Interview with Prof. Dr. Friedrich Magerl

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Friedrich Paul Magerl (1931) became an orthopaedic surgeon in 1970 after his general surgical and orthopaedic training in Graz and St. Gallen, Switzerland. He received his PhD degree in 1986 from the University of Bern and became professor in orthopaedic surgery in 1992 at the same university. His main interests in orthopaedics are spinal surgery and surgery of the foot. He was inventor of the AO external and internal spine fixator that were to radically change the management of the fractured spine. In 1994 he published a paper on a new classification scheme for thoracic and lumbar fractures that became the AO-classification. Although he retired from orthopaedic practice in 1999, he continues to be active as a member of editorial and advisory boards of various international scientific journals.

How did you get involved in the operative management of traumatic spine fractures while conservative treatment was the more common practice and spine surgery was something exotic during your training period?

During my work as a resident in Graz, we treated most thoracic and lumbar pathology conservatively. This changed when we had a Japanese colleague over for some time in Graz who had experience in the anterior reduction and fusion of cervical dislocations. From him we learned how to become more comfortable with the operative treatment of the upper spine. This was, by the way, all back in the early sixties.

How did you proceed from then?

Slowly, we worked our way down from the cervical to the thoracic and lumbar levels as we gained more experience. The indications were mostly deformity and degenerative pathology. At that time we usually did not operate on spinal fractures. In the mid-to-late '60s, we (Weber, Zimmermann, Magerl) performed a large series of anterior interbody fusions according to the technique described by Mercer. Our results were surprisingly good compared to the OR. Mind you, we started fixing these spines in paraplegics only in order to facilitate their treatment. In the end, the credit for transpedicular fixation goes to Harrington who performed the very first clinical pedicle screw insertion to get a better ‘three-point fixation’ for his rod instrumentation. And before that, in 1949, the anatomists Michele and Krueger had already thought of the transpedicular route to access the vertebral body for biopsies, for example.

You did not go to the anatomy lab to see if it could be done in a safe way?

No, as I said we learned as we got along with the operations. That was the usual practice and also the way to advance techniques and treatments. However, in this context I would like to mention, that I worked for nine years at the Department of Anatomy in Graz, prior to starting with clinical practice. For our treatment of paraplegics, we got massive critique from the followers of Guttmann. Guttmann was the most prominent proponent of the traumatic fractures were still treated conservatively or did that change with the increased knowledge you gained from the degenerative series?

Well, in the early sixties we sometimes did operate on traumatic spine fractures; but mostly on paraplegics. The paraplegia was not considered treatable, but the gross spinal instability that often accompanied it, was. To facilitate better nursing and earlier mobilization, in a wheelchair for example, we aimed to stabilize the spine as quickly as possible without all those cumbersome treatments like bedrest, skull traction or plaster jackets that had to be applied for several months. In 1963, in St. Gallen, my later colleague Weber begun to stabilize the spine by wiring the spinous processes and ‘cementing’ them together with polymethyl methacrylate cement. Some years later, AO plates with transpedicular fixation were used in St. Gallen for treatment of severe spine fractures at thoracic as well as at lumbar levels (Figure 2a-b).

But, who taught you how to safely insert these pedicular screws?

Hmm, no one. In these days you learned and taught yourself and others in the OR. Mind you, we started fixing these spines in paraplegics only in order to facilitate their treatment. In the end, the credit for transpedicular fixation goes to Harrington who performed the very first clinical pedicle screw insertion to get a better ‘three-point fixation’ for his rod instrumentation. And before that, in 1949, the anatomists Michele and Krueger had already thought of the transpedicular route to access the vertebral body for biopsies, for example.

And traumatic fractures were still treated conservatively or did that change with the increased knowledge you gained from the degenerative series?

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Figure 2.


B Same patient with the spine stabilized with AO plates and transpedicular screws.
conservative treatment of paraplegia in his days and long after that. We operated because we wanted to get the patient’s spine stable enough to get him/her out of bed as soon as possible instead of the, then usual, treatment of bed rest, logrolling and other bedridden activities. I still do not see anything wrong with our policy. Anyhow, surgical intervention in those days was strongly advocated against. This is something, which, considering the, then, widespread practice of laminectomies as a form of neurologic decompression that made things only worse (especially decreasing the stability of the spine), might have turned out too bad after all. Guttmann’s authority on the conservative treatment however, made any surgical practice suspicious, which I think has seriously delayed advancements in this field.

Who or what influenced you during these early days of traumatic fracture management?

Weber’s work that I mentioned, was among the first that I encountered because we worked together a lot. But Harrington’s system, originally developed for scoliosis correction surgery, was becoming the regular treatment of burst fractures of the spine. Although it was possible to fixate a fracture with the Harrington rods, restoration of lordosis was not possible and one had to fuse five to seven levels to get adequate stability. I wasn’t a big fan of this multilevel fusion practice. I wanted a short fixation system that could provide immediate stability and good alignment without the need of postoperative treatment with plaster jackets or braces. It just had to be stable from the beginning to the end. So, one day a young patient presented with an L3 burst fracture. Stabilization with Harrington rods or AO plates would mean a fusion from at least L1 to L5, meaning no motion left in the lumbar area. That was unacceptable to me. In this case we needed a device that could guarantee adequate stability and a correction of the sagittal angle without the multisegmental immobilization. Since we had quite some experience with external fixation of fractures of the extremities, I decided to treat this spine fracture with such an external fixation device. It worked! It worked well. The external spine fixator was developed shortly afterwards (Figure 3).

The compliance of the Schanz screws in combination with the external spine fixator was key in the technical success of this new spine fracture treatment. We did not encounter failures because the combination of external fixator and Schanz screws was flexible enough to withstand peak pressures. It just did not fail.

The external spine fixator was the first true minimal invasive device for the treatment of traumatic conditions of the spine. However, to my knowledge just one international paper, written by you, has been published in the literature. Why did this new treatment not succeed?

That is not completely true. There have been quite some fracture series with very acceptable results after external fixtion but they did not get a lot of attention.

Why is that?

Probably because there is more than just patient outcome and the number of complications, it is about the acceptance of a new technique. It sounded like you had a very promising technique up your sleeves. Why would colleagues or journals not accept it?

First of all, the aftertreatment of these external fracture fixations was difficult, also for the patient. It was much easier with the internal fixator, which was developed only some years later. But I am not sure whether a commercial aspect might have played a role also. You can use one external fixator over and over so, commercially, it was not very interesting. That significantly decreases the support you get from some parties. Mathys, the company that manufactured it, has maybe sold twenty or thirty, probably not much more than that. I think (pointing to an assortment of external fixators on his desk) I have twenty percent of all the external fixators that were ever made here in my office. You and I together have around thirty percent.

Then the internal fixator followed and became a big success. Were you involved in the development of the AO internal fixator?

Yes, in another patient I was confronted with the problem of a serious instability at the levels L4 to S1 following discectomy. In this case we needed a prolonged period of stabilization in a lordotic position for an interbody fusion to occur. This would have been possible with the external fixator (and impossible with the Harrington rods) but it would be quite patient unfriendly due to the extended period that it had to be attached to the patient’s back. I asked Mathys to redesign the fixator to a more compact layout so that it could be implanted under the paraspinus muscle. We used it in combination with anterior lumbar interbody fusions at both levels and with good result (Figure 4). Walter Dick, who was a junior staff member at Basel, asked me if he could use this principle to treat traumatic fractures with it and to redesign
Do you think a relation exist between the level of detail of the AO-classification and a more reliable prediction of prognosis? For a lot of fractures, I think there is. In case of A-2 type fractures, for example, I think that you need an anterior procedure instead of a posterior or conservative treatment. In classifications with less detail, they probably would be called stable burst fractures for instance, and treated accordingly. Possibly with failure as a result. So, yes I think this level of detail is useful, sometimes even needed, for deciding how to treat certain fractures and that has a direct influence on outcome, obviously. The most important thing is the discrimination between A-type, B-type and C-type fractures because this is most decisive for the treatment. The majority of the first, which are A1 fractures, will heal with little or no intervention while the other two, in my opinion, almost always require stabilization otherwise will develop into severe malalignment or even neurologic deficit.

In a recent paper in Spine about spine fracture management, I was a little surprised to see that the Denis classification is not dead at all. There still seems to be a lot of support for that classification, especially in the United States. Why has the AO-classification not become the golden standard? (meaningful smile) Probably because the AO-classification was “not invented here”? Whatever the reasons, if you feel comfortable with a particular classification you should use it. It also has a lot to do with training; who you were taught by and what you will teach the next generation et cetera.

How would you comment on relatively new techniques such as anterior and circumferential approaches; do they offer advantages in traumatic spine fracture management over posterior techniques? For selected cases I think the anterior technique is valuable. Take for instance severely impacted wedge fractures, coronal split burst fractures or fractures with severe comminution of the vertebral body. You will find it hard to restore the anterior column support in these types of fractures from posterior. In these cases I would seriously consider the anterior approach as the treatment of choice.

Ok, but what about the circumferential technique; what would be the indication? Jurgen Harms was the first to advocate this technique. He was forced to go anteriorly because the posterior instrumentation he used was too weak to function as stand-alone implant for severe burst fractures. I would say that nowadays, the circumferential technique might be useful for some specific indications and maybe also if you want to come up with nice X-rays showing excellent alignment of the spine. But as you will recall, the correlation between radiographics and final clinical outcome is weak.

the instrumentation together with Mathys. I agreed and that is, more or less, how the AO internal fixator got born.2

Do you think it was necessary to perform posterolateral bone grafting after fixation with the internal fixator in fracture treatment? Walter Dick did not perform posterolateral fusions, I almost always did. Our clinical results were virtually identical but I wanted to have a ‘second line of defense’ in case something happened during the healing period with the fixator in situ. I realized, though, that restoration of the anterior column support was the most important job. Without an adequate anterior column, kyphosis would invariably develop, even with a posteriorly fused spine, due to bone remodeling under the forces that acted upon the spine.

How did you classify spine fractures in the ’70s and ’80s before the classifications invented by Denis and yourself?3,4

In the early days we used the Holdsworth classification but it didn’t really capture the whole spectrum of lesions that we encountered. We used the Denis classification and after that McAfee’s for a while but the same problem arose. Since we saw a quite a number of spine fractures that could not be classified by these classifications, the amount of ‘miscellaneous’ grew bigger and bigger.

Why did you feel the need for a new classification and how did you proceed?

If I think of a ‘fracture dislocation’ and you think of a ‘fracture dislocation’, the chances are very small that we think of an identical fracture type. The main reason for the development of the classification was thus quite an academic one; I wanted the spectrum of possible lesions to be described in detail, making discussion between physicians about fractures more meaningful, and in the process, simultaneously, decrease the size of the ‘miscellaneous’ group. I had very long conversations with Jurgen Harms and the other co-authors of the paper, about how to set up a classification. We agreed that the AO concept, using the familiar 3-3-3 structure might be a good basis for the spinal fracture classification as well. It took us ten years to develop the classification.

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Figure 4.

Postoperative radiographs of the first patient with the internal fixator in situ. The screw on the right of the radiograph was used to fixate the allograft in the defect of the iliac crest that resulted from the autograft procedure for the interbody fusion.
tial approach is very invasive so it should be reserved for those special occasions in which a single approach cannot stabilize the spine sufficiently.

Why did transpedicular spongiosaplasty, used to augment the anterior column from posteriorly and described first by Daniaux, perform worse than expected? It depended on the type of spongiosaplasty that was performed. In my opinion it is impossible to achieve an interbody fusion using the transpedicular intervertebral spongiosaplasty technique. You just do not have enough working space when going through the pedicles to remove the disc completely, even partly. In the end it probably made things worse by adding instability to that segment. Intra-vertebral spongiosaplasty has been more successful because it was only meant to fill the defect in the vertebral body that resulted from the traumatic impact. We performed it in a lot of patients and were quite pleased with the results we obtained, meaning lower instrument failure rates.

Still, various recent papers have failed to find a beneficial effect from the intravertebral spongiosaplasty. It is not unreasonable to suggest that the pedicle screw instrumentation itself has improved in the last two decades. That could be the reason that instrument failure has decreased enough to make the spongiosaplasty procedure become superfluous. In the early days of the internal fixator we were confronted with a substantial number of failures due to instrument fatigue. I think it was a useful additional procedure then.

Do you think (patient-) factors exist that are overlooked or ignored, that might influence the outcome of spinal surgery in general profoundly? Beside all the obvious factors, I would suggest that collaboration between physicians, or more specifically, the willingness to collaborate is an important factor for patient outcome. Furthermore, I think psychological and social factors of the patient influence outcome more than the type of treatment that is performed. Maybe, now that I think of it, bone mineral density might also play a role in deciding how to treat patients. Maybe young healthy patients with high BMD’s could be treated more conservatively. On the other hand the same holds for elderly patients with low BMD’s. It would actually be interesting to see if BMD could be a parameter in deciding how to treat patients.

Do you think that conservative fracture treatment is gaining popularity again and if yes, can you explain? No, it is not gaining popularity. It should however, because more cases could and should be treated conservatively. I have the impression that there are some hospitals where you are at risk of undergoing a circumferential fusion operation if you are diagnosed with a fracture that, in my opinion, could be treated conservatively. They sometimes seem to treat spines, not patients.

What do you think of the development of spinal surgery in the last three/four decades in general. Which ideas in particular do you consider to be good? What I consider to be good practice is wanting to know everything about the nature of the pathology of the patient’s spine. It is a prerequisite if you want to be of help to your patients. Furthermore, it is good practice if you know how to vary your treatment scheme according to the type of pathology. And of course, that is only possible if you know about its nature. Furthermore, and especially in degenerative disorders, we must consider, that psych factors may significantly influence the clinical symptomatology and thus the treatment modalities.

What would you consider bad practice today in traumatic spine fracture management? Over-treatment. As I just mentioned, when confronted with a spinal fracture you should choose your treatment according to the fracture type. In a lot of cases that would mean; ‘Stay off!’. But that is one of the hardest problems for a surgeon, to not operate on something that is broken.

What technique or type of treatment would you like to develop today if you were to start over now? Hmm, another difficult question. I would focus my attention on inventing minimal or at least less invasive techniques to be used intravertebrally or for interbody fusions. Implants that I would use in these techniques would be bio-resorbable, elastic enough to transmit physiological forces, but sturdy enough not to break during the initial healing period. Also, they would be radiolucent or at least not cause these awful artifacts on computed tomography or magnetic resonance images. It is a big nuisance when diagnosing problems in that area and you cannot see anything due to all these streak artifacts.

Do you see a place and/or function for robots and computer assistance in the OR? Yes, if you mean navigation; it is a useful addition. For very experienced surgeons it is really easy to insert screws through the pedicles. But even for them it becomes a lot harder when confronted with severely degenerated or scoliotic spines. The navigation techniques today are a little cumbersome to handle and it takes extra operation time but, in general, it would be beneficial to the patient. Every technique that holds the potential to decrease iatrogenic damage should be considered and tested even if it is not perfect yet and not very cheap. Just think what costs are involved, if we for arguments sake ignore the much more important social drama, if you cause a paraplegia in one patient; you could buy an OR full of navigation devices for that. The matching process of the navigation is still the weak point however, one should improve on that. The use of skin markers and modern imaging techniques, like the rotational X-ray imaging modality that you are involved in, might provide a solution. Robots I am not familiar with. I cannot think of a purpose for a robot in spine surgery right away. We will see what will happen.
Currently, spine surgery is performed by orthopaedic surgeons, neurosurgeons and sometimes traumatologists. Do you foresee the advent of a separate specialty: spine surgery?

Absolutely. And it would be a big step forwards. Just like urologists and thoracic surgeons, for example, are mainly concerned with one organ or system, some of the surgeons you mentioned above, should be trained thoroughly in spine surgery. The results from the past show that it takes experience and practice to become good in this work. Neurosurgeons do not understand or handle bone as well as orthopaedic surgeons, who, in turn, do not understand so much of the central nervous system, which is important for decompressions for example. Combining their knowledge for the treatment of spinal pathology would be advantageous. So I would strongly advocate for an ‘official’ spine specialism, definitely.

In our center we are currently considering treating some traumatic fractures (A-3.1 and some A-3.3s) by reduction and stabilization with the external spine fixator and subsequent balloon vertebroplasty with calcium phosphate cement. The Schanz screws are to be removed after the cement is cured. What do you think of such a treatment?

Sounds interesting, you probably already asked yourself at what time you can safely remove the external fixator considering the various types of fractures and whether the reduction will be maintained after fixator removal?

Yes, we did and do not know the answer, do you?

Well, no but you just have to try, I think. I know that in the ‘70s and ‘80s it was probably easier than today with all these institutional review boards watching over your shoulder, but in the end you just have to learn from the experience you gain by treating patients. Not everything can be revealed by cadaveric or animal experiments. You just go ahead and tell us in five years if it was a good idea or not.

References
