Professor John Strang

A series of three recent papers in *The Lancet* summarised the findings of a six year project published as 'Drug Policy and the Public Good' published in 2010. John Strang was part of that project and here we discuss the general process of gathering and reviewing evidence in an always controversial subject area.
Interview by **Harry Shapiro**

Can you give some background to the Drug Policy and the Public Good (DPPG) project?

It built upon the work of an alcohol project which began in the mid 70s. The international scientific community were beginning to understand things about alcohol, alcohol policy and health and it was thought that these should be written down in a way that the ordinary, intelligent reader could understand. The book *Alcohol and the Public Good* was published in 1994. It was very influential and was attacked by the alcohol industry: they didn't like the conclusions and they deliberately went out of their way to discredit it by offering to pay researchers to write negative reviews (see also News page 2). It was updated in the book *Alcohol: no ordinary commodity*. The books laid out, in plain English, the 'Known Knowns' of the alcohol policy field. From that emerged the idea of *Drug Policy and the Public Good*.

What was the underlying principle behind it?

The objective was to bring scientific rigour to bear – what conclusions can we come to that are clear and true – whether the news is good or bad? And let's tell the reader where we haven't much to contribute at all. It summarises the strength of the collective body of evidence.

So in the prevention arena, ten years ago we would have had to say that we don't know whether anything makes much difference, but nobody is quite sure. We can now say that there is a strong evidence base for only minimal impact. People might go, 'well that's no different'. But it's massively different – previously we didn't have evidence of impact – now we have good evidence of minimal impact. The same applies to much of the activity around law enforcement . And with other areas, such as residential rehabilitation, even though the intervention maybe very popular and widespread and lots of people maybe passionate about it, we actually find only a modest body of high quality evidence of effectiveness.

Presumably there were people even within the group who were unhappy with the messages?

Absolutely. All of us had attitudinal baggage and we were all aware of that. So there were people who had worked with Democrats, with Republicans; people who had worked to introduce law enforcement in different countries, drug courts or who had run prevention programmes – and part of the rites of passage of being part in the group was to hang those pre-formed conclusions at the door – and if I drift towards being a campaigner, for example, you are mandated to stop me. Those were the rules.

Just thinking about how policy change happens though, isn't it true that politicians are really only motivated to act in a crisis rather than take a considered, objective view of the evidence? I'm thinking for example harm reduction in the UK as a response to the threat of HIV/AIDS to the wider community or a similar response to Needle Park in Zurich?

Yes, you're right, the time course doesn't always permit careful consideration of the evidence. So when the first needle exchanges opened in 1986, I was the newly appointed advisor to the Chief Medical Officer and the discussion was – do we shut them down or roll them

out? And the answer was a classic civil service compromise of using the two that had opened as a monitored research project, while they slowly expanded – and that actually became Gerry Stimson's research enterprise.

But policy change can happen in the light of evidence. Supervised heroin clinics in Denmark are a good example. They examined the recently-published results from well-designed trials in other countries and considered whether they needed to do any research – and decided that the research evidence base was sufficient to go ahead.

So what constitutes robust scientific evidence in our sector?

Scientific rigour is often mistakenly framed in terms of rigidity – people mistakenly think that you must always have randomised controlled trials (RCT) or double blind studies. That's completely wrong. What we were trying to encourage was buying into the scientific process, not any particular way of doing it. Obviously you can't do RCT studies for everything, but that doesn't mean you abandon scientific rigour in the search for a causal relationship.

A situation arises and you say – what is the best we can do in the compromised environment? Can we devise a study so that, in three years' time, we can look back and check to see if it was the right judgement call? So an example would be in the mid-1990s a number of us came to realise that unsupervised prescribing of methadone was probably contributing to a rise in methadone deaths. We had many more methadone deaths than other countries per capita and we were distinctive in that we didn't pay much attention to supervision – so maybe there was a correlation. A decision was made to introduce supervision in Scotland and then, a few years later, in England. And about ten years later we worked out that we could test if that was a right judgement. It wasn't a randomised trial, but it applied the rigour of science. Deaths per million dispensed doses dropped massively and we worked out that around 2500 lives were saved. You couldn't have done an RCT on that but it was good-quality science nevertheless

You have recently started a large prison-release take-home naloxone trial. But don't we know already that naloxone saves lives?

Yes, we know that naloxone works (spectacularly). But we don't know for certain, not 100%, that it saves lives overall. So in the public debate, some people will be saying, well that just encourages people to inject drugs because they know they have an antidote. For example, we don't know that giving somebody an advance pack will save lives. They might not have it on them when they need it or whoever is with them might be frightened to give it or it could even have an unintended negative consequence, by generating a sense of safety around injecting heroin or using more heroin. I don't think that will happen, but we need to test it out.

This one will be an RCT. In the trial 50% of people will be given a DVD about how to manage overdose plus an emergency dose of naloxone and the others will just be given the information. And in our view and the view of the ethical committee, it is ethical because at the moment, giving out naloxone is not current practice, so you are not withholding that from anybody. We are bringing an experimental intervention which we think will reduce deaths. If you can genuinely demonstrate lives saved, then that answers any criticisms that it doesn't work. Whether it subsequently gets widely implemented, is another issue that is then the next step in the translational process

So let's talk about the impact of the research process.

The best example in the UK would be NICE. The mind set is similar to that which existed within the DPPG group. NICE will say 'there are some key issues in the treatment field where we think there is evidence there to be gathered and we are going to do that in an authoritative scientific synthesis'. NICE has a top grading of reviews called a Technology Appraisal (TA); TA116 was to do with methadone/buprenorphine maintenance and because it is a TA, and if NICE say it is both clinically and cost effective, it is essentially mandatory for the NHS to provide it. The next level down is a CG or clinical guideline which is not quite compulsory but there must be reasons why you don't follow it.

The NICE process also says what you shouldn't do (or at least raises questions about the evidence base and challenges whether it should be done). We looked at CBT specifically for the treatment of dependence and as far as we could see there wasn't any evidence that CBT was of any use in the treatment of the dependence itself although it had clearly been shown to be of use in the treatment of co-morbidities like depression/anxiety. At best, the impact on dependence and associated behaviour was marginal – and we contrasted it with contingency management with which there was evidence of an amount of benefit (an "effect size") that was much larger. Now that upset a lot of people, but it is what the evidence found.

Going back to the DPPG; your conclusion that neither enforcement nor prevention had much impact was really questioning two thirds of most government drug strategies (the other being treatment). What has been the impact there?

It wasn't our job to make the final decisions about what drug policy should be. Some of us found that difficult because with other hats we might be on committees or government advisory groups and some of the people sat around the table had sat in government departments. Our job was to identify clear conclusions that should inform the public debate – not to tell the public what to do.

Through the book we want to ensure that the reader understands that however much you allocate to certain activities (like enforcement or prevention) don't expect to get a return on it. And there are big differences in the "effect size" of different strategies (in the different chapters) as well as between different methods (within each chapter). These are simple messages but also profound; simple in that the conclusions where they exist are strong, but they are particularly profound because they are in conflict with what we do.