Reviewer's report

Title: Determinants of hospitalization for cutaneous injection-related infections among injection drug users: a cohort study

Version: 3 Date: 16 January 2010

Reviewer: Dan Ciccarone

Reviewer's report:

This is a re-review of: “Determinants of hospitalization for cutaneous injection-related infections among injection drug users: a cohort study.”

I have carefully read the first line reviews, authors’ responses and the current paper and find myself of two minds. One, the rewrite is very response to many of the reviewers’, including my, concerns. The authors have clearly worked hard on this revision. In addition, I believe nurse interventions are important and this one seems of likely value to the community served. The SIF-based nurse intervention also appears likely to lead to increased and/or sooner hospitalization for the participants.

On the other hand, the inference on the reduction in hospital length of stay by nurse intervention is less than clear given the data:

• Length of hospital stay is likely related to two main reasons: patient delay (infection gets worse) or occult infection (more difficult to detect, more insidious and challenging to treat once detected). Nurses are likely intervening at the social level by reducing personal, social or institutional barriers to acceptance of hospitalization (patient) or admission (ED staff). It is unlikely they are more astute than hospital staff at picking up occult infection. The result: nurses are likely screening out more obvious, visible (vs. occult) or acute (vs. sub-acute) diagnoses and aiding those persons into inpatient care sooner than they might on their own.

• The response to this concern was addressed with the statement: “Individuals with an ED visit who were referred by a nurse were more likely to be hospitalized within three days (P=0.011).” This statement doesn’t address the concern of selection bias in an intervention with heterogeneous outcomes including clinical outcomes that are both easy and difficult to diagnosis. The data point simply confirms the concordance of nurse and ED interpretations of illness. It neither supports nor denies the negative bias towards occult infection and hospitalization or positive bias towards visible and/or acute infections. It only supports concordance. ED physicians have difficulties diagnosing endocarditis too, esp. given stigma towards treating drug users.

• Just to be clear about the clinical conundrum of occult and/or sub-acute infections: these infections are difficult to diagnose because they do not have obvious clinical findings of fever, pain, heat or deformity. Earlier stages might be unrecognized, or ignored by the patient, and be unconvincing to the nurse or ED
attending. The clinical work-up to diagnosis can be cumbersome and may be delayed by reluctant parties. These infections are likely to “brew” longer, entrench deeper and when they finally manifest, lead to more serious and lengthy hospitalizations.

Another inference conundrum: HIV and CIRI. The authors found that HIV seropositivity was a predictor of hospitalization for CIRI. Yet the reasons stated, “susceptibility to bacterial infections,” and “high-risk injection practices,” are predictors of infection - not hospitalization. The same authors however did not find this in previous analyses. The stated reasons are not a reasonable explanation. The reasons for greater HIV admission is that HIV seropositivity is an interaction variable on the pathway towards greater clinical concern and increased likelihood for decision for inpatient management vs. outpatient management. E.g., two similar patients with similar degrees of cellulitis, the one with HIV is more likely to get admitted.

Minor essential revisions:

Tables 2 & 3 add value to the findings of the paper. However they need better labeling and perhaps, detailed footnoting in order to aid interpretability.

There are still a number of “to hospital” phrases whereas “to the hospital” was agreed to be better. (First para. of results)

Discretionary revisions:

That this analysis didn’t find heroin to be a risk factor is interesting and worthy of discussion. Type of heroin was brought up by two reviewers. Given the interest in the literature and treating community in type of heroin and CIRI, some discussion could take place in the discussion section. Papers that have suggested heroin as a predictor of soft tissue infection include Murphy, et al CID 2001, Passaro, et al JAMA 1998, Binswanger, et al CID 2000, among others. For a detailed discussion of heroin type and consequences: see Ciccarone, “Heroin in brown, black and white: Structural factors and medical consequences in the US heroin market,” IJDP 2009.

Conclusion:

This is a sound study with important findings and implications. I am only concerned with one major inference, but it is an important one! I strongly urge a revision in the limitation section similar to this: That while nursing intervention is of clear value in reducing barriers to hospitalization, the finding of decreased length of stay must be interpreted with caution. Selection bias towards more visible or acute infections is possible, leading to a selection bias of CIRI with reduced length of stays (e.g., abscess vs. endocarditis). (No qualification)

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a
statistician.

Declaration of competing interests:
I declare that I have no competing interests