Letters to the editor

Trigeminocardiac Reflex


Abstract

Objective. Surgical manipulation of the fifth cranial nerve during its intra- or extracranial course may lead to bradycardia or even asystole as well as arterial hypotension, a phenomenon described as the trigeminocardiac reflex (TCR). The authors studied the impact of this reflex on postoperative auditory function in patients undergoing vestibular schwannoma (VS) surgery.

Methods. One hundred patients scheduled for VS surgery were studied prospectively for parameters influencing the postoperative auditory function. The evaluation included sex, age, pre- and postoperative auditory function, preoperative mean arterial pressure, preoperative medical diseases or medication (for example, antiarrhythmia drugs), tumor size and localization, and the intraoperative occurrence of the TCR.

The TCR, which occurred in 11% of the patients, influenced the postoperative hearing function in the patients with Hannover Class T3 and T4 VSs.

With an overall hearing preservation of 47%, 11.1% of the patients in the TCR group and 51.4% of those in the non-TCR group experienced preserved hearing function postoperatively. In cases involving larger tumors (Hannover Class T3 and T4), an intraoperative TCR was associated with a significantly worse postoperative hearing function during VS surgery (p = 0.005).

Conclusions. The hypotension following TCR is a negative prognostic factor for hearing preservation in patients undergoing VS surgery. Patients' knowledge of this can be increased pre- and postoperatively. Further study of this phenomenon will advance the understanding of the underlying mechanisms and may help to improve hearing preservation by controlling the occurrence of the TCR.

Our first description of the TCR by a case-series study was in 1999.5 Given the new results of Gharabaghi et al., the data were reevaluated with respect to the newly recognized influence of the TCR on postoperative hearing function.

Comparing tumor sizes and postoperative hearing preservation, there were some differences between the TCR and the non-TCR subgroups in our study. Looking at all patients with large cerebellopontine angle (CPA) tumors in the non-TCR subgroup, 62% had their auditory function preserved (Gardner Classes I–III), whereas in the TCR group, there were no patients with large tumors with postoperative hearing preservation (Gardner Classes I–III) (Table 1). In the TCR subgroup the overall preservation rate was 7% (one of 14 patients). The only case of preserved hearing was in a patient with a medium-sized tumor.

Intraoperative occurrence of the TCR and tumor size influenced postoperative hearing function after surgery for CPA tumors. Patients with intraoperative occurrence of the TCR had significantly worse postoperative hearing function than those without occurrence of the TCR during tumor surgery in the CPA (p < 0.005, calculated using a stepwise logistic regression analysis with a likelihood ratio test).

We are the second group to document the influence of the TCR on postoperative auditory function and our data are consistent with those of Gharabaghi and colleagues that there is a need to detect appropriate measures to prevent the TCR. The tumor size has only indirect influence, however; the main determinant of the occurrence of the TCR is the localization of the lesion that is operated on. This fact is underscored by our studies in other types of skull base surgery3,4 in which there are different incidence rates of TCR as those found by Schaller and coworkers or Gharabaghi et al.

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References


Response: We appreciate Dr. Schaller’s comments regarding our prospective study on the impact of the TCR on auditory function in VS surgery. By reevaluating a previous retrospective case series of CPA surgery he could confirm our finding that the TCR is a negative prognostic factor for postoperative hearing function.1

Because the TCR is induced by intraoperative manipula-
As reported by Ferraresi and colleagues, May and J. Neurosurg. / Volume 107 / July, 2007

T we continue to perform the clas-

mosis for reanimation of the face. Technical note.

Abstract

the TCR. Surgical strategies to be applied to prevent the occurrence of whether these findings are related to different pathophysio-

matics after the TCR are as yet unknown, it is intuitive that TCR-related hearing loss following VS surgery will be induced by additional factors compared with other CPA lesions.

Therefore, we have conducted a prospective study to analyze the impact of the TCR on hearing preservation after surgery of non-VS lesions of the CPA. Preliminary results indicate that there are differences in postoperative hearing function in VS and non-VS tumors following the TCR. Investigators in future studies will have to demonstrate whether these findings are related to different pathophysiological mechanisms and whether this necessitates different surgical strategies to be applied to prevent the occurrence of the TCR. (DOI: 10.3171/2007.07.O garbage)

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Reference


Hypoglossal-Facial Anastomosis


Abstract

The aim of this paper was to report on further experience with a new technique for reanimation of the facial nerve. This procedure allows a straight end-to-side hypoglossal–facial anastomosis without interruption of the 12th cranial nerve or the need for graft interposition. It is technically demanding and time consuming but offers an effective, reliable, and extraordinarily quick means of reinnervating the facial muscles, including the orbicularis oculi muscle, thus avoiding the need for a gold weight in the eyelid or a fascial sling.

The paper describes a novel technique of hypoglossal-facial nerve anastomosis (HFA), in which the facial nerve (FN) is attached end-to-side to the hypoglossal nerve (HN). Several aspects of this technical note are rather surprising, difficult to understand, and merit some comments.

We have experience derived from more than 100 classic end-to-end HFAs (E.F., unpublished data). In this operation, the entire trunk of the HN is sacrificed in order to be connected end-to-end to the FN with no further procedure designed to reinnervate the distal stump of the hypoglossus and the corresponding hemitongue. Facial muscle recovery begins 3 to 4 months after this operation and final recovery is generally observed after approximately 1 year. The result of this HFA procedure regarding the function of facial muscles is generally excellent, especially when the operation is performed within the first 6 months after FN injury. Very importantly, functional recovery of the orbicularis oculi muscle permits good closure of the eye, and it is not then necessary to add a gold weight in the eyelid or a fascial sling. Patients are generally very satisfied with their facial functional recovery and tolerate the hypoglossal deficit well.

According to these positive experiences, which confirm data published by others,2 we continue to perform the classic end-to-end HFA. We believe that if the FN deficit is to be considered the primary problem, then the sacrifice of the HN is justified by the resulting good facial recovery.1 The results obtained when sacrificing only one portion of the HN, instead of the entire trunk, are those rationally expected considering that the number of axons innervating the facial muscles are proportional to the entity of the HN sacrifice.1 As reported by Ferraresi and colleagues, May and colleagues’ hypoglossal–facial jump graft “involves the use of only a portion of the hypoglossal nerve and has the enormous advantage of avoiding the tongue dysfunction that results from resection of the entire hypoglossal nerve. It is technically safe, and the results are consistent and durable. Recovery starts approximately 7 to 8 months postoperatively... and the final functional result is generally good...” but “Closure of the eye generally requires... insertion of either a gold weight or a fascial sling in the upper eyelid.”

Although we are critical of most HFAs using HN-sparing techniques, we strongly disagree with the technique presented in this paper, which pursues the philosophy of end-to-side nerve anastomosis, in which the axons of the donor nerve (in this case the HN) are maintained intact. In this paper some data appear rather surprising.

1) The two patients underwent operations 1 year after the occurrence of FN palsy, which is past the ideal time for performing FN repair according to modern surgical principles.

2) The FN was isolated and cut in the fallopian canal in such a manner as to gain an additional length of 3.5 cm in comparison with the length of the nerve in the classic end-to-end HFA. Despite obtaining the additional length of 3.5 cm of the FN, rendering the facial muscles further away for regenerating axons, with the technique of Ferraresi and coworkers, reinnervation started 2 months postoperatively, muscle tone was excellent at 3 months, and at 6 months facial muscle function was very good. The timing of FN recovery appeared therefore significantly better, as compared with the beginning of reinnervation 3 to 4 months after the classic HFA technique, and 7 to 8 months after using May and associates’ technique.

3) The distal FN was connected end-to-side to the HN through a small epineurial window and an opening of the endoneurium of two descending fascicles, that were not cut but left in continuity. Classically, the fascicles of nerves are sheathed by the perineurium and the axons by the endoneurium. What exactly is intended by the term endoneuria? In Fig. 2, the term “epiendoneural opening” of the hypoglossus (leaving the fascicles in continuity) is used; we argue that the classic term should be epiperineurial opening.
4) The authors claim that the results of their operation are unquestionably dependent on the precision of the suture. The precision required in end-to-end nerve repair, in which the best apposition of the nerve stumps is strongly emphasized constantly, is understandable. The precision in an end-to-side neurorrhaphy with a cut end of the FN perpendicular to the lateral part of two HN fascicles left intact is less understandable.

Therefore, in the two reported cases, something quite surprising occurs: the patients undergo operations relatively late, the FN stump used is longer than that in classic HFA, no axons of the donor HN are sacrificed and only two fascicles are approximated to the FN, and yet the FN recovery is earlier than after other techniques and the results are excellent (reported as House–Brackmann Grade II). The authors therefore conclude that this technique might even be a substitute for a direct FN repair.

We believe that this conclusion cannot be based on just two patients, particularly because there is still no recognized experimental explanation of the mechanism of end-to-side nerve anastomosis. The clinical report of two cases is more anecdotal than scientific, and cannot justify the abandonment of using the more typical end-to-end HFA technique and its variants that aim to spare axons of the HN. Unfortunately, there is not yet enough data to support the principle that one damaged nerve can be repaired using a close donor nerve and leaving it intact, and for repairing other peripheral nerves, the as yet scientifically undemonstrated efficacy of the so-called end-to-side neurorrhaphy. To accept such a principle could cause a revolution in peripheral nerve surgery. In fact, if it is accepted that a nerve coaptation performed during an end-to-side repair guarantees good functional results, as stated by Ferraresi and associates, then all the technical advice on the performance of a good end-to-end nerve repair should be declared exaggerated or even false, considering the ability of axons to cross the connective sheath of the donor nerve and to find and enter the endoneurial tubes of the recipient nerve.

The report by Ferraresi and colleagues is certainly interesting and thought-provoking, but further investigation of the mechanisms of end-to-side nerve coaptation is necessary before abandoning the classic, well-known, accepted techniques of nerve repair.

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References

Response: We are greatly honored for the attention we have received from our colleagues in Rome. Their interest in the treatment of facial nerve palsy as well as in HFA is well known. We also share some of the problems raised by their letter and will summarize our comments on these issues.

1) Despite their defense of the end-to-end HFA with section of the entire HN, the complete atrophy of half of the tongue muscles is far from being considered a well-accepted deficit. In some patients this deficit entails moderate to severe difficulties in swallowing and speaking, and is worse among younger patients. It is understandable that some of them may consider it quite acceptable, but only because they are faced with the alternative of a permanent complete facial palsy.

Technically, moreover, the end-to-end suture between the FN and the entire HN entails a waste of hypoglossal fibers because a considerable mismatch in the square section of the two nerves is readily visible (the HN is approximately two times the size of the FN). But this is not the most important question. The technique popularized by May and colleagues nicely covers both the facial palsy and the integrity of the tongue. The only disadvantage of this technique is the use of a graft, which limits the number of fibers passing through the two suture lines, and may explain a prolonged reinnervation time using this technique, as well as a variable loss of axons with the consequent inability to reinnervate the periorcular muscles.

2) The intratemporal technique results in a shorter recovery time than the jump graft technique by May et al. The technique we described in our paper has the enormous advantage of eliminating the graft, and this understandably shortens the reinnervation time with two mechanisms. The unique suture line is certainly the main factor explaining a quick recovery, but the shorter distance crossed by the regenerating axons cannot be overlooked. The intratemporal rerouting of the facial nerve adds a 3.5-cm length compared with the nerve sectioned at the tragal pointer, but this length is still considerably less than the 6–cm-long graft typically required using the technique by May et al. Therefore, it is not surprising that recovery may begin sometime in advance of other techniques, due to these two factors.

3) The timing of recovery is even shorter than with the traditional full-sized HFA. Since the publication of our original study, we have followed-up two more patients who underwent operations using the intratemporal technique, and recovery in these two new cases also began within 3 months. The shortness of recovery with our technique is therefore an unquestionable fact, despite any explanation that might be considered (from a theoretical point of view) fully convincing. Nevertheless, we would like to point out the role of the extensive wallerian degeneration that takes place in the nuclear motor neurons after nerve severance. The consequent “bereavement” due to the reorganization of Nissl substance and the growth of neurofilaments takes time, which has been beautifully presented by Fernandez himself in experimental papers. We speculate that this time interval might be somehow proportional to the severity of nerve damage, and conversely almost negligible if only an epiperineurial damage of the fascicles is produced.

4) The timing of surgery (1 year after nerve injury) is claimed to be too late. We had excellent results in patients who underwent operations 1 year after the onset of facial nerve palsy, another unquestionable fact. We agree that in nerve repair, the sooner the operation occurs the bet-
ter, which is, generally speaking, our policy. Waiting 12 months before undertaking a nerve repair is mandatory, however, when the surgeon finds substantial integrity of the FN after acoustic tumor removal. Under these conditions, both our experience and data from the literature confirm that most spontaneous recovery is observed within the first 12 months, and only after this time period can one be sure that a spontaneous recovery of the FN will no longer occur. Clearly then, any HFA procedure performed earlier will involve a definite risk of cutting a nerve capable of spontaneous recovery.

5) Regarding the need to insert a gold weight in the eyelid, in two of our cases we were able to remove the gold weight due to satisfactory recovery of the periorcular muscles without long-term complications. This occurred approximately 1 year after repair, and we hope that in all cases, once the reinnervation has consolidated, insertion of a gold weight will be the last option.

6) Results depend on the precision of the microsuture surgical technique. We confirm that the results strongly rely not only upon the precision of the microsuture, but also on the accuracy of the microsurgical work in its entirety. In fact, at different stages, the final result can be jeopardized. Eventual hazards include: the harvesting of the delicate distal stump of the FN from the facial recess; degluing the FN from the fallopian canal; the paraneural stay sutures on the FN required to approximate the donor and recipient sites; the final stage of the microsuture procedure in which small details, such as an intervening epineural flap, can represent an insurmountable barrier between the donor FN and the recipient FN stump. The anatomical location and pattern of a classic HFA with an end-to-end suture, on the contrary, is able to better tolerate a lack of precision, due to the abundance of available motor axons. Lastly, we note that in nerve surgery a very important role, if not the principal one, is represented by the peculiarity of the donor and recipient nerves. For example, for a radial nerve, a microreconstruction is not really needed. The term “micro” is often misused simply for the sake of prestige. It is well known that even a gross technique and a rather thick naked-eye epineurial repair of a radial nerve in the arm gives good to excellent results. On the contrary, in the repair of the ulnar nerve in the axilla, even the best microsurgical technique cannot guarantee a satisfactory result.

7) Regarding the use of the term “epiperineurial” versus “epiendoneurial” opening of the fascicles and end-to-side suture, only on this final point do we completely agree with the observations of Fernandez and colleagues. Epiperineurial opening of the HN nerve would better convey the meaning of the technique, but in light of recent further experiences we have to admit that a small to moderate degree of fascicular damage cannot be excluded during the preparation. Although the surgical plan is still intended not to interrupt the nerve fascicles, this may occasionally be the case. Even with these limitations the final result is certainly a gross preservation of the integrity of the HN, and good results of the “true” end-to-side suture are confirmed by several recent studies applying the same technique we described.

We would like to make one final comment. We are aware that this technique requires much work and its applicability is limited to ear, nose, and throat surgeons or neurosurgeons with basic skills in skull base approaches. This technique is beyond the level, in general, of plastic surgeons, but in our opinion the technique still meets all the criteria required for an efficient FN repair. The advantages of our technique—one single suture line, no harvest of a graft with resultant scar and neuroma formation, no sacrifice of the donor nerve, and the eventual removal of the gold weight—demonstrate that it provides quick and excellent results at a very low cost. 

References


Intracerebral Hematoma Lysis


Abstract

Object. Currently no adequate surgical treatment exists for spontaneous intracerebral hemorrhage (ICH). Implantable polymers can be used effectively to deliver therapeutic agents to the local site of the pathological process, thus reducing adverse systemic effects. The authors report the use of stereotactically implanted polymers loaded with tissue plasminogen activator (tPA) to induce lysis of ICH in a rabbit model.

Methods. Ethylene vinyl acetate (EVA) polymers were loaded with bovine serum albumin (BSA) only or with BSA plus tPA. In vitro pharmacokinetic (three polymers) and thrombolysis (12 polymers) studies were performed. For the in vivo study, 12 rabbits were fixed in a stereotactic frame, and 0.2 ml of clotted autologous blood was injected into the right frontal lobe parenchyma. After 20 minutes, control BSA polymers were stereotactically implanted at the hemorrhage site in six rabbits, and experimental BSA plus tPA polymers were implanted in six rabbits. All animals were killed at 3 days, and blood clot volume was assessed.
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The pharmacokinetic study showed release of 146 ng of tPA over 3 days. The tPA activity correlated with in vitro thrombolysis. In the in vivo study, the six animals treated with tPA polymers had a mean (± standard error of the mean [SEM]) thrombus volume of 1.43 ± 0.29 mm³ at 3 days, whereas the six animals treated with blank (BSA-only) polymers had a mean (± SEM) thrombus volume of 19.99 ± 3.74 mm³ (p < 0.001).

Conclusions. Ethylene vinyl acetate polymers release tPA over the course of 3 days. Stereotactic implantation of tPA-loaded EVAc polymers significantly reduced ICH volume. Polymers loaded with tPA may be useful clinically for lysis of ICH without the side effects of systemic administration of tPA.

The authors performed two in vitro studies and an experimental study in 12 rabbits in which they created a frontal hematoma by injecting a total of 0.3 ml of autologous arterial blood. In six animals, tPA polymers were implanted on the pial surface at the bur hole site, while in control animals only vehicle control polymers were implanted. The animals were killed on Day 3 and the remaining hematoma volumes were compared. The tPA-treated animals showed a significant hematoma volume reduction.

To date, there is no optimal therapy for the management of spontaneous ICHs; comparisons of surgery with conservative medical management demonstrate that neither is superior. The authors propose an alternative to macro- or microsurgical clot removal or stereotactic catheter placement for tPA injection and drainage of the liquefied blood.

Considering the pathophysiology of ICH in humans, the data of Thai and associates regarding stereotactically implanted tPA polymers should be considered with several caveats. 1) The authors experimentally created a cortical-to-subcortical ICH. A tPA polymer placed on the pial surface lysed the hematoma sufficiently. The majority of hematomas for which stereotactic procedures are performed are deep-seated however, and hence less likely to be dissolved by cortical tPA. 2) An intracranial hemorrhage causes secondary brain damage via compression-induced perilesional ischemia. Therefore, rapid reduction of the clot volume (and removal of toxic blood products) is advantageous. Accordingly, fibrinolytic therapy with intrahematomal tPA injection is often combined with aspiration and/or drainage of the liquefied blood and blood products via the stereotactically implanted catheter. 3) The theoretical benefits of tPA polymer implantation (such as a lower risk of infection and a less time-consuming procedure) must be weighed against the disadvantage of slower hematoma volume reduction. 3) Tissue plasminogen activator is not toxic in the intact brain, as demonstrated again by the in vivo component of the study by Thai and coworkers. Yet in the presence of intracerebral blood, tPA exerts toxic properties either by the excitotoxic pathway, and/or by lowering the thrombin-inactivating effect of plasminogen activator inhibitor-1. The search for nontoxic fibrinolytic agents is hence more relevant than the search for alternative delivery methods. 4) No imaging was done immediately after hematoma induction in either the tPA-treated group or the control animal group. Thus, it is theoretically possible that the demonstrated tPA effect was biased by more successful blood injections in the control group than in the treatment group.

Although therapeutic agent–releasing polymers may hold promise for some indications, tPA polymers have yet to be demonstrated as an effective treatment for deep-seated ICHs requiring rapid evacuation.

RESPONSE: We appreciate the insightful comments of Drs. Rohde and Samadani, and their interest in our study. As stated in their letter, surgical and conservative medical management of intracerebral hematomas have shown limited benefits and have failed to demonstrate significant advantages of one paradigm over the other. In our study we present a technique for stereotactic implantation of tPA-loaded controlled-release polymers in a rabbit model of ICH. The polymers described in our study showed effective sustained-release of tPA over time both in vitro and in vivo, and constitute a potential therapeutic alternative for the treatment of experimental ICH.

The authors correctly note that in our model of ICH, hemorrhage occurs in a cortical/subcortical location. As shown by histopathological analysis of cross-sections of the rabbit brain with induced ICH, however, the shape and location of the hemorrhages closely resemble those of human ICHs.

The authors also comment on the pial placement of the polymers, and on the deep nature of ICHs that would be amenable to stereotactic polymer implantation. We and others have previously shown that the physical properties of EVAc polymers allow preparation of thin cylinders that fit in a stereotactic needle, and we have demonstrated in preliminary studies that using the Leksell stereotactic frame and a standard targeting algorithm, a polymer loaded with a tracking substance (Evans blue) can be accurately deployed to a selected area and can release the loaded substance in a sustained fashion with adequate penetration and diffusion through the human brain parenchyma.

We also agree with the authors’ comments on the benefits that rapid reduction of clot volume and removal of toxic blood products can provide. By incorporating thrombolysis polymer implantation with the standard technique, stereotactically implanted catheters for clot aspiration can be complemented, because postaspiration polymer deployment can facilitate reabsorption of the hemorrhagic products that remain, especially in subacute ICH cases in which fibrinous membranes can decrease the amount of clot volume aspirated with a catheter.

Regarding the safety of intracerebral tPA administration, we did not find behavioral changes in animals implanted.
with the treated doses of tPA, and histopathological analysis of the animal brains did not show any evidence of toxicity, although this part of the study was performed in animals without ICH. In the efficacy component of the study, animals with induced ICH that were treated with tPA did not exhibit behavioral or histopathological signs of central nervous system toxicity. Neurotoxicity related to activation of the excitatory pathway or plasminogen activator inhibitor-1 levels was not investigated and as mentioned by the authors may play a role in the restoration of homeostasis and in the overall outcome of ICH. Therefore we expect that as new thrombolytic agents are developed these can also be incorporated into polymeric matrices for stereotactic implantation.

Drs. Rohde and Samadani comment on the use of imaging to evaluate the amount of hematoma reduction in the animals, to correct for differences in the volumes of injected blood. As stated in our methods section, all animals injected received a total of 0.3 ml of autologous blood, and cross-sections of the brains did not reveal significant differences in the sizes of the hematomas in the control group.

We anticipate that stereotactic implantation of controlled-release polymers loaded with thrombolytic agents could be a beneficial adjuvant treatment of ICH and an ideal complement for stereotactic catheter implantation. Furthermore, as the field of neuroprotection evolves, other agents that could be of benefit in the treatment of ICH—which cannot reach effective central nervous system concentrations via systemic administration—could also be delivered.

We thank Drs. Rohde and Samadani for their comments and interest in these issues. (DOI: 10.3171/JNS-07/07/0246)

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External Ventricular Drain Infection

To THE EDITOR: We read with great interest the article by Lo and colleagues (Lo CH, Spelman D, Bailey M, Cooper DJ, Rosenfeld JV, Brecknell JE: External ventricular drain infections are independent of drain duration: an argument against elective revision. J Neurosurg 106:378–383, March, 2007).

Abstract

Object. The authors explored the relationship among the duration of external ventricular drainage, revision of external ventricular drains (EVDs), and cerebrospinal fluid (CSF) infection to shed light on the practice of electively revising these drains.

Methods. In a retrospective study of 199 patients with 269 EVDs in the intensive care unit at a major trauma center in Australasia, the authors found 21 CSF infections. Acinetobacter accounted for 10 (48%) of these infections. Whereas the duration of drainage was not an independent predictor of infection, multiple insertions of EVDs was a significant risk factor. Second and third EVDs in previously uninfected patients were more likely to become infected than first EVDs. An EVD infection was initially identified a mean of 5.5 ± 0.7 days postinsertion (standard error of the mean); these data—that is, the number of days—were normally distributed.

Conclusions. This pattern of infection is best explained by EVD-associated CSF infections being acquired by the introduction of bacteria on insertion of the drain rather than by subsequent retrograde colonization. Elective EVD revision would be expected to increase infection rates in light of these results, and thus the practice has been abandoned by the authors’ institution.

We would like to congratulate the authors for carrying out this thoughtful analysis, which concurred with the results of our randomized controlled trial.

Our data concurred with the authors’ results that elective EVD revision did not decrease the CSF infection rate and might actually increase CSF infection rates, possibly from introduction of bacteria during EVD insertions. But we disagree with the authors’ institutional policy of not administering prophylactic antibiotics, and believe this policy may in effect exacerbate the CSF infection rate with more EVD revisions. In a recent similar study by Arabi and colleagues,1 prophylactic antibiotic use was associated with a lower rate of ventriculostomy-associated infection; infection developed in 7 (12%) of 58 catheters in which prophylactic antibiotics were used and in 12 (29%) of 41 of catheters in which systemic antibiotics were not used (p = 0.03). The importance of prophylactic antibiotic use can be understood from its similar role in ventriculoperitoneal (VP) shunt insertion, where it is well established that antibiotic prophylaxis reduces the infection rate by 50%.2,3

At our institute, we performed an antibiotic study in the era of regular elective EVD revision, comparing perioperative prophylactic antibiotic use only with prolonged prophylactic antibiotics as long as the EVD remained in place.4 The results showed that prolonged prophylactic antibiotic coverage was associated with a significantly lower CSF infection rate than perioperative antibiotic use only. It remains uncertain whether the abortion of regular elective EVD revision will abolish the beneficial effect of prolonged prophylactic antibiotics. Another new development is ventricular catheters impregnated with antibiotics, and it will be interesting to see if their use will replace the policy of using prophylactic antibiotics during EVD revision.

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References


Response: We thank Drs. Wong and Poon for taking the time to critically appraise our recent paper. They agree with our conclusions that rather than preventing EVD-associated...
considered CSF infection, the elective revision of EVDs may actually increase the incidence of infection, and that such infections are likely to be introduced at the time of drain insertion rather than by subsequent retrograde colonization. Such infections should be particularly sensitive to the use of prophylactic antibiotics and Drs. Wong and Poon raise important concerns about the use of these in our study.

In the methods section of our own paper we stated that “Prophylactic antibiotics were not routinely prescribed” and did not further address the relationship between the use of antibiotics and the incidence of EVD-associated CSF infection in the study population. Drs. Wong and Poon have interpreted this to mean that it was the institutional policy at the Alfred Hospital not to give antibiotics to patients undergoing EVD insertion. Between October 2002 and May 2004 it was institutional policy for neurosurgical cases in the operating room to receive prophylactic antibiotics, and many of the patients in the intensive care unit and the emergency room would have received antibiotics around the time of their EVD insertion for unrelated reasons. However, there was no cohesive policy regarding the use of prophylactic antibiotics in EVD insertion. Following the preparation of this paper such a policy was introduced and patients not already receiving broad-spectrum antibiotic therapy now receive a single dose of cephazolin at the time of insertion of their EVD.

That the use of prophylactic intravenous antibiotics decreases the rate of VP shunt infections is generally accepted; however, their role in the prevention of EVD-associated CSF infection is less clear. In the study by Arabi et al. discussed by Drs. Wong and Poon, the authors’ own conclusion was that the use of antibiotics did not significantly alter infection rate. Despite this conclusion, the use of systemic prophylactic antibiotics for coverage during insertion of EVDs is reasonable. The use of catheters impregnated with minocycline and rifampin has been shown to significantly reduce the risk of infection in a randomized controlled trial, although the cost implications of their use remain to be explored. Intraventricular antibiotic prophylaxis may reduce the rate of VP shunt infection compared with systemic administration alone; we know of no direct evidence regarding their effect on the incidence of EVD-associated CSF infection. That intraventricular application may be a further route by which antibiotics can help to reduce the incidence of this, the most common complication of external ventricular drainage, is an intriguing possibility.

References