

Author's Note from *The Emperor of Scent*

Pages 227-239 from Burr, Chandler, (2002) *The Emperor of Scent: A story of perfume, obsession, and the last mystery of the senses*, Random House

**Comment by P.W. Warren:** Michael Grosso's "Hume's Syndrome: Irrational Resistance to the Paranormal," Journal of Scientific Exploration, 2008, Winter, v. 22, #5, pp 549-556 provides a history of pathological skepticism which apparently became strong with the "Age of Enlightenment" and the back lash to the power of the church. Pathological skepticism is alive and well in current science and is not confined to the paranormal as this "Author's note" by Chandler Burr demonstrates. It's a startling and depressing example of "Hume's syndrome," defined as "a state of involuntary negative hallucination with regard to seeing or acknowledging facts that appear to disrupt one's cherished worldview." As in the age of enlightenment, those now so afflicted are educated, smart or even brilliant, esteemed with prestigious positions and honours. They just go off their nut when their nut buttons are pushed. As Burr states the situation, he discovered a "**complex story of scientific corruption, corruption in the most mundane and systemic and virulent and sadly human sense of jealousy and calcified minds and vested interests. That it was a scientific morality tale.**"

Many people have the hallucinatory image of scientists and academicians as noble and courageous knights bravely riding white steeds of reason and wielding the weapons of methodology and self correction to cut down superstition, error, fear and suppression so that beautiful truth may shine forth to enlighten us. Should the reader have this image he need only read the following excerpt which dramatically illustrates the extent of the pathology of canonical science. They treat their theory and model as sacrosanct and any who dare question it are thus heretics and dealt with in the contemporary inquisitional manner. Canonical science (Scientism) doesn't literally burn scientific heretics at the stake. However, time and again, they excommunicate them, declare their work blasphemous and use peer reviews, the scientific version of the Catholic Church's "Congregation of the Doctrine of the Faith" whose function is validate "true-canonical beliefs" and thus prevent "untrue-heretical beliefs" from being disseminated.

It's time to awaken from single vision and CSICOP's\* sleep (apologies to William Blake) and bring the Weltansicht of mainstream (canonical) science, especially psychology, biology and medicine into the 20th if not the 21st century! Unfortunately, too much of science is now closer to absolutism and the religion of "scientism" in it's worship of theory. Too many modern universities are like their medieval counterparts in that they preserve the past and support the current

*power systems (the government/military/industrial/corporate/ religious/education complex to expand Eisenhower's warning) rather than provide a critique of them.*

\*CSICOP = *Committee for the Scientific Investigation of Claims Of the Paranormal* which see themselves as the defenders of the faith and religion of Scientism. Robert Anton Wilson [(1986/1991) *The New Inquisition: Irrational Rationalism and the Citadel of Science*, New Falcon Pubs] says the acronym should more accurately stand for *Committee for Slander, Invective and Calumny of Open-minded People*. Indeed, they function as the religion of Scientism's version of the Catholic Church's "Congregation of the Doctrine of the Faith" whose function is to silence dissenters.

*Burr's analysis of this sorry affair between those who believe that odor depends on molecular Shape, the Shapists, and Turin, the Vibrationists school. Below is the beginning of the passage quoted. [brackets indicate material added by P.W.Warren]*

You may have noticed by now a strangeness, a journalistic imbalance, in this account. It comes from something I myself noticed only gradually.

The default of the serious reporter -- it's certainly mine -- is to present both sides of a story. You generally realize at some point, at once indefinable and crystal clear, past the initial excitement but short of true understanding, that you've stumbled into a story. I happened to meet Luca Turin because the Eurostar from Paris to London was twenty minutes late. We were in line in the Gare du Nord, and we started talking about the finale of *Mission: Impossible*. Where they fly some helicopter into the Eurostar tunnel? "Oh yeah, sure," said Turin, "although of course that's physically impossible." Really? "Problem with air mass." How's that? He explained it, using his hands. So, I said, he was a physicist then? "Biophysicist." What was he researching? "Smell. And vibration." As we got on the train, I asked for details. The point arrived just before Waterloo.

So you scout out the view of the side through which you stumbled into the story (because you inevitably enter through one side's point of view), understand it thoroughly, learn its arguments, and then you decamp for an equally thorough stay in the enemy lands on the other side and *those* opinions. After that, you go away and synthesize a-balanced presentation from the two sides.

It is therefore disorienting, almost surreal, to enter a story through one side and then, gradually -- I thought I must be imagining things -- find that that side is *so loathed by its opponents, so vilified by and toxic to them, and so axiomatically unworthy in their estimation of serious consideration that they actually refuse to share their own view with you. Or are so unremittingly hostile that their attempts to do so fail to qualify as "thoughtful," which amounts to the same thing. Or proffer a critique so tendentious as to be substantively worthless.* [emphasis added] Which leaves you, as a journalist, in the bizarre and infuriating position of knowing, or assuming, that there's a second, legitimate side but having effectively no access to it. I began this book as the simple story of the creation of a scientific theory. But I continued it with the growing awareness that it was, in fact, a larger, *complex story of scientific corruption, corruption in the*

***most mundane and systemic and virulent and sadly human sense of jealousy and calcified minds and vested interests. That it was a scientific morality tale.*** [emphasis added]

*The response to Luca Turin's controversial theory embodies the failure of the scientific process, both as an ideal and in the day-to-day functioning of prestigious scientific organizations like Nature or, for that matter, the giant research mills at the Big Boys* [in the field of smell and perfume; emphasis added]. Turin once grimly opined to me that the reason Geoff Webster, CEO of huge Givaudan Roure [perfume company], couldn't rationally evaluate Vibration -- and thus Givaudan's true corporate interests (I met Webster at a Fragonard perfume launch in Paris, and he insisted to a group of us that smell was definitely shape) -- was not Webster's fault but the fact that "he has to listen to some Shape-chemist dwarf in Zurich with an axe to grind." I discounted this as hyperbole. I wound up agreeing with it as analysis.

Start with the fact the most objective, thorough, scientific criticism Turin's theory has gotten, the one that normally I would use as the journalistic foundation for presenting Shape's point of view to you, is contained in the four peer reviews from *Nature* magazine, on the basis of which *Nature* rejected Turin's paper. Reviewers 1, 2, 3, and 4 remain anonymous. The scientists that Turin and Stewart identified by deduction -- Dodd, Hamm, Shepherd, and Hopfield -- deny authorship, as they would arguably in any case be obligated to were they in fact the authors (such is the system), and *Nature* naturally will not divulge that information. Because of the way scientific peer review is set up, this meant I couldn't ask Referees 1 through 4 anything. All I had to work with were the reviews themselves. *The problem is, the reviews are incoherent.* [emphasis added]

I first went over them in detail with three scientists in the Turin camp, whom I knew and whose expertise covers the three fiefdoms inhabited by the paper [of Turin], chemistry, physics, and biology: chemist Walter Stewart, physicist Marshall Stoneham, and biologist Mark Dearborn, all esteemed in their fields. Their analysis ultimately agreed with my own, which is to say that as far I can tell (1) Turin's vibration paper's respective chemistry, physics, and biology are entirely scientifically viable as such and (2) the *Nature* peer reviewers' comments -- as well as those of Philip Campbell, *Nature*'s editor in chief -- relied on patent tautological criticism. To reiterate their gist: the biologists said the chemistry was wrong, the chemists said the problem was the physics, and the physicists said the fault lay with the biology. *This is not to mention the casual, obvious, often grotesque inaccuracies filling their readings.* [emphasis added]

I incidentally note for the record that I have, by no means, always seen eye to eye with Turin during the process of reporting this story. I told him when we reached Waterloo that I wanted to write about him, a prospect he greeted with pleasure. It was a friendly beginning to a professional relationship, and during the four subsequent years of reporting, the relationship almost ended several times. Turin at various moments told me with fury that I knew nothing about science, rejected my assessments of him personally and professionally, demanded "total control" of the manuscript "so that," he said, "if I don't like a single word in there it doesn't get published," and threatened to withdraw his cooperation entirely from the project. He doesn't like or agree with all the opinions I've expressed here [in the book], nor with all my characterizations,

particularly of him. Turin's partisans, notably the scientist I've called Mark Dearborn (who was by turns difficult, negative, patently hypocritical when he felt it served his purposes, and "completely paranoid," his own descriptive of himself and one I fully agree with), at times actively warned Turin not to cooperate with me and withheld information. Walter Stewart took half a year to return my e-mails requesting an interview. This hardly endeared them to me.

But in the end what matters is the data, which is to say the facts as far as they are discernible. The Vibrationists gave me data. Had the Shapists given me, or had I uncovered, plausible counter-data demonstrating that Turin's findings were flawed, or could be explained away, or were factually inaccurate in favor of Shape, I could not have written the book. I tried, repeatedly, to go over the *Nature* peer reviews with the Shapists. There turned out to be simply no *there* there.

Initially, there was the Shapists' behavior. If you're familiar with the field, you have already noticed that the opinions of several of its major scientists are absent here. To give a taste of why this is so: I called one of the most highly respected senior olfaction researchers, someone I'd been assured would be an able intellectual match for Turin, at his prestigious New England university. The instant I identified the call as concerning Turin's theory, he immediately said, "This is off the record" (Why? "That's my business"), which meant that I could print none of it. He then launched into a diatribe against Turin's paper. It consisted of weak criticisms I had already heard countered convincingly by the Turin camp, one of which was answered by a graphic in the *Chemical Senses* paper [1996, "A spectroscopic mechanism for primary olfactory reception," *Chemical Senses*, Vol 21/ 6 pp 773-791]. I pointed that out.

"I haven't seen that graphic," he snapped.

I paused; the graphic is central to the paper. I asked, You have read the paper, haven't you? "I don't need to read the paper," he said sharply. Well, I asked, would you please read the paper and then speak with me about it? "I don't have time!" he said, "I'm busy." I started to say something else, but he hung up. This is one of the world's preeminent Shape scientists. *I cannot recall previously having come across a professional willing to attack, on or off the record, a paper that he had, in fact, not read; virtually every Shapist I attempted to interview did exactly this.* [emphasis added]

It was, at least, an education in human nature. In Coorg, India -- that this story is coming up -- a group of us took a break from the smell conference we were there for and went on a short hike in the jungle. I caught up with a molecular biologist I'd been hoping to talk to. (We'd exchanged maybe four or five pleasantries up to that point.) I opened politely with the statement: I'd like to get your opinion on Luca Turin's theory." Her face iced over instantly. "What about it?" I said I wanted her scientific assessment of it. "You're writing about this?" I'm writing, I said, about Turin's Vibrational theory, yes. She looked directly at me and spoke crisply. "So basically, if he's right, he's a genius and we're all assholes working on garbage. Right?" She turned her back to me. She never spoke to me again about the theory, nor did she ever provide me with a scientific critique. She was not unique. I never got used to it.

The fact is that the neurobiology she and others are working on is not garbage. But she's not stupid. If Turin turns out to be correct, the Nobel for smell -- and everyone is betting there will be one -- will almost certainly be awarded for the discovery of this astounding biological mechanism rather than for describing the system's wiring and plumbing, the receptors and neurons. In the sense of winning a *Nobel*, if he is right, she is indeed wasting her time. That's the way the scientific rewards system goes. To refuse to discuss the theory's scientific merits is another proposition entirely.

What signifies in the end is *the lack of Shapist scientific ammunition* [emphasis added]. The battle between Shape and Vibration is not over, and I categorically do not (and Turin, equally, does not) suggest it is. Though Turin has provided fascinating and convincing preliminary evidence, there of course has to be independent confirmation by other labs before Vibration is accepted. However, if the question in this story is "Should Luca Turin's theory, as any such theory seriously propounded and regardless of whose vested interests might be threatened, have been given fair and serious consideration by the scientific community?" or, more pointedly, "Was *Nature*, in relying on ego and emotion and incompetent analysis, professionally remiss in not publishing Turin's paper?" then the answer seems to be clearly yes. It isn't merely that Shapist data was always transparently weak. (One Shapist told me that the metallocene experiment Turin had put in his paper was worthless because the nickel fattens up the molecule more than the iron, and the receptors could feel this difference in Shape. I took this to Turin, who pointed out that background thermal movement constantly changes the molecule's shape ten times more than the nickel does. I then took this back to the Shapist, who, literally, changed the subject.) But as I say, it isn't merely the weak data. It comes down to something even more basic: *almost none of the critics of Turin's Vibration paper actually read it.* [emphasis added]

When I reached G-protein expert Heidi Hamm on the phone in September 2000, she explained at length that Turin's paper was wrong. So I asked her if she had read the paper. No, she said, she had not. I asked her if she would please read it. Irritated, she said that she would not, she was busy. I noted that criticizing a paper one hasn't read is considered tricky, factually speaking. Her response was that she didn't need to read it since it was wrong on its face. She then ended the conversation.

I called Randall Reed, one of the biggest olfaction scientists. First, he commented ad hominem of Turin, "Well, some people thrive on controversy" and said that he, Reed, didn't believe in Vibration. But he didn't believe in Shape either. He wrote off Strong Shape and then, immediately, Weak Shape: "Linda Buck believes we feel parts of the molecule," but this, he said, couldn't work. Why? Simple, he explained. The Weak Shape theory ("feel parts of the molecule") is the three blind men feeling the elephant, and whatever part each one feels is added to the others and -- voila, that specific combination is the smell. This can't work for the completely obvious reason that if you say OK, there's a receptor for the specific shape of aldehydes, for straight long-chain carbons, and for alcohols, you have a problem: mixtures. Mix a four-carbon alcohol with an eight-carbon aldehyde, and then mix an eight-carbon alcohol and a four-carbon aldehyde; both of them have an alcohol group, an aldehyde group, a four-carbon chain, and an eight-carbon chain -- and there would be absolutely no way you could tell those

apart by their shape. They would have to smell exactly the same. And in fact, said Reed, they smell completely different. Ergo, Weak Shape doesn't work.

(I asked him what they smelled like, but he laughed uncomfortably and said, "Well, you'd have to ask a perfumer.")

"I sort of don't know," said Reed, "what the critical test of the Turin hypothesis would be, really." I suggested to him that the critical test would be simple: finding two differently shaped molecules with the same vibration and asking, "Do they smell the same?"

The following then happened. Reed said that, first, he didn't know how Turin calculated vibrational numbers, a strange comment given that the method of their calculation is standard. Then he asked how Turin defined "what is the same vibration," which was even stranger; again, the calculation method is standard and objective. (This is like asking how someone would define the frequency of a photon vibrating at 483 nanometers. The answer, of course, is 483 nanometers.)

"Also," Reed said, "I'd ask Turin this: How can you have two molecules that have different atoms but similar shapes? If they have different atoms, wouldn't they have different shapes automatically?" This is a question of basic chemistry, but Reed is a biologist, not a chemist, and -- and this is not at all rare-- apparently was unaware that there are examples everywhere of same-shaped molecules with different atoms inside them.

He then said, "I'd also ask him: When it comes to smells, what is a 'very similar smell'?" Here was, once again, a smell scientist simply not believing that mint smells like mint, which is to say not believing in the objective reality of smell. He ended by saying, "Buck believes in Weak Shape. Turin would go the other way and say Vibration. I fall in the middle. I believe a given molecule may fit a number of receptors, that the molecule puts its hands all over the odorant, it feels every part of its shape, but it's not always a perfect fit." Which was to say: a) I don't know how smell works, but b) I think it's Weak Shape, which c) I just disproved. That was my conversation with Dr. Randall Reed.

When I called Linda Buck, I started by asking her about Turin's theory, and she immediately said, in a very serious, guarded voice, "I would be very, very careful. This theory was around in the 1950s and was disproved."

But inelastic electron tunneling [an physics aspect of Turin's theory] was not around in the 1950s and could not have been disproved.

"I was," she says, "pretty upset about the *BBC* program [called "A code in the nose" aired late 1995 on the television science series called *Horizon*] because they didn't tell me that it was going to be promoting this theory to its audience. There is, as far as I'm concerned, no evidence for this theory at all. It's all based on a few correlations. It's imaginary. From what I understand, people who understand the physics part of this theory don't think it makes any sense either."

I responded that UCL's [University College London] Marshall Stoneham and C. J. Adkins of Cambridge, among other physicists, have spent quite a bit of time on it and find the physics solid. Buck seemed not to hear. "I was just reading about this group that doesn't believe in evolution," she said, "and they think that aliens put us here on earth. You can't refute it absolutely, but I doubt it's true. This isn't a theory of olfaction. This is an entire theory of biology itself." [which is true; see the work of Ma-Wan Ho, Fritz Popp, Bruce Lipton, James Oschman, Stewart R. Hameroff, M. Sue Benford. Edgar Mitchell, Karl Pribram, Dejan Raković plus many others of the Belgrade International Anti-Stress Center, and many physicists and biophysicists among others. Very few psycho-biologists however. Pity!]

Buck is phenomenally knowledgeable about the smell field. She talks with precision about all sorts of things, the molecular biology, pseudogenes; she cites all the big names in smell, Eric Moyers and Anne Houston and Stuart Firestein. And it's all very interesting, but none of what Buck talks about addresses the central question: How do we smell? What exactly is the smell receptor recognizing? Ask her directly, and she gives a categorical answer: "The shape." How? Again, categorical: "Weak Shape." Yet when she heard Randall Reed's quote about her believing in Weak Shape, she appeared, oddly, to change her mind. She laughed, not happily. "I don't know why Randy would say that because that's not what I think." Yes, she admitted, she once threw out a suggestion that the receptors were fixing on several different shapes of a single smelly molecule. "But we won't know exactly what they're feeling until the structural biologists tell us," she said, adding, "I don't really hold to the [Weak-Shape] theory." Which was to say that she didn't know how the receptors measured the shape, but she knew that they measured shape.

Well, then ... Turin? "I've never met Turin." Grudgingly but honestly: "I've heard he's a bright guy." Hardening a bit; "I mean, if there were a compelling argument, I'd certainly listen to it, but I haven't heard one."

But what, I asked, did she think of the arguments Turin presents in the paper? [1996, "A spectroscopic mechanism for primary olfactory reception", *Chemical Senses*, Vol 21/ 6 pp773-791]

Buck said evenly: "I haven't read the paper." Which meant, I pointed out, that she had not read the argument of the theory that she believes "imaginary." She responded (not pleasantly) that she had "heard a lot about it." She said several times that she'd "heard a lot about it."

OK. What if I sent her the paper to read? She demurred tersely: "No. Thank you."

Well, would she read it on the Web? I started to give her the paper's URL [[http://www.flexitral.com/research/chemical\\_senses\\_complete.pdf](http://www.flexitral.com/research/chemical_senses_complete.pdf)]. She cut this off. She was busy. She had other things she had to read.

But, I said, she'd stated that if there were a compelling argument she'd certainly listen to it.

She laughed rather irritably and said, "Well, I'm writing a grant."

Then she said good-bye and hung up the phone.

I called Ken Palmer, Charles Sell's lab tech at *Quest* [perfume company] who had performed the isotope experiment and had confirmed that the two acetophenone molecules (same shape) smelled different. I needed to fact-check Turin's account of the episode, and Palmer confirmed it over the phone. A day later, on July 15, 1999, I received an e-mail from Palmer's boss, Charles Sell. Sell conveyed Ken's "uneasiness" about my citing him as an expert "since he has to deal with perfumers every day." When I told Turin about this, his concise comment was "Please, the perfumers don't give a shit about this," a conclusion I'd immediately reached on my own. Palmer didn't speak to me again.

But the rest of Sell's e-mail illustrates perfectly the quality of Shapist arguments. He went on to explain his underwhelmed reaction the day that Turin and Palmer laid the acetophenone-isotope affair on his desk. "Yeah," he said, "I know Luca thinks the acetopheenone experiment we did for him proves his theory. But it doesn't." (Why not?) Simple, said Sell: "There could be other explanations." See, the vibrations Turin was getting by hitting these two molecules with infrared light was not, Sell wrote me, the only thing different between them. Hit them with nmr (nuclear magnetic resonance) spectra instead, and you'll get an even bigger difference in vibration. (Hm!) "As far as odour is concerned," wrote Sell, "take out Hydrogens, put in Deuteriums, and you change the thing's chemical properties." Replacing all the Hs with Ds "will change the rate at which it will undergo keto-enol tautomerism," as well as affecting the desire of lone pairs on the oxygen toward hydrogen bonding. Any of this could make the receptor smell the acetophenone differently.

At first blush this sounds like a legitimate scientific argument. But then you realize ... that, scientifically speaking, you can *always* come up with other explanations; the point is coming up with one, that fits the data. When you actually looked at what Sell was saying, you found an immense amount of ... nothing. Or more precisely, everything, an infinitely expanding number of infinite possibilities. Sell had proposed that nuclear magnetic resonance (NMR) might "possibly contribute" to a difference in smell, and Turin's succinct, and logical, reply was "If Charles can prove to me that we do NMR spectrometry with our nose, I will carry his luggage to Stockholm to pick up his Nobel. That's the NMR Theory of Smell, and Charles is welcome to propose it, but this is the first I've heard of it. Why stop at NMR? If you shine a neutron beam at deuterated acetopheenone, it'll be absorbed more readily than nondeuterated. There: that's Turin's Neutron-Beam Theory of Smell! Great! I'm happy with that! What does it mean? Who knows."

It wasn't that Sell was wrong. It was that if one followed his logic, which was to cite every possible reason, one would end up arguing that smell could have been installed in our heads by the aliens who put us here on earth. Why not? Turin replaced Hydrogen with Deuterium in the molecule acetophenone, which had the same shape, and yet he and Palmer found a *difference* in smell. Sell argued that it wasn't Vibration by noting that changing Hydrogens for Deuteriums "changes chemical properties," which, he proposed, may change the smell chemically. OK. But

he failed to note one fact: you can take bacteria, replace every Hydrogen atom in their microscopic bodies with Deuterium, and they'll live perfectly normally. Which is rather astounding and which dictates that any difference in either shape or chemistry in these isotopes will range from infinitesimal to nonexistent (while the smells, notably, are different). Sell's logic, more to the point, prompts one to ask, So, what's *your* theory of smell then? And that, for Turin, is exactly the problem.

Charles makes all these criticisms, criticisms that imply alternative theories, but then never bothers to follow through on any of them. I was at *Quest* [perfume company] giving a lecture and told the group about ferrocene and nickelocene. Charles said, "Oh, but they have different electrochemical behavior." Which means that if you made batteries out of ferrocene and nickelocene the voltages on the outputs would be different because they have a different electron affinity. Well, fine. True. We all know that. AND??? *How does that relate to smell?* Because if smell turns out to be not Shape, and not Vibration, but Electron Affinity, then that's fascinating! But ah! -- Charles doesn't have any evidence for the Electron Affinity theory of smell. All his alternatives to Vibration are different, and none are linked, theoretically or experimentally, to any of the others. Which is called desperation. So I said, "I usually say that Shapists don't have enough of a theory, but the problem with you, Charles, is that you have too many." He just straightened his spine in his seat while everyone laughed. No wonder he loves me.

Turin's personality certainly played a role in the reception of his science -- a problematic one. It is a difficult personality, not at all by intent (entirely despite it, actually; he is completely well intentioned) but simply by nature. Under all normal circumstances, and when he is taken seriously scientifically, he is delightful, articulate, cheerful. People perceive him as intimidating, or arrogant, or combative, or insensitive because he is also these, in the blink of an eye, when contradicted or blocked. His fury at his colleagues' refusal to examine his work -- Why, he asked, didn't they ever invite him to their big smell conferences in Boston and Los Angeles, the cozy colloquies where they got together to listen to one another? -- comes out in bitter sarcasm. Also in recalcitrance, Turin is simply incapable of playing by someone else's rules if he doesn't see their use. Which then compounds their refusal. But *the field's unwillingness to examine his data is, as far as I can see, ultimately based, entirely, on vested self-interest and bad science.* [emphasis added] And the data, in the end, is what counts. Every once in a while, accidentally, they showed their hand. I was asking Tim Pearce, an engineer at *Leicesster University* who builds artificial noses (primitive machines that use conductive polymers to trap and detect molecules), about the refusal to look at Turin's theory. He responded calmly that it was "because there's a history involved." He rather clearly meant: We've been slaving away, and suddenly he just waltzes in here? "There've been over a hundred years of study in smell, and when somebody comes along with no history in the subject and then suddenly announces that there's a solution to all of this that everybody's overlooked, there's bound to be a reaction."

Pearce added, "Turin's been talking about this for a long time, but I don't think any data have been published."

I pointed out that in fact Turin had published a paper, in *Chemical Senses*. [1996, "A spectroscopic mechanism for primary olfactory reception", *Chemical Senses*, Vol 21/ 6 pp773-791]

Pearce shrugged and said, "As far as I know, it's very thin on data." But he hadn't read it.

I placed my first call requesting an interview to Dr. Richard Axie's lab on Thursday, August 20, 1998. There was no response. I left another message on Monday, August 24, 1998. Nothing. When I mentioned much later to one prominent olfactory scientist that I'd tried five or six times with no result, he snapped, "Richard never returns anyone's calls" and looked away.

Sir Aron Klug of the Royal Society formally declined to be interviewed, twice. As for Nick Short at *Nature*, I began phoning him in the spring of 1998. Since Mr. Short was always in meetings, or away from the office, I would leave messages from New York with polite voices in London -- my number, my name, and the name of *The Atlantic*, for whom I was working on an article on Turin. Yes, they would give Mr. Short the message. Yes, Mr. Short would return the call. Mr. Short never returned a call. Not one. I began writing this book.