Reviewer's report

Title: Surgical treatment of infective endocarditis in active intravenous drug users: A justified procedure?

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Reviewer: Michael S Firstenberg

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Weymann and colleagues present their experience with the surgical management of patients with infective endocarditis who are still considered active drug users at the time of presentation. Of the 451 patients who underwent surgical treatment for endocarditis, they present the 20 who had active IVDA (<5% of the total cohort). Their follow-up of 2504 +/- 1842 days was impressive as many studies on the topic only focus on short-term outcomes. In addition, their long-term survival at 1, 5, and 10 years (90%, 85%, and 85%) is impressive and provides justification for intervention in a complex patient population. No patients required re-operation and several variables were identified for risk for long-term mortality. It must be remembered that this experience is in Germany.

While this is an interesting paper and offers good insight into the justification for management of a challenging patient population, I have a few concerns that need to be addressed (both small and large issues) prior to feeling this manuscript being suitable for publication.

1) The authors comments that IE in IVDU is different than the general population – a population which I assume is a general population with endocarditis (???) – yet they do not prove this or elaborate further than a single reference. How is the IVDU sicker or different than some of the other patients that they operated on for IE? It is the experience of this reviewer that sometimes non-IVDU patients tend to be sicker because their IE is secondary to their co-morbidities, such as indwelling pacer/ICD leads, dialysis lines, chronic immunosuppression rather than a younger population sharing needles. (MAJOR)

2) The authors describe a standard surgical technique to the management of IE – is their management any different in IVDU different than non-IVDU patients? (MINOR)

3) In their methods, they describe patients with active IVDU – this needs to be better clarified – do they mean using drugs up until the day of admission or a recent history. Furthermore, they comment on drug use history – but include some drugs that are typically not administered via an IV route – i.e. marijuana (smoking), cocaine (inhaling), amphetamines (oral), ecstasy and methadone (oral) – even though many of these can be administered in an IV form. Was this considered – for example endocarditis from smoking marijuana might be different than IE from sharing needles and heroin. (MINOR)

4) In the introduction, the authors describe IVDU has having more complex
disease – however in their experience (of 20 patients out of over 450) most patients only had single valve involvement with 40% being tricuspid. They mention 1 valve repair – but do not comment on whether more aggressive reconstruction was needed in any patients (as was outlined in their description of surgical approach). (MAJOR)

5) The authors describe 12 patients - >50% of their patients – as needed simultaneous splenectomy. This is an interesting and unusual approach to the problem of a splenic abscess. Was splenectomy performed at the same time as cardiac surgery or at a different time? What were the indications for intervention in this group. (MINOR)

6) Their results were very good – short and long-term. But, this is where I have my concerns. It is the experience of this reviewer – and in discussion with colleagues – that our collective experiences show a very high rate of recurrence in this population. Patient continue to use (abuse) drugs, re-infection rates are high, mortality from drug abuse is high and patient are lost to follow-up and disappear only to be presumed to be dead. Hence – how did the authors achieve success or how is their system able to “cure” the drug abuse? It is generally viewed the IE from IVDU is a catastrophic and often end-stage problem because of the poor survival rate of long-standing abusers of IV drugs. Please elaborate on this more and speculate why the outcomes are better. How good is their national database as a source of death or are they just assuming that if they are not listed as being dead, they they are still alive – I must question the accuracy of this method considering this patient population. How many patients had actual face-face follow-up contact during the study period? (MAJOR)

7) An additional concern is the conclusions – the multivariate analysis that is performed on the outcomes of 20 patients. It is unclear as to whether this is a large enough patient population to support definitively the conclusions – particularly considering the number of patients at risk at >10 years (MAJOR)

8) The graphs at the end while somewhat interesting, are in general probably not necessary and contribute to making the manuscript longer than necessary. (MINOR)

9) Of the variable that did predict 90d death – operative duration (but not cross-clamp or bypass time, interestingly) was predictive – why was this so? (MINOR)

10) HIV status was predictive – but what was the extensive of this disease burden? How many had end-stage HIV, active treatment, low CD+ counts, high viral loads, etc. Any insight into this would be useful – although there was only 1 patient in each group and hence definitive conclusions are difficult to make. (MINOR)

In general, the authors describe that this is their experience with a high-risk group, but they offer no comparison. How did the 20 with a history of active IVDU compared with the >400 other patients they took care of who didn’t have active IVDU? Similarly, was there any data on risk stratification of these patients – such as Euroscore – to help put some of their comorbidities into context? A review of an experience of 20 patients probably should include much more comprehensive
assessment of risk factors and some of the other problems these patients encountered to get the excellent outcomes. How many were discharged home? How many had recurrent admissions for endocarditis but were not operated on (many surgeons will not perform re-ops on patients still actively using)? What was the post-op drug treatments did these patient participate in? This reviewer is also surprised that over a 20 year period that only 20 patients with active IVDU were operated on. This reviewer and his colleagues are seeing an epidemic of this disease in our practices (and far greater than 5% of all endocarditis patients are active IVDU). Where there many cases of IE with IVDU admitted to this hospital that did not undergo surgery? If so, why not and how did these patients do? Given the small patient population studied, there probably should have been a comparison group to better understand how these patients should be managed especially since it is unclear if this truly represents a comprehensive cohort. (MAJOR)

Overall, this is a very interesting topic on a challenging patient population but the number of patients studied over a 20 year period make definitive conclusions difficult to relate to current practice experiences – especially if the question is whether these patients should be managed differently or if their outcomes are different.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I have no competing interest or conflicts related to the publication or review of this manuscript.