pursuing this subject now. Did he mislead us when he claimed that immune characters were related to asymmetry of tail feathers in swallows? Immune connections with symmetry are now routinely reported. Well, was Anders misleading us when he provided a superb review of these findings in 2006? It would be very hard to say "yes" without doing the massive literature review that he did.

In short, you can take two polar views of Anders Moller. He is either a genius whose "empirical" articles are best viewed as brilliant interpretations of what data would like if one bothered to collect it, or else a careful and thorough scientist, who brilliantly demonstrates the importance of key ideas, which later work confirms. Take your pick. In either case, he is a teacher worth paying close attention to.

In conversation and correspondence he has put me on to a steady stream of fascinating work that has nothing directly to do with his own, e.g. the importance of melanin as a factor protecting against own, e.g. the importance of melanin as a factor protecting against infections (re skin color in humans) or a very novel test of self-deception he once suggested, following from the fact that hemispheric specialization in women is associated with their tendency to believe their left breasts are larger than their right. Has he ever misled me, sent me to work of doubtful quality, wasted my time with work that proved trivial etc? Not yet.

Thornhill has also failed to lead me astray. In fact, he has been one of the most reliable (and creative) guides to human sexual selection. Does anyone doubt his finding that women (not on the pill) prefer the smell of symmetrical men at the time of their ovulation? Then let them repeat the meticulous work that led to this finding. Do women off the pill who are paired with asymmetrical men fantasize at ovulation more often about extra-pair copulations? Well, let me put it this way. He and co-workers have now shown the same thing using MHC similarity between mates, and MHC similarity is unambiguous in its measurement (compared to fluctuating asymmetry).

What about his critics? The first thing they do is throw the baby out with the bathwater. Statistical arguments over review papers are common, and it is inevitable that theoretical biases will affect the organization of data one reviews. But the point is that the first serious review was done by Thornhill and Moller. They did a tremendous service bringing together all the data (or most of it). If the presentation was skewed let those not busy actually doing original work, study the matter more carefully, with the aid of the references and analysis already provided to them.

It is worth bearing in mind, that Gregor Mendel's great work in genetics was shown by R.A. Fisher in the mid-thirties to have a less than one in a million chance of occurring, without active fudging of the results by Mendel. Fisher's statistical conclusion is beyond the dreams of Anders' critics, yet Mendel's work is widely understood to have provided the foundation for the science of genetics. Did Mendel make up his results out of whole cloth? Of course not, he would not have come anywhere near the truth. Probably he did sufficient work to realize the underlying logic and then gathered data with strong expectations in mind. Either consciously or unconsciously data was organized to as to show the world what he thought he had discovered.

Richard Palmer's work I respect. He emphasizes methodological and statistical rigor and we all need that. However, his mind sometimes also has a negative cast to it—not only in obsessing over the possible transgressions of others—but also in tending to claim that associations are unlikely which later turn out to be commonplace. When my co-workers and I showed that human dance obeyed striking symmetry associations predicted from the work of Moller and Thornhill (using motion-capture cameras to create a relatively pure test of theory), Palmer thought he immediately spotted a hideous mistake. It looked like our subjects showed average levels of relative asymmetry of 10 to 30%. He slyly asked if perhaps our youngsters had an unusual degree of deformity. It soon became apparent that he had merely read the paper carelessly, that we added our nine asymmetries, instead of taking their average. Palmer's kind of bias repeated day in and day out will drive a scientist further and further from the truth, as he struggles all the while to subject work he finds suspect to rigorous scrutiny.

Houle is not in the same basket. His review of Moller and Swaddle's book deserves to be published in the Annals of Psychiatry. It begins by thoroughly misrepresenting the history and logic of his own discipline, population genetics, and then goes downhill from there. In one wild moment, he claims that the only value of this important book, is in showing others how not to do science. Never mind that you could burn the entire book except for the bibliography and you would still have a treasure (or vice-versa, in which case you would have a wealth of useful and new ideas). Houle's piste de resistance is the discovery that in online material accompanying the book, Moller and Swaddle fail to attribute 200 words in a row that they borrowed from someone else, a mistake to be sure, but not quite the end of the world.

In turn, corresponding with Houle is to enter into a strange world of accusation, denial and misrepresentation. When I sent him my dance paper, he asked me to resend it because my spam-detection machinery had blocked my outgoing attachment. Sounded unlikely on its face (indeed, the first such occurrence in my life) but a moment's study of his message showed that Houle's own university informed him that they blocked the attachment and he could retrieve it if he acted within 24 hours. Further correspondence produced displacement of his mistake on to others and additional bizzarities not worth describing.

In falling so easily into the roles of prosecutor and judge, both these organisms forget Jesus' lesson (among those of others) that we should "judge not, lest we be judged, for with the judgment you pronounce shall you be judged". A little bit less obsession with the possible failings of others and a little more with their own failings ought to produce positive results all the way around.

Finally, I do think—as I have told Anders a long time ago—that he made a mistake not taking total personal blame for the oak leaf problem; that is always the better posture and would likely have put this matter to rest. But should he be crucified for choosing the route he did? Not in my book, basket or moral system. He is a brilliant biologist who has taught the world more than his fair share of the truth. He has his defects as do we all, but there is no way he can be right about a subject, time and time again, and be the charlatan his critics try to make him out to be.

---

comment:

The misconducting genius

by Jorgen Rabol

[Comment posted 2007-02-05 14:55:11]
I read the comment by Robert Trivers and the hard core in his argumentation is that if you are a genius you are allowed - and perhaps even wellcomed - to be a cheater and data-fabricator.
I am sorry to repeat myself but as far as I can figure out APM cheats whenever necessary and possible, i.e. the oak leave paper was not his one and only misconduct. On the my homepage www.jorgenrabol.dk I mention two further cases of suspected cheating/fabrications.

---

comment:

Missing comments
by Erik Nørby

[Comment posted 2007-02-06 16:44:23]
To The Scientist,

What happend to the interesting notes attached to this article?

---

comment:
Something rotten in the Kingdom of Denmark
by Anders Pape Moller

[Comment posted 2007-02-16 07:44:59]
While being accused in public by a number of people for several horrible crimes, I would like to present some facts, without intending to enter a discussion based on hearsay, assumptions, or other evidence that can be used to smear a colleague. Brendan Borrell, the author of the article in The Scientist, breached my trust because he never allowed me to correct the factual content of the article as he originally promised. He stated explicitly to me that the article was for his magazine Dragonfire. Later he chose to publish the article a second time in The Scientist without first asking me. If a scientist asks for information for a particular purpose from a colleague, it would be considered unethical if the information was used for another purpose without first requesting a permission. Not so for some journalists, apparently. Furthermore, if a scientific author publishes an article a second time, this is akin to scientific misconduct. Not so for journalists, apparently.

The entire case against me arose when I was head of the ecology group at Department of Population Biology, University of Copenhagen, Denmark in 1995-1996. This period was a great upheaval for the department, which had just been evaluated by an international committee of university ecologists, who wrote a devastating report on the state of the department. My appointment was not less bizarre than the state of the department. Although I obtained the Ôchair in ecologyÕ, I did not have an office or for that matter a chair or a desk for the first two months. That time I shared an open office with six mastersÕ students. Every time I received a phone call, a secretary had to run from another building to ask me come to the phone! Eventually I had to buy furniture for my office using my own start-up money for research! Although I was promised a studentship for a PhD student upon arrival, the dean had no problem never fulfilling his promise during a period of 20 months. I arrived with three post-docs who brought their own funding. They worked at home in my apartment for three months because the department was unable to find a chair, a desk and a lamp for each of them! The department was hopelessly nepotistic, advertising job openings in the journal of the Danish university teachersÕ union to avoid that foreigners applied. I fought a bitter fight as chairman of several job search committees, asking why the University of Kampala in Uganda advertised their jobs in Nature, while the University of Copenhagen that is over 500 years old did not. Another professor at the department, who had previously been chairman of the Natural Sciences Research Council. stated at a meeting in the departmental research committee that they did not want the best candidate for tenured jobs, they simply wanted the local candidate! From then on I realized that there was no way of changing this corrupt and nepotistic department, and I left my of changing this corrupt and nepotistic department, and I left my position in 1996, after having fought to the bitter end without giving way one single millimeter.

When I was head of the ecology group in 1995-1996 J¿rgen Rab¿l, who had a position as associate professor, habitually accused internationally renowned scientists of fabricating data during our weekly seminars, without ever presenting any evidence. Prof. W. Wiltschko, Prof. L. W. Simmons and Prof. D. Gwynne were among a long list of scientists being accused without any evidence whatsoever. Eventually I asked J¿rgen Rab¿l not to attend the seminars because his behavior was beyond what I considered acceptable for a staff member at a student forum. J¿rgen Rab¿l had equally disruptive behavior for a normal scientific environment in many other contexts, and when I discussed issues of research ethics with students during seminars, he not once contributed in a constructive way to these discussions. All this can be verified by several of my former students and post-docs who were present during these seminars.

The paper that was the basis for J¿rgen Rab¿ls accusations was published in Oikos in 1998. With all due respect, Oikos is a low to medium ranked ecology journal with an impact factor of 2.4. I have published 33 papers in that journal since 1987, and I will claim that I can publish in Oikos whenever I want. I would just have to submit a good paper. To put the accusations in perspective I have recorded the impact factors of papers published by fraudulent scientists as reported by Nature and Science during the last 20 years. The mean current impact factor of the journals that published fraudulent papers by these scientists was 28 (SD = 2.1). Thus, the difference in impact factor between the journals that published these papers and the paper being the object of the present case against me is (28.0 Ð 2.4) / 2.1 = 12.2 standard deviations. The probability of this happening by chance is less than 1 in 100 million. This should assure even the most skeptical scientist that this case is not an ÔordinaryÕ case of scientific misconduct.

I have during my career published scientific papers with over 180 co-authors with whom I have shared data files. Since the decision by the Danish tribunal at the end of 2003 I have published 83 papers with 142 co-authors. Although I have not been lead author of all of them, I have contributed significantly to all papers. Claiming that I am a pariah, as done by Brendan Borrell, is thus way beyond any evidence and fair judgment of available evidence. On the contrary, I doubt that any other biologist has published with so many colleagues during this period of four years.

The Danish case raised against me appeared after I had filed complaints with the Chancellor of the University of Copenhagen concerning cases of nepotistic appointments for jobs at the university, and a number of other cases of misconduct that on some occasions were on the borderline of criminal acts, and a number of other events that went beyond what I have ever experienced anywhere in the world during my 25 yearsÕ scientific career. Already Shakespeare wrote in Hamlet that Òthere is something rotten in the Kingdom of DenmarkÓ, and that is unfortunately still the case.

The case raised by J¿rgen Rab¿l against me had a number of peculiarities that have never been explained. For example, the only evidence presented in the case was a computer file that was only evidence presented in the case was a computer file that was in the hands of the accuser for over 10 months before a security copy was ever produced. That is not how evidence is treated in ordinary courts in democratic countries.

No committee members that investigated the case had any knowledge of the subject, and only one was a biologist. He was a friend of a scientist that had a nepotistic appointment at the department in Copenhagen. The scientist who was member of the committee also lied about his connections with me during the interview before he was appointed. As stated later by one Canadian post-doc, who was working at the department during the course of the case against me, there was no way that I would ever have a fair and impartial trial because everybody was on the hunt to get me convicted independent of the evidence.

The case breached the human rights convention in a number of ways. First, I was never given a fair and public hearing by an independent and impartial tribunal (thereby breaching article 6.1 of the European Convention for the Protection of Human Rights) because the hearings by the tribunal were closed, even to me. A prominent member of an ad hoc committee was not impartial with respect to the case. Second, I was never allowed to defend myself in person or through legal assistance (thereby breaching article 6.3.c of the European Convention for the Protection of Human Rights) because I was ever only allowed to respond to the tribunal in writing. Finally, I was never given the right to have my case reviewed by a higher impartial tribunal (thereby breaching protocol no. 7, article 2.1 of the European Convention for the Protection of Human Rights). The only instance of complaint was a bureau chief of the same ministry that was responsible for the tribunal, and that person only considered procedural issues while any complaints about the factual issues were not even considered.

I asked the committee investigating the case why the biological material that formed the basis for the original paper was never re-measured, because measurements by a third, independent party would resolve the case once and for all. Were the measurements made by the technician appropriate? Did the measurements allow reconstruction of the results in the original publication? The committee answered my request by stating that such an exercise would constitute new research and thus could not be approved!

I have in my book on asymmetry, co-authored with Dr. J. P. Swaddle been accused of plagiarism. However, there is absolutely no evidence of plagiarism. In the section of dispute I fully attributed a number of statements at the proof stage to the original source. Unfortunately, these changes in the proofs were never corrected by the copy editor at Oxford University Press. Because authors are not provided with a second set of proofs that allow them to check if requested changes have been made, I had no opportunity to make these checks. These statements can be confirmed by the publisher which still has the corrected proofs. I and my co-author apologized to the scientist affected by this omission by the publisher and we also posted an apology at the web site of the book. The scientist concerned fully accepted our apology in an email to us. Therefore, there is absolutely no basis for claims about plagiarism.

Finally, the Danish committee convicted me of being responsible for scientific misconduct in November 2003, but did not meter out any punishment. I and my co-author had already retracted the any punishment. I and my co-author had already retracted the paper even before the case started. Subsequently I have been subject to a behind-the-scenes prosecution that is akin to a witch hunt. My former students and friends have on at least five occasions been threatened by J¿rgen Rab¿l and his friends either by phone or email to force them to stop collaborating with me. None of them has bowed to these threats, and all have assured me that this only confirms their suspicions and strengthens our ties. J¿rgen Rab¿l has also contacted my former head of department attempting to convince him of firing me. Again, this failed miserably because my former boss submitted the correspondence as evidence of harassment to the French natural sciences research council (CNRS). J¿rgen Rab¿l has worked hard to have my ringing license revoked after 35 years of volunteer bird ringing and scientific research. The Ringing Center at the University of Copenhagen received hundreds of complaints about this arbitrary and unjustified act. It is of great consolation to me that several of my colleagues from Eastern Europe have told that they fully support me and my cause because the present case reminds them so much of what happened in their countries during communist times, when somebody who stood up against corruption and nepotism and was subsequently singled out as the target of prosecution.

---

comment:

P.S.

by robert triv4rs

[Comment posted 2007-02-18 19:24:02]
In my comment on this subject, i should have said that it was Rich Palmer himself who quickly spotted his error in reading my dance paper--turning his own critical faculties on himself, as good scientists should. I apologize for leaving any other impression.

---

comment:

Something rotten in the mind of Møller?

by Jørgen Rabøl

[Comment posted 2007-02-19 22:49:12]
Møller was convicted for the fabricated data he personally sent to the Danish Committee. The data file which opened the case first in OIKOS and later on in Denmark had - as emphasized by the Danish Committee - no influence on the conviction/decision. According to Møller both these files were fabricated by the drunken technician - and me? who is considered the revengeful and envious former colleague of Møller. Later on the data files were stolen from the computer of Møller, and the oak leaves disappeared from the house of his parent in northernmost, most rotten part of Denmark in 2001. Furthermore one of the members of the Danish Committee was against the science of Møller for political reasons and the release agent in a serious tragedy in the Møllerian family.
French CNRS bought all these weird scenarios, and Møller still fabricates lot of science fiction papers in the important peer reviewed scientific journals.
In his comment there is a good example of such science fiction. On the basis on impact factors he calculates the probability of his OIKOS paper being fradulent as less than 1 in 100 millions. Or perhaps I misunderstood something, and he was only joking.

perhaps I misunderstood something, and he was only joking. People interested in a more balanced presentation of the facts in the Møller case should consult www.jorgenrabol.dk under the headings Scientific Misconduct and Overview. An updated version of the latter will probably be available within a few days.

---

comment:

The Peacock's feathers and proper quotation

by Fred E. Indig, Ph.D.

[Comment posted 2007-02-20 16:07:11]
Dear Editor,

I read with interest your article, "a Fluctuating Reality", about the controversial work of A.P. Moller. On p.30 you explain his ideas, giving the impression that he originated the theory that the tail feathers of the peacock show his overall fitness, and so influencing sexual selection. Actually, this is known as the Handicap Principle and was originated by Amotz Zahavi over thirty (30) years ago, see Zahavi, A. (1975) Mate selection - a selection for a handicap. Journal of Theoretical Biology 53: 205-214.

In an issue devoted to ethics I would expect proper attribution of intellectual ideas.

Sincerely,
--
Fred E. Indig, Ph.D.
Head, Confocal Imaging Unit

---

comment:

Is Anders Pape Møller unable to produce scientific fraud??

by Jørgen Rabøl

[Comment posted 2007-02-22 12:11:23]
I have carefully read and re-read the comment of Anders Pape Møller (APM). Even after seven years of combat and many letters and statements from APM I still discover new aspects of his remarkable mentality!

Trivers in his comment states that it is all right if a genius like APM fabricates. He says that the important point is i) whether APM is right, or will be right in future, and ii) if a lot of paper-production and social interactions from and between scientists emerge in his foot-steps. Trivers is wrong, but his way of thinking from "the Crawford Price layer" just beneath Darwin and the Good Lord is understandable, friendly, reconciling - and a little stupid.

APM in his "impact factor calculation" goes further – and much further – than Trivers. APM PROVES by means of a statistical test, that the probability of his OIKOS paper being fraudulent science is less than 1 in 100 million. This is pure nonsense from a black hole, but in the inverted mind of APM it is a legal consideration and calculation. For people only knowing the famous professor from the distance his comment probably appears unpleasantly violent, hysterical and bragging. Also his persistent rage about what he considers unjustified prosecution by various (mostly Danish) persons and institutions leaves the impression of a person on the persons and institutions leaves the impression of a person on the border of normal behavior. However, even his friends probably now realize that this border has been significantly crossed with his "impact factor calculation".

APM considers himself as hovering high above the ground of ordinary scientists and scientific misconduct. His massive paper production and many co-authors and friends constitute the ultimate proof that he surely cannot be guilty in data fabrications. Or if he did something that looked like data fabrications he was in his good and divine right to do it, because it was a necessary step to restore order and harmony.

Finally two comments:
i) APM states that I contacted his former head "attempting to convince him of firing me". I had several contacts to Jean Clobert but according to my emails I never proposed that. However, of course French CNRS should fire APM.
ii) I never worked hard to have the Danish ringing license of APM revoked. I never worked for that at all. However, I understand why a ringing center taking its work seriously cannot co-operate with a person appreciated for and convicted of data fabrications. What I do not understand is that a lot of scientific journals still publish papers based on data delivered by APM.