

An occasional column, in which Caveman and other troglodytes involved in cell science emerge to share their views on various aspects of life-science research. Messages for Caveman and other contributors can be left at caveman@biologists.com. Any correspondence may be published in forthcoming issues.

Journal of Cell Science 115, 237-238 (2002)
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When the hunter becomes the hunted

You'll have to forgive me, but I am a little out of breath. I am, as you might recall, the hunter-gatherer for my clan. I had followed an apparently docile herbivore, at least that is what I assumed him to be, for several kilometers, trying to keep downwind. I had already worked out how to tackle him - a large stone to stun the beast and then my trusty flint knife to finish him off. I had done this many times, nothing to it. But, just as I was within range to heft the stone, the wind changed. The beast turned slowly and looked at me. The look was not surprise or terror, or even offense at my smell, which was worse than his. No, it was more of a smirk - a sort of 'now what do we have here?' look. Suddenly, the proverbial table was turned; the hunter had become the hunted. I turned and ran. I don't know when I lost him, but I didn't stop to look until I had made it all the way back to the cave. Frankly, the clan has been very unsympathetic. My story was received with a lot of skepticism, especially the part about the size, speed and aggressiveness of the

beast. My reputation for being able to distinguish herbivores from other beasts has taken a severe beating, and I have become the butt of jokes about my odoriferous tracking skills and my ability to turn tail and run at the slightest sign of trouble.

You may be wondering, where is Caveman going with this particular piece of fantasy: where is the relevance, the pithy insight? Well, I have been ruminating on the duality of our scholarly endeavors to publish in journals and on the hunters and the hunted.

In a real way, scientists are hunters: you scour the land for new ideas, plot an attack on an idea, marshal forces to bring to the impending battle, use different approaches to probe the idea, attack and finally capture it and call it yours. The culmination of all of this is to publish the history of one's battle. Now you frame the work in the context of the field (the big advance, the unexpected insight, the reinterpretation of results of others in the field), marshal the data and spruce up the representative gels and pictures into

figures. Now you can tell your tale, rewrite the field, extol the importance of your work and show your competitors how good you are. In the writing of the work, you must steal yourself to be critical. Ah, but it is so difficult when it is your own work. It is easy to make bold statements about the data - after all you have thought for a long time about their design - and go for the positive interpretation and downplay, if not just ignore, the alternative explanation. You construct the paper as a story, careful to lead the reader through the labyrinth of data and down the garden path to enlightenment. You try to be helpful by finishing each set of data with your interpretation and then starting the next section with a sub-heading that is a declarative statement of the interpretation of the next batch of experiments.

Exhilarated with the completed paper, you submit it to an august journal, sure in the knowledge that it will be received with praise and adulation by your colleagues.

But as the days become weeks as you wait for the reviews, your feeling of invincibility begins to weaken, your cocky manner regarding the work is

gradually whittled away. Those experiments in Figure 3 lacked a control that at the time did not seem so critical; you should have performed the additional experiments, and perhaps you did rather overstate the interpretation of the work and the fact that no one had thought of this interpretation. Why had you decided to use the word 'discovery' (five times)? Why had you downplayed the work of several colleagues who had done similar experiments several years ago? Now the ghosts in the field have taken to the air. They are circling your lab, their knives are out and you are the target. Yes, you, the hunter, have become the hunted.

The hunters are looking for an easy kill - the slow-witted herbivore of a paper lacking critical controls and containing weak representative data and over-interpreted results - their hunger sharpened by the insulting manner in which the paper is written and their bloodlust heightened by your dismissal of the work of others and the overbearing arrogance of your 'new discoveries'.

The kill is made: the rejection letter and reviews arrive. One of the hunters

dispatches you cleanly by simply stating that the work is overinterpreted, lacks controls and does not advance the field. Meanwhile, the others butcher the paper - long paragraphs about the poor experimental design, the dubious quality of the results, the need for controls that even a novice would know, their interpretation of the work (considerably less faltering) and the relevance of the work to modern thinking in the field. You sit, helpless, eviscerated by their comments. You contemplate your scientific death.

Then you recall the invitation to join one or two others for a hunt. You sharpen your scientific knife and ask to be told the identity of the hunted and the time period required for the hunt. You agree and are told that the paper for review will be in the mail that day. Now you can become the hunter.

And, the moral of this piece is? How about being as critical about your own work as if you were one of the hunters reviewing it? And when you hunt a paper as a reviewer, remember how you write one and are hunted.

Caveman

Letters

JCS welcomes correspondence provoked by articles in all sections of the journal. Responses to articles in the Sticky Wicket section should be sent directly to Caveman (email: caveman@biologists.com). Correspondence relating to Research Articles, Commentaries and Cell Science at a Glance should be addressed to the Executive Editor and sent to

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