Another NICE chapter in interventional cardiology: but it's time for responsible debate

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So now we know ... drug-eluting stents (DES) are not to vanish from our armoury at the whim of a National Institute for Health and Clinical Excellence (NICE) appraisal committee containing not a single specialist in interventional cardiology. When the provisional guidance report was published in the fourth quarter of last year, stating "*Drug-eluting stents are not recommended for use in PCI in patients with coronary artery disease*" as point 1, paragraph 1, it was really difficult to comprehend how the committee had come to this ridiculous conclusion.

Most interventional cardiologists can understand that there is a need to audit the use of novel technologies in order to ensure the maintenance of patient safety and protect Department of Health (DoH) expenditure. Not only do we understand it, but we are committed to such a process. The preliminary judgement from this NICE group, however, was in my opinion, a significant low point in the process of appraisals. The fact that the final appraisal determination now states, *"Drug-eluting stents are recommended for use in PCI for the treatment of coronary artery disease, within their instructions for use, only if: (a) the target artery to be treated has less than a 3 mm calibre or the lesion is longer than 15 mm, and (b) the price difference between DES and bare-metal stents is no more than £300"*, is undoubtedly a relief if you are committed to patient care at the front line, although it raises questions about the appraisal mechanism.

There appear to be two schools of thought in relation to this latter issue. First, that the original analysis demonstrates that the process undertaken was fundamentally flawed in its design:

- expert witnesses only contributed to the appraisal rather than being built in as a core component of it
- there was a dependence upon health economists whose previous analyses betrayed an important degree of anti-DES, pro-coronary artery bypass graft (CABG) bias.

Such factors would allow the more cynical to predict a negative appraisal, even in the face of powerful randomised and observational data to support clinical benefit: data that was never disputed, and in fact clearly stated, in the preliminary document.

Second, by contrast, is that it is a triumph for the design of the appraisal process that there was such a substantial turnaround in the conclusion reached after the public consultation exercise. I am sure that NICE will claim vindication for the process based upon this argument.

The position now

So where does the current guidance leave us? First, it provides interventional cardiologists enough freedom to be able to treat most of our patients in what we consider to be an evidence-based manner. This desire to provide optimal care for our patients has been, incidentally, repeatedly and insidiously questioned over the last 12 months – but I will return to that issue later. In fact, I know that I am not alone in feeling that the guidance should have included diabetes as an indication for DES independent of the 3.0 mm/15 mm parameters. Are there any large observational or randomised series of stent activity that do not describe diabetes as a predictor of restenosis?

Second, and by virtue of the first point, it will spare a large number of patients around the UK from CABG surgery when they could have had percutaneous coronary intervention (PCI) with DES. Few interventionalists would have been able to stomach treating high-risk cases (narrow vessels, long lesions, bifurcations, chronic total occlusions [CTOs]) with bare metal stents (BMS) and hence returning to the bad old days of high rates of clinically significant restenosis. Can a group of individuals on an appraisal committee fully understand the value to real-life patients of reducing restenosis in the real world? I suspect not.

The 'life event' aspect of re-presentation and the requirement for a further procedure cannot be

effectively measured in a health economy model or a chart of mortality but, nevertheless, is of enormous importance to patients and their families! Furthermore, we have had extensive opportunity over the last nine months to repeatedly see that comparison of BMS and DES arms in pooled data from randomised trials yields no difference in the combined end point of myocardial infarction (MI) plus mortality, despite the now generally accepted increased rate of late stent thrombosis in the DES populations. We surely have to conclude that there are early events in the BMS populations that are later balanced by stent thrombosis. It is likely therefore that restenosis in BMS is not a benign entity. Thus, had DES been removed from our treatment options, then a lot more patients would have been referred for CABG than for BMS PCI ... and I didn't see any inclusion of the logistic or financial implications of this potential change in trends of treatment in the NICE modelling. Had this consequence of their recommendation even occurred to the panel?

Third, it leaves us with a suspicion of the NICE mechanism of appraisal. This is an important negative and should not be underestimated. Interventional cardiologists have access to an unparalleled body of clinical evidence that accumulates with extraordinary speed and variety. We have, as a group, undoubtedly been guilty of extrapolating from clinical trial results to our real-life practices even when the data are not necessarily relevant to the patients in front of us. However, to deliberately alienate us from the appraisal committee seems unlikely to facilitate a clinically appropriate guidance document. Interventional cardiology, as I mentioned earlier, has come under some heavy fire in the last few months. A negative perception of the subspecialty has smouldered ... and at time this has been fanned into full blown flames of hostility by various factors. One such is the provisional NICE guidance.

Responsible reporting

Another example is the editorial by Professor David Taggart in the *BMJ* in March 2007.[1] To put forward a robust and at times mischievous argument in favour of CABG surgery versus PCI is exactly what one would expect of a Professor of Cardiac Surgery. However, for an apparently important journal, with a general readership, to publish these views without automatically including any balanced redress was, in my opinion, highly irresponsible. Perhaps if I quote some of the article and analyse this in the context of its possible interpretation by general physicians, commissioners and, most importantly of all, patients, then the degree of this irresponsibility can be appreciated?

- The subtitle of the piece is the first example, *"Surgery is effective on clinical and economic grounds, but stenting does not seem to be cost-effective".*
- *"Griffin and colleagues highlight the tension between the adverse economic implications of the phenomenal growth in stent procedures and the absence of an appropriate evidence base to support such a policy. More importantly, this strategy has denied many patients with multi-vessel disease the prospect of a better long term outcome in terms of survival and freedom from reintervention offered by surgery."*
- *"Disquiet about the lack of improved outcome with DES, despite their increased costs, has recently been superseded by concerns about the increased risk of late stent thrombosis and its high mortality."*

I do not need to rehearse the counter arguments to these assertions in this article, and this has been done by colleagues on behalf of the British Cardiovascular Intervention Society (BCIS) already.[2] However, the concern has to be the potential damage done to the patient or commissioner who is not armed with a counter argument, but a newspaper headline necessarily pregnant with sensationalism! As it happens, the cost-effectiveness issue seems to have eventually been resolved by the NICE appraisal guidance! Furthermore, my reading of the literature is that in randomised trials comparing CABG with stent, there has never been robust evidence of survival or MI advantage. As I have already discussed, there is undoubtedly a noise that late stent thrombosis occurs in DES at a rate of around 0.5–0.6% per year but this is balanced by events in the BMS groups early, possibly because of the higher rate of restenosis. Interventional cardiologists are actively seeking to understand this complication better.

In summary, my criticism is not focused upon the predictable anti-stent line peddled by David Taggart. In fact, he makes some excellent points about the importance of multi-disciplinary decision making, but rather that damage can be done if such extreme views should be published without balance. Where in his editorial, for example, does he address the issue of high-risk patients being turned down for CABG? This is a frequent and familiar occurrence for the interventional cardiologist.

Perhaps a third example of the negative perception of interventional cardiology practice surrounds the fall out from the publication of the Clinical Outcomes Utilizing Revascularization and Aggressive Drug Evaluation (COURAGE) trial.[3] Again, extreme views have caused alarm among patients. Loud voices, led by Professor Mike Chester, have been loudly heard. Again, I am comfortable that the process of debate is healthy. COURAGE held few surprises for me and nor did its findings cause a change in my practice because I have frequently declined to stent patients referred to me if they are free of symptoms, regardless of objective evidence of ischaemia, on the basis that there is no prognostic benefit. I have some sympathy with the fundamental concern that Mike Chester raises that the discovery of a coronary stenosis has become an inevitable trigger for revascularisation. But the critical thing is to provide balance when these important debates are engaged ... otherwise it is the patients and their families who get caught in the fall out.

Now, isn't it time NICE assessed our treatment of acute coronary syndromes? ●

Conflict of interest

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