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Bracker C E. Ultrastructure of fungi. *Annu. Rev. Phytopathol.* 5:343-74, 1967.
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This review article summarizes and analyzes work on subcellular organization and development in fungi. The components of fungal cells are described, with emphasis on membranes. The roles of subcellular structures in developmental processes and interactions between pathogenic fungi and plant cells round out the discussion. [The *SCI*® indicates that this paper has been cited in over 335 publications.]

The Fungal Cell: An Inside View

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The delicate boundary between pleasure and pain can sometimes be elusive and ephemeral. That is how it was for this 1967 review article. Few people know the pain I suffered in preparing the paper and in its aftermath. Fewer yet will want to know the joy and satisfaction I have felt because of its success.

I was a young assistant professor struggling to make my mark nearly 23 years ago when I received an unforgettable letter from Dr. James Horsfall, then editor of the *Annual Review of Phytopathology*. (I have kept that letter and each year share it with students in my Scientific Writing course.) Among other things, Dr. Horsfall wrote:

> We believe that you are the person to whom we should entrust the development "of a consistent body of theory" for that part of our science represented by the topic ULTRASTRUCTURE OF FUNGI. We would appreciate your personal perspective of this topic, your imaginative appraisal of it, not a synoptical summary. We hope that you will consider yourself as the architect of a new structure, an evocative and provocative intellectual structure, a symphony if you will.

A symphony? Nobody had ever asked me to write a "symphony" before. Dr. Horsfall really knew how to appeal to one's ego. Moreover, he gave me 13 months to produce the manuscript. I couldn't say no. Flattery got him everywhere, and I was trapped.

What followed was very painful. Of course I procrastinated, just as I have procrastinated for 10 years in writing this piece for *Citation Classics*.

I labored for some time without much progress. Not only was there no symphony, I couldn't even produce a simple tune. Then horror stuck me. I was failing. It was an impossible task. The new structure, of which I was to be the architect, would not be built. With about two months to go before the deadline, I called Dr. Horsfall and told him I wanted out; the science had not progressed far enough to produce a proper analysis, and I was not up to the job. But the man would not take NO for an answer. He persisted, and by force of character got me to go back and try again.

To make a long story short, I wrote the manuscript out of a profound sense of fear in what was one of the most uncomfortable periods of my life. It is amazing what you can do when you have to. The manuscript was submitted late, but it was finished at last. Can anyone out there know the sense of relief that comes with completing such a task? Probably every scientist has experienced it at one time or other.

The paper was widely cited, I believe, because it was the main review article that brought together information in an area that was attracting a lot of interest in those days. I was most gratified to learn that for about six years during the late 1960s and early 1970s it was the most-cited paper on fungi. Horsfall was right.

I hoped that readers found it informative and provocative. I knew some were provoked because I was aware of some of the controversy that arose from some of the things I wrote.

The most extreme case was the one I most hoped to avoid. I spent a lot of time and great care crafting the paragraph about lomasomes on page 350. I wanted to plant an ample seed of doubt about these structures so that readers would be challenged to do experiments to determine whether or not they were bona fide structures in the living fungal cell. But I also wanted to be very careful not to claim outright that lomasomes were artifacts or that they did not exist in reality. I knew I was walking a tightwire, and I took extreme pains with precise wording to avoid saying that lomasomes did not exist.

Then, at a break during an international meeting in 1971, a man with a fierce look on his face came running at me, waving a reprint in his hand and shouting my name. When he reached me he pushed the reprint at me; his first breathless words were, "You said lomasomes did not exist! Look at this!" Sure enough, as my eyes started scanning this reprint of his most recent paper,[1] I read with disbelief the first words of the introduction in which Bracker (1967) was accused of proposing that lomasomes were artifacts. So much for my gallant attempts at precision.

The pain has long worn off, but it left its mark in that I have a distaste for writing review articles. But writing that paper turned out to be one of the best things I have ever done. It wasn't a symphony, though—a sonata, perhaps.

[Editor's note: Among the papers that have recently cited this work are those by T.W.K. Young[2] and G.T. Cole.[3]]

1. **Bronchart R & Demoulin V.** Mise en évidence par le cryodécapage de lomasomes dans la basidiospore de *Hypholoma fasciculare* (Huds. ex Fr.) Kummer (Occurrence of lomasomes in the basidiospore of *Hypholoma fasciculare* [Huds. ex Fr.] Kummer as revealed by freeze etching). *Planta* 94:229-32, 1970. (Cited 5 times.)
2. **Young T W K.** Ultrastructure of mucoralean sporangiospores. *Bot. J. Linn. Soc.* 91:151-65, 1985. (Cited 5 times.)
3. **Cole G T.** Models of cell differentiation in conidial fungi. *Microbiol. Rev.* 50:95-132, 1986. (Cited 10 times.)