sequence. While 104 consultations lasted five minutes or less, 88 lasted over 20 minutes. The number of patients seen within half an hour of their booked appointment time, without correction for other factors, was 228 (55%).

The survey found hospital practices that caused delays; these have now been altered. Five of the 15 clinics started more than 10 minutes late because medical staff were completing ward rounds or attending lunchtime meetings. The computer booking system was programmed to book both old and new patients at five minute intervals.

As patients and their relatives have to wait in the outpatient department the facilities provided should be reasonable; 108 patients found the seats uncomfortable, 112 did not know where the toilets were located, and 50 did not know where to obtain refreshments. Surprisingly, only four patients complained specifically about waiting times. Altogether 277 patients were over retirement age, and their apparent lack of concern about waiting may reflect an older generation's acceptance of queuing and the fact that most were not taking time off work. Also, 201 were seen by nursing staff before their medical consultation in order to check visual acuity, dilate pupils, or test visual fields. They therefore probably considered that they received attention without much delay.

We agree with Ansons and colleagues that the current targets for waiting times are unrealistic for many busy eye clinics.

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Increasing the accessibility of data

EDITOR,—George Davey Smith's editorial on increasing the accessibility of data is too optimistic about the power of sharing data to arrive at "truth." Sharing data can be an impediment to truth. That happens when people start arguing in such detail about the "right" analysis of data that outsiders give up and conclude that there is no evidence.

An epidemiologist once told me that he would never share his data with an opponent since that person would almost certainly find some decision concerning the analysis of the data or some misclassified subject to decry in public—preferably to a mass media audience. The recent slandering of careers that has occurred as a result of some errors in data from clinical trials—which randomised trials tolerate—serves as a good reminder, even if the origin of the errors cannot be excused.² A notorious case in which not only truth and careers but also public health suffered was the reanalysis and reinterpretation of data from the first case-control study of Reye's syndrome and salicylates. It destroyed the study's credibility in the eyes of many. Only many years and many patients later did new and costly studies show that the original study had been right.³ It is no solution to say that the intention of a reanalysis should not be irresponsible or malevolent, because to the true Popperian the intention of an argument is not important, only the argument itself counts.

Davey Smith concedes that in observational research the ability to reach different conclusions based on the same data is recognised.⁴ Randomised trials do not, however, escape subjectivity. It suffices to read the literature published after the trial of global utilisation of streptokinase and tissue plasminogen activator for occluded coronary arteries (GUSTO) to see that even members of the same trial board, who had access to the same data, can greatly differ over their interpretation. This is not new in medicine and epidemiology. Some useful lessons about analysis of data were learnt from the protracted discussion about the university group diabetes programme trial: repeat analyses of data and discussions took more than a decade, only to arrive at deadlock.⁵

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Research into outcomes and effectiveness

EDITOR,—Trevor Sheldon's critique of the research into the outcomes and effectiveness of treatment carried out by the US Agency for Health Care Policy and Research and, in particular, the patient outcomes research teams, which are supported by the agency, is based on a fundamental misunderstanding of our programme, mission, and methods.¹ The US Congress created the Agency for Health Care Policy and Research in December 1989 to focus on a set of important issues that were not being addressed by existing research institutions. Specifically, the law establishing the agency and the medical treatment effectiveness programme mandates a new, hard look at the effectiveness and appropriateness of current health care practice in a wide array of clinical conditions. It requires examination of a broad range of outcomes (not merely mortality and morbidity but relief of symptoms, functional status, quality of life, and cost) as achieved in non-selected patients and practices.

Many of the agency's studies of outcomes focus on conditions and procedures that are not life threatening but that impact appreciably on the quality of life—that is, low back pain, cataract, benign prostatic hyperplasia, and hysterectomy. Because many of these conditions and procedures have not been studied before, the agency's first generation of studies invested heavily in fundamentals—documenting practice patterns, showing the need for better evidence, identifying the relevant outcomes, and developing research methods and measures of outcomes. These building blocks are important in their own right, and collectively they have led directly to the development of a set of rigorous studies to test hypotheses.

For example, work by the patient outcomes research team on prostatic diseases was critical in showing the weakness of evidence supporting current treatment for benign prostatic hyperplasia and in convincing the clinical and research communities of the need for a randomised trial. The team developed measures of the symptoms and treatment outcomes specific to the condition that are important to patients and a symptom scale for benign prostatic hyperplasia that has been

validated and adopted by both the American Urological Association and the World Health Organisation. In collaboration with the American Urological Association the team has designed and obtained funding for a randomised controlled trial that will compare directly the effectiveness of transurethral resection with two drugs (finasteride and doxazosin). Similarly, the randomised controlled trials in prostatic cancer, back pain, and cataract that are now under way in the United States, as well as a case-control study by the patient outcomes research team for cataract, would never have been undertaken without the preliminary analyses by the teams.

Systematic, critical reviews of the literature on their topics is another important contribution of patient outcomes research teams. The conclusions of these reviews, while often disappointing with respect to the quantity and quality of the literature, provide valuable documentation on the state of the evidence regarding the effectiveness of different treatments and have led to some fundamental changes in thinking about what is critical to study. Proponents of randomised controlled trials and traditional meta-analysis who have criticised attempts by the patient outcomes research teams to synthesise the findings of non-randomised studies should appreciate the contribution of the teams in documenting the non-existence of randomised controlled trials and, in some cases, the poor quality of trials that have been done.

In clinical areas where solid evidence of the benefit of treatment exists, investigators supported by the Agency for Health Care Policy and Research are determining whether these benefits are routinely achieved. The patient outcomes research team for acute myocardial infarction has shown that a significant number of patients eligible for thrombolysis do not receive it.² The agency's mandate in research into outcomes does not stop with the publication of a study showing effectiveness but requires analyses of practice to determine whether effective treatments are used routinely. If they are not, strategies must be developed to change practice.

For many clinical conditions, when multiple outcomes and alternative treatment strategies are considered, research (even randomised controlled trials) can seldom identify a single, "correct" decision regarding treatment. At best the evidence to guide decisions will be in the form of a set of probabilities associated with the various risks and benefits. Large simple trials offer little help, because the outcomes that need to be measured are seldom simple.

Unlike death, relief of knee pain or changes in sexual function, visual function, or affect are difficult to measure, and information has to be obtained from patients—a process that is neither simple nor inexpensive. In the absence of clear answers about best treatment the preferences of informed patients must play a major part in clinical decisions. Patient outcomes research teams have drawn attention to the critical dimension of patients' preferences and have shown the capacity of the teams' approach to obtain good information about these.

While we certainly do not believe that randomised controlled trials are a panacea, neither do we propose observational studies as a substitute for experimental methods. Observational methods can help to define the critical constructs and the hypotheses that need to be tested experimentally and determine whether practice patterns are consistent with evidence of effectiveness.

Experimental studies certainly have a place in research into outcomes too. This is because research into outcomes is not, as Sheldon suggests, a method; it is a conceptual framework for studying the relations between health care services and their outcomes. It uses multiple research designs and methods drawn from the full array of epidemiological and clinical research.

As a new theoretical and methodological enter-

prise, research into outcomes is still evolving; we believe that it will benefit from constructive criticism and debate on both sides of the Atlantic.

Our external review of patient outcomes research teams in February 1993 led us to streamline the programme, moving more directly to hypothesis testing and telescoping the efforts to describe existing practice patterns and the state of the evidence. This new approach was reflected in a June 1993 research solicitation by the Agency for Health Care Policy and Research that signals a programme more heavily focused on hypothesis testing. This change is in emphasis, not in concept, for there will always be a need to understand existing practice patterns and to develop outcomes measures before experimental studies can be designed and mounted. We are not choosing between randomised controlled trials and all other methods; rather we are encouraging the efficient application of the appropriate methods to identify critical hypotheses and then test them.

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Usefulness of comments of previous peer reviewers

EDITOR,—In asking for the comments of previous peer reviewers when papers are submitted to the *BMJ* the journal does many conscientious authors a disservice.¹ Authors often either alter or completely rewrite their paper, taking into account criticisms that have been raised. Any further submission should be judged only on its own merits.

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1 Minerva. *BMJ* 1994;309:550. (20-27 August.)

Identifying conflict of interest

EDITOR,—The letter from Peter Lee, a consultant in statistics and epidemiology, has a footnote saying that he is a consultant to the tobacco industry.¹ Should readers infer that his assertions are somehow less professional, less objective, or less accurate? May we now expect all letters from the many advisers and consultants who serve the interest of Action on Smoking and Health and other antitobacco pressure groups to have similar footnotes? I hope so, otherwise the *BMJ* is open to the charge that it employs intellectual discrimination.

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****Conflict of interest is a condition, not a behaviour, and when we attach a statement of conflict of interest to a piece we are not saying that it is less worthy than a piece with no such**

statement attached. It may or may not be, but we leave readers to make up their own minds. Our policy is to disclose conflict of interest, not to try to abolish it. If we knew that the author of a letter on smoking was a member of Action on Smoking and Health then we might well attach a statement of conflict of interest. Often correspondents are only too keen to declare an association with that organisation.—ED, *BMJ*.

Returning to work after herniorrhaphy

EDITOR,—We disagree with several points made in the editorial on "returning to work after herniorrhaphy."¹ The authors state that "troublesome wound soreness is rare seven to 10 days after operation," but we have shown that 15% of patients experience some groin discomfort up to six months after surgery.² This may be related to the repair technique used: the authors of the editorial use a mesh to reinforce the posterior wall of the inguinal canal, whereas most British surgeons use either a darn or a Shouldice-type repair,³ both of which are probably more painful. Therefore, the advice given by the authors is inappropriate for most patients undergoing herniorrhaphy in Britain.

We also disagree that it is counterproductive to warn against strenuous activity too soon after surgery. Such activity may cause severe pain, making the patient wary of attempting more vigorous efforts later and thereby causing a delay in eventual return to normal life. Finally, it has been shown that driving should be avoided for at least 10 days after herniorrhaphy, not because of a risk of recurrence but because of delayed reaction time in the affected limb.⁴

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Treating an engorged penis

EDITOR,—The report by S G Vesey regarding removal of a metal ring from an engorged penis¹ neglects to point out an alternative and simpler method—namely, puncture and exsanguination of the glans. The glans is punctured with one or two wide bore needles and exsanguinated by proximal to distal binding with ribbon gauze. We have used this method successfully to remove a chrome-vanadium steel ring spanner, which could not be cut without further injury resulting.

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1 Vesey SG. Minerva. *BMJ* 1994;309:550. (20-27 August.)

Oral infection and surgery

EDITOR,—Christos A Bartzokas and colleagues report a relation between oral infection and haematogenous infection after total joint replacement operations.¹ We recently reported a study of oral health in patients undergoing vascular surgery, which showed a high incidence of important periodontal disease in these patients; we postulated that this might be a cause for some cases of infection of vascular grafts.² Like patients receiving joint replacements, patients having vascular surgery tend to be elderly.

Patients with prosthetic heart valves have traditionally been given clear instructions relating to dental health and antibiotic prophylaxis for dental procedures. Patients undergoing equivalent vascular surgical procedures, however, like patients receiving joint replacements, are much less commonly given such advice. We suggest that Bartzokas and colleagues' recommendations concerning preoperative dental check ups and postoperative oral care in joint replacement surgery should be extended to include patients having vascular surgery. Since joint replacement operations and vascular grafting are carried out in most district general hospitals perhaps such hospitals should issue an advice card that patients could show to their dentist at check ups and that recommended appropriate antibiotic prophylaxis.

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Purchasing vascular services

Independent advice needed

EDITOR,—The paper by J A Michaels and colleagues on the organisation of vascular services and the accompanying commentary by Roger Greenhalgh highlight the need for the rationalisation of vascular surgery services into larger specialist units.¹ A working group for the National Medical Advisory Committee in Scotland came to the same conclusion. It also specified the minimum requirements for such a unit, which included an emergency rota fully staffed by surgeons with special skill in vascular surgery; access to an intensive care unit in the same hospital; and the availability of well equipped operating, radiological, and vascular laboratory facilities.² The implication is that a unit that does not meet these requirements may fail to reach appropriate standards of safety and effectiveness.

In their letter on behalf of the Vascular Surgical Society of Great Britain and Ireland, John H N Wolfe and colleagues indicate that, despite a need for adequately staffed and equipped vascular units, some trusts continue to appoint singlehanded vascular surgeons and do not provide the minimum requirements for a modern vascular unit.³ Although trusts are rightly criticised for these actions, the continuation of such a service must also indicate a failure of purchasing. The purchasers, whether health commissions or fundholding consortiums, could be blamed for not buying an accepted and appropriate quality of service and could presumably face litigation. Purchasers would thus be well advised to take note