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Results of Chymopapain Chemonucleolysis

To the Editor: The recent article on chemonucleo-

lysis with chymopapain was very interesting (Shields

CB, Reiss SJ, Garretson HD: Chemonucleolysis with

chymopapain: results in 150 patients. J. Neurosurg 67:

187–191, August, 1987). The authors conclude in their

abstract that “these results cast doubt on the long-term

benefits of chymopapain in the treatment of lumbar disc
disease.” Unfortunately, however, their conclusions are

not supported by their own data. The authors noted a

very unfavorable response rate of 40% following

chymopapain chemonucleolysis in their group of pa-

tients who were contacted by means of a long-term

follow-up questionnaire. They stated that the disparity

between the very low success rate found in their study

and the results of others (70% to 73%) cannot be due to

“different methods of information retrieval alone.”

I agree with this comment but disagree with their anal-

ysis as to the reasons for this difference. They imply

that the poor results in their series can be attributed to

a failure of chymopapain to adequately treat the disc

herniation. Their data indicate, however, that the real

reason for a high failure rate was poor selection of

patients for chemonucleolysis.

A failure of chemonucleolysis certainly cannot be

implicated in the 50 patients (40%) who reported a

good or excellent response. In the group of 76 patients

(60%) who assessed their response as fair or poor, 53

patients at reevaluation “were not considered candi-
dates for (postchemonucleolysis) surgery.” No mention

is made in the article by Dr. Shields, et al., as to why

these patients were not surgical candidates. It would

seem from their article that the clinical and radiographic

evidence of disc disease in these patients was so minimal

at reevaluation that surgical exploration was not war-

anted. If chemonucleolysis had failed in these 53 pa-
tients and residual disc material was present, then why

were the patients not operative candidates? If no resid-

ual disc herniation was present following treatment,

then either chemonucleolysis must have been effective

or there was yet another unrecognized cause of the

patient’s persistent pain. When considering the opera-
tive findings in the “failures” who came to surgery, I

greatly suspect that the latter is true.

Among the 23 patients with persistent pain and

enough evidence of disc disease to warrant surgical

exploration, it is obvious that failure of chemonucleo-

lysis can be implicated in no more than seven cases.

No abnormality was found at laminectomy in five

patients; central spinal or foraminal stenosis was present

in 12, mechanical instability in three, scarring in one,

and extruded disc fragments in three. Shields, et al.,

should not be surprised at the poor results of chemo-

nucleolysis in these patients. Indeed, they are to be

expected. The proper time to diagnose the above ab-

normalities is not after chemonucleolysis, but before.

Finally, it is even unclear whether the ineffectiveness

of chymopapain can be blamed for the “failure” of che-

monucleolysis in the remaining seven patients. A bulg-

ing disc (redundant anulus syndrome) was found at

exploration in all of these patients. From the authors’

article it is not possible to determine whether the origi-
nal lesion that was injected was a true disc herniation

or merely a bulging redundant anulus, a condition for

which chemonucleolysis is inappropriate. I also find it

interesting that, in the group of 23 “failures” that came
to surgery, a herniated disc was not found to be the

cause of failure in any patient.

The data published by Shields, et al., do not, in fact,

condemn the effectiveness of chemonucleolysis, but

rather indicate the ineffectiveness of their methods of

patient selection. With modern neuroradiological

imaging and accurate interpretation of these studies, pa-
tients with pain syndromes that are unresponsive to

chemonucleolysis (for example, foraminal stenosis, lat-
eral recess stenosis, central spinal stenosis, hypertrophic

scarring, mechanical instability, sequestered disc frag-

ments, no abnormality, or redundant anulus syndrome)
can be identified. If patients with these lesions are

injected, then the blame for failure must be put where

it really belongs — on a failure by physicians to properly

select patients for treatment. Although it may be true

that all of the injected patients in the authors’ study

“clearly were candidates for surgical intervention,” it is
equally clear that all who were injected were not can-
didates for chemonucleolysis. Laminectomy or conser-

vative treatment is the only alternative in those patients

who have unresponsive causes of low-back pain and

sciatica.

I have attempted to view this controversial and highly

charged topic with as little bias as is possible for a

neuroradiologist who does not treat patients with either

surgery or chemonucleolysis. As a peripheral “obser-

ver,” however, I feel compelled to make several com-

ments concerning this debate.

1. The effectiveness of chemonucleolysis cannot be

judged on the basis of studies that indiscriminately treat

patients with little regard to the responsiveness of the

lesion to chymopapain.
2. Many papers purporting to study the long-term effectiveness of chemonucleolysis are biased toward poor results because patients were entered into the study before the advent of water-soluble myelography and computerized tomography (CT). Accurate exclusion of untreatable causes of pain is certainly not possible with Pantopaque myelography. It is also not consistently possible with water-soluble myelography unless CT scanning is performed as well. In the early days of chymopapain therapy, many patients were treated with chemonucleolysis for "disc herniations" when in reality they had a variety of abnormalities that could not be accurately differentiated from true focal disc herniations.

3. Very rigid exclusion criteria must be used when selecting patients for chemonucleolysis to avoid injecting patients who have untreatable conditions. This is more important than when selecting patients for laminectomy. In the latter situation, the exploration can be extended if the expected disc herniation is not found at surgery.

4. Because of the importance of proper clinical selection of patients, chemonucleolysis should not be performed by neurosurgeons and orthopedic surgeons who are inexperienced in the technique. Equally important is the necessity of consulting a neuroradiologist who is experienced in spinal imaging. Critical scrutiny of the imaging studies for the presence of abnormalities responsive to treatment other than chymopapain is mandatory if good results are to be achieved. Careful collaboration of the clinician and the neuroradiologist is essential for optimal results.

5. Chemonucleolysis should not be performed at levels associated with previous surgery unless it is possible to completely exclude scarring as the cause of the visualized extradural abnormality. Patients who have incomplete pain relief following chemonucleolysis at a previously operated level should, more properly, be considered surgery/chemonucleolysis failures.

6. Incomplete relief of pain after chemonucleolysis in patients with unresponsive lesions should not be termed a "chemonucleolysis failure" but a "selection failure."

In fairness to Shields, et al., many of their patients were selected prior to the availability of modern neuroradiological imaging (CT, magnetic resonance imaging, water-soluble myelography). Although they cannot be faulted for this limitation, they should face the real culprit of their poor results — poor patient selection. We also made many mistakes in the selection of patients for chemonucleolysis prior to the availability of CT and water-soluble myelography.1-2 It can be advantageous, however, if one learns from these mistakes rather than misplacing the blame. I believe that the study by Shields, et al., reaffirms the necessity for proper selection of patients for chymopapain chemonucleolysis. The only statement that seems valid, in their data analysis is: "with further investigation . . . and more rigorous exclusion of some cases, our results might be improved."

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References


RESPONSE: We appreciate Dr. Gentry's thoughtful comments, which likely represent the consensus of the neurosurgical service under the direction of Dr. Javid, who has actively endorsed the use of chymopapain since its early development.

Dr. Gentry's statement that a 40% success rate in our series represents certain benefit from chymopapain cannot be accepted without question, inasmuch as the placebo effect of any form of therapy for pain, including disc-space injection, is at least 30%. Outcome is strongly influenced by the enthusiasm of the treating physician, the patient's expectation of a particular therapy, and the treating physician's role in carrying out the follow-up assessments.

Great care was taken to specify the criteria we used for success: that is, the patient's satisfaction with the treatment as expressed to an independent observer, and the patient's ability to return to work. We believe that these criteria are the only meaningful ones, and manipulation of data to emphasize the relief of leg versus back pain in a patient unable to return to work is not helpful. More than 40% of the patients who were considered to be treatment failures in our series experienced relief of radiculopathy, but continued to have back, buttock, or thigh pain, which prevented the patient's return to gainful employment or required the continued use of analgesics. Continued debilitating back and thigh pain precluded patients in our series from being considered successes. In some reports, relief of radiculopathy alone has been considered a successful outcome. Of the 76 patients who failed chymopapain treatment, 53 had persistent low-back pain without radiculopathy and so were not considered candidates for surgical decompression of their nerve roots.

The issue of patient selection is undoubtedly important. The early patients in our series were investigated with lumbar myelography, antedating intrathecal contrast-enhanced computerized tomography scanning or magnetic resonance imaging. However, all patients had unequivocal radiculopathy with an appropriate myelographic defect. The radicular symptoms and myelographic appearance for each patient were consistent with a specific disc protrusion or extrusion. Some patients had disease related to both discs and bones, but in each case the discogenic component was judged to