LETTERS TO THE EDITOR

LETTER TO THE EDITOR REGARDING REIG ET AL., THERMOCOAGULATION OF THE GANGLION IMPAR OF WALTHER: DESCRIPTION OF A MODIFIED APPROACH. PRELIMINARY RESULTS IN CHRONIC, NONONCOLOGICAL PAIN. *PAIN PRACTICE* 2005;5(2):103–110

I read with interest the article by Reig et al. on Thermocoagulation of the Ganglion Impar. They noted a decrease on the visual analog scale (VAS) of an average of 4.46 units subsequent to treatment.

I would typically expect treatment of the ganglion impar to be useful for coccygeal or sacrococcygeal pain syndromes or for neuromatous anal pain syndromes. Patients 2, 3, 5, 6, 8, 9, 11, and 12 qualified for this diagnostic group and had an average pain reduction of 5.75 units on the VAS vs. n average reduction of 2.4 units on the VAS for those not satisfying that criteria. I would hypothesize that the lesser figure represents the placebo rate for this procedure.

This reinterpretation would suggest that it is appropriate to conduct a randomized, double-blind, placebo-controlled trial for patients with coccygeal or sacrococcygeal pain syndromes and/or neuromatous anal pain syndromes who have responded to a 50% or more reduction in the VAS with local anesthetic block of the ganglion impar. Other diagnostic groups should await confirmation within this primary diagnostic group.

I would appreciate if Reig et al. could provide the duration of analgesic response for the individual patients 1 to 13 in their study sample.

I congratulate them on providing outcome data on these difficult-to-treat conditions.

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REPLY TO DR. MARC RUSSO

We appreciate the comments and appropriate questions raised by Dr. Russo.

The patients subjected to the technique were not selected with the purpose of conducting a study. In fact, they constitute a sample of convenience[1] involving patients that we felt to be appropriate for this type of technique in view of their pathology. We have no clear explanation for why the outcome was so encouraging in some individuals and unacceptable in others. It is possible that the patients in whom the results did not exceed 40% improvement differed from the rest in terms of the pathology involved. In our series of only 13 patients, this was the situation in 4 cases. Patient 1 was diagnosed with pain in the sacral region; patient 10 with chronic postsurgical anal pain; and patient 13 with neuropathic pain of the glans, of unknown origin. Patient 11 (a female), in turn, suffered from coccygodynia. As is well known, pain in the sacroiliac zone is difficult to treat, because of the important innervation found in the region.[2] Patient 13 seems to be a clear case of neuropathic pain refractory to treatment, where it is often necessary to resort to more aggressive procedures such as neuromodulation, or treatments that include more structures.[3] Patients 10 and 11 do seem to have responded to treatment, although in the latter subject retrograde stimulation was carried out[4] without the desired results. At this time, she is awaiting intrathecal testing. As Dr. Russo suggests, it seems clear that these patients, in whom 30% improvement was not achieved, could represent the placebo effect of the technique.

On the other hand, the mean duration of improvement was 2.2 months (1 to 6 months), and assessment of the technique included those patients who failed to respond to the treatment adequately. If these 4 patients are excluded from the series, the mean duration of improvement is seen to increase to 3.5 months (1 to 6 months), taking into account that the study involved a 6-month follow-up period.

Our results are preliminary and refer to a limited number of cases ($n = 13$), representing a heterogeneous population and with a short duration of follow-up. Clearly, it would be both interesting and necessary to conduct a randomized, double-blind study to assess the

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technique in its full magnitude—as occurs with almost all interventional therapies for the management of pain,[5] although the principal objective of our study was the description of a new and apparently more simple approach for blocking this structure.

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LETTER TO THE EDITOR REGARDING FUKSHANSKY M ET AL., THE ROLE OF OPIOIDS IN CANCER PAIN MANAGEMENT. *PAIN PRACTICE* 2005;5(1);43–54

I would like to acknowledge the work and effort put forth by Fukshansky and colleagues, in compiling the recently published article "The Role of Opioids in Cancer Pain Management."[1] Many of the recommendations and comments included in this article are important points regarding the use of opioids in the management of malignant-related pain. However, I cannot find any in vitro or in vivo studies that would conclusively uphold the authors' conclusion that "Hydromorphone interacts with other medications, which can potentiate or reduce its effect." In fact, it is not known whether hydromorphone is metabolized through the human cytochrome P450 system.[2] Furthermore, hydromorphone is not expected to inhibit the metabolism of other drugs metabolized by the CYP1A2, 2A6, 2C9, 2D6, 3A4 isoforms,[2] nor has it been shown, to my knowledge, to have any true cytochrome P450 mediated drug–drug interactions. On the other hand, I agree with the authors that there are indeed pharmacodynamic drug–drug interactions with hydromorphone related to enhanced sedation, respiratory depression, constipation, etc. But in this case, it appears that the authors are suggesting that hydromorphone can reduce the pharmacological effects of other medications through pharmacokinetic means. It has recently been shown that hydromorphone is glucuronidated at 3-carbon to form the inactive hydromorphone—3-glucuronide (H3G) via the expressed human UDP-glucuronosyltransferase UGT 1A3 and 2B7 enzymes,[3,4] and potentially other UGT isozymes.[4] There is very little literature to indicate what occurs to hydromporphone's efficacy when UGT enzymes are inhibited or induced by other drugs.[4] In addition, adding the 3-glucuronide moiety to hydromorphone inactivates the analgesic efficacy of the drug, and the pharmacological activity of this glucuronidated metabolite in humans has not been established.[4] As hydromorphone is not metabolized by cytochrome P450 isozymes to any extent, inhibition/induction or genetic polymorphisms of P450 should have little to no effect on the metabolism/clearance of hydromorphone.[4] With all of this in mind, I cannot find any supporting evidence that would imply or substantiate the authors' conclusion that "Hydromorphone interacts with other medications, which can potentiate or reduce its effect."

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REPLY TO DR. DAVID CRAIG

We would like to thank Dr. Craig for his careful reading of our article on the role of opioids in cancer pain management.[1] What we meant by our statement "hydromorphone interacts with other medications which can potentiate or reduce its effect" are the pharmacodynamic drug–drug interactions described by Dr. Craig in his letter so eloquently. We did not intend to imply any other cytochrome or induction mechanisms.

We are impressed with the level of detailed reading of *Pain Practice* and this should serve to keep all *Pain Practice* authors on notice to be careful with their verbiage and references. We view hydromorphone as a relatively "clean" and safe opioid which helps our pain patients immensely.

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DISC OR DISK?

Between the U.S. and Europe, there has historically existed a spelling divergence relating to medical and other terminology. Generally the U.S. spelling steers away from the use of diphthongs, such as in anesthesia as opposed to the European anæsthesia. While both are accepted as being correct, there are other examples where variant spellings are used, sometimes interchangeably. One example is the intervertebral *disc* . . . or should I say intervertebral *disk*? Which spelling is correct?

The Oxford Concise Medical Dictionary defines as follows: *disc* n. (in anatomy) a rounded flattened structure such as an intervertebral disc or the optic disc.

The Oxford English Dictionary, as is hardly surprising, opts for *disc* while indicating that *disk* is a U.S. variant.

On the other hand, Churchill's Medical Dictionary favours, or should I say favors, *disk* as does the American Association for Medical Transcription's Book of Style.[1]

Turning to the etymology of the word reveals that *disc* is derived from the Latin *discus*, which also gives rise to the French *disque*. The English language owes its rich heritage significantly to Latin so it might seem logical that *disc* should prevail. Alas, English also has strong Greek roots, especially with reference to medical terminology. In any case the Latin *discus*, it seems, is based on its ancestor—the Greek *diskos*. It is from here that the spelling *disk* originates directly.

It is all rather confusing. Which is correct, *disc*, *disk* or both? The Federative International Committee on Anatomical Terminology, who are the final arbiters in all matters anatomical, regard Latin as the basis for anatomical terms.[2] So the correct spelling is *disc*.

Yours *disc*erningly,

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