The editorial suggested that this must at least question the Black Committee's statement. The authors of the letter agree this main finding in their second paragraph.

There is of course no question of Dr Crombie's data measuring health needs and he did not claim to do so. We must all hope for a future study on this very important subject.

Drs Wilson and Madeley believe that 'Occasional papers should surely be seen as attempts to analyse the strengths and weaknesses of general practice'; as editor I agree and believe that the topic of the use of health services by people in different occupational categories is a topic of great importance and one where it has been previously suggested is a weakness of general practice.

Their concluding sentence that Occasional papers should not be used 'to persuade ourselves that all is well in the state of general practice' is self-evident. Dr Crombie did not make this claim.

D. J. PEREIRA GRAY
Honorary Editor, College Publications

References

1. Crombie DL. *Social class and health status. Occasional Paper 25.* Exeter: Royal College of General Practitioners, 1984.
2. Gray DJ. Social class and health status inequality or difference. Editorial. *J R Coll Gen Pract* 1984; **34:** 241-245.
3. Black Report. *Inequalities in health.* Townsend P, Davidson N. (Eds). London: Penguin, 1982.

Sir,

I wonder if McPherson, Coulter and McPherson (Letters, September *Journal,* p492) have read the Black Report[1] let alone my paper properly. They start by identifying my main criticism of the Black Report as the calling into question of its argument that substantial social class differences in mortality could be partly attributable to differences in health care provision between social classes. In fact, McKeown,[2] Black and myself all agree that there is no evidence to suggest anything of the sort. The only exceptions here with which it happens I agree are those that relate to perinatal and neonatal mortality. Apart from these exceptions, Black goes even further and has to admit that even differences between social classes in morbidity rates have probably no attribution to differences in health care provision.

They are perturbed by imprecise definitions. Which definitions? They identify specifically definitions of a doctor initiated consultation. It is clearly stated in the text, and supplemented in the appendix, that doctor-initiated or doctor-controlled consultations are simply by definition all consultations other than the first for any given episode of illness. The first consultation for any episode of illness is defined as patient-initiated. In passing, the patient-initiated consultations equate with patient consulting rates and are a reasonable proxy for the rate at which patients initiate access to services. This, albeit arbitrary, classification gets round any element of bias in coding.

Indeed, much if not all of the excess of consultations given to social class 5 is due to differences in morbidity. Of course there are differing illness rates between social classes. For example, scabies, followed closely by chronic bronchitis, has the steepest gradient of all from social class 1 to social class 5 and dermatophytosis in reverse.

The question usually asked is whether the increase matches the excess morbidity, However, since there are no absolute measures of morbidity and therefore of need, it is not surprising that all attempts to answer the problem in this way have been easily discounted by those who have a vested interest in disproving any attempt to show absence of differences between social classes. Indeed I simply demonstrated that irrespective of the absolute baseline of morbidity (a) social classes 4 and 5 elected to bring only marginally more episodes of new illness to doctors and (b) doctors respond by providing markedly more services to social class 5 than social class 1. This was done by the expedient of grouping illnesses into: (a) all illnesses with onset during the survey year which the patients elected to bring (episode types 2 and 4) and, (b) chronic illnesses which began before the onset of the survey year (episode type 1).

For the new, and on the whole less serious, illnesses there were no or only small variations in episode rates and total consultation rates by social class. For chronic illnesses there were steep gradients from social class 1 to 5.

In passing it should be pointed out that access to primary care depends on patient initiative, and *disparities* in *opportunities* for access are the only basis for 'inequality'.

Therefore, whatever the ratio of trivial to serious conditions in the problems that social class 5 elected to bring, doctors responded by redressing the bias towards the patients' under-reporting or under-use of services, which is implied in these findings. In these circumstances, I think the onus is on critics to show evidence that differences in illness rates, which of course exist, could alter these conclusions. Apart from this basic conclusion, I believe I also demonstrated that there was no evidence that there were any overall differences of services given in relation to real need within this primary compensatory mechanism.

The data that I used for this purpose is not new. The figures that I produced in Tables 14 and 15 can be repeated for any of the 18 Chapters from data published already.[3] Patient consulting rates per 1000 population at risk by age, marital status and social class, and standardized patients consulting ratios, are available in the same publication for specific disease categories in Tables 7, 8 and 9.

I would also comment on the recently published paper by Blaxter[4] which McPherson and colleagues have rightly praised. Unfortunately this appeared after the publication of my paper and therefore I could not refer to it. She used a more sophisticated grouping of individual morbidities but it so happens that her main findings are very

similar to those that McPherson and colleagues find unproven in my paper, namely 'The greatest difference in consultation rates comparing patients in social classes 4 and 5 with those in classes 1 and 2 was for life threatening, urgent, chronic or incapacitating conditions thus matching the presumed difference in need'.

The authors then move on to doctor variability and make the statement that 'The second main part of Crombie's argument rests on the finding that variations in aspects of practice style between general practitioners is greater than between social class, age or sex'. I did not say this. I simply said that there are greater variations in utilization rates between the doctors than between any groupings of the patients by age, sex or social class or by any environmental characteristic of the practices. *These differences are facts.* In the wordy fog which follows this misstatement of my findings, they seem to be suggesting that these huge differences would not be unexpected because doctors take the major role in determining these rates. This is roughly what I concluded myself and only included the world 'style' to summarize these differences. I used this term partly because of the great consistency over time of any doctor's unique pattern of activities. They then look for reasons for the relatively small differences in the rates between social classes. They conclude that this could be due to the fact that social class 'may be too crude a measure to pick up whatever differences in utilization may in part be responsible for their mortality differences'. With this I of course agree but this last statement simply destroys any basis for the case for social class inequalities having any relevance at all. Rose[5] has recently shown in his study of standardized mortality ratios in civil servants that there are much larger differentials between four main groupings of civil servants than between the conventional social classes. This seems to me a good example of the way in which we should be approaching the study of inter-group differences among our patients.

The authors do not like my interpretation of the data about inter-doctor variability. McPherson and colleagues cannot have this both ways. The whole case for inequality turns on an acceptance that consultations (and referral rates) can be taken at face value as some sort of measure of relative quality of care when used with reference to differences in these rates by social class. If this is thought to be so (and of course I do not take it to be so), then surely they can be used at face value in the same way to measure relative differences between the quality of care given by different doctors. If they believe that these measures, whether consultation rates or referral rates, reflect some aspect of quality of care, then it is the huge differences between general practitioners that they must concern themselves with.

McPherson and colleagues then take exception to my suggestion that this huge range of variability may reflect (albeit inefficient) different ways to effective care. Relative effectiveness and differences in quality of care are rapidly replacing social class 'inequalities' as the prime academic jousting ground. All that we can say for certain about 'effectiveness' and 'quality' can be summed up in the statement that 'measurable differences of effectiveness and of quality of *outcome* of care are scarce to non-existent'. The common sense explanation of this fact would be that differences in effectiveness and quality, therefore, are so small that at present they defy measurement. There is certainly no evidence that any of these huge differences in styles of practising have any relationship with measurable differences in outcome. However, we are of course examining this possibility and would be grateful for any suggestions or leads to further studies.

The authors do not like the concept that varying degrees of ability to cope with life distinguishes the social classes. We certainly have differential uptake of necessary services by our patients. Where does the resistance to the concept of 'non-coping' by patients as a possibly important element come from? All general practitioners, whether obsessed by social class or not must acknowledge variations in an ability to cope and adjust their response to the patient accordingly.

Facing the element of 'non-coping' squarely when it exists, demands at least an attempt to be a true teacher. This is easiest when lack of knowledge is the defect, still feasible when new skills are needed but becomes difficult to impossible when basic attitudes and values are involved.

The 'non-coping' patient whose problems relate to inappropriate or unsatisfactory attitudes and values demands a most complex demanding and time-consuming response from the doctor.

It is much more emotionally comfortable, cosy and 'normal' if one can side completely with the patient and put the 'blame' for his problem somewhere else on some impersonal external whipping boy or scape-goat.

May this not be the source and origin of their need to equate the difficulties of 'non-coping' patients with defects in society rather than on the patients' unsatisfactory attitudes and values. It is because coping, social class and its associated personal characteristics do go together that the 'defects' in society take the form they do and constitute such an internally coherent package. If so, then it is also why doctors and others with this problem might rationalize their own set of basic attitudes, values and beliefs to match.

I leave others to decide whether McPherson and colleagues' woolly criticisms really disprove my basic conclusions and also whether my comments on differential ability to cope are any more naïve than much of the commentary by social analysts with obsessions about social class.

They then criticize my use of data from the paper by Collins and Klein.[6] Unlike the writers of this letter, I do not accept Scott-Samuel's[7] so-called serious methodological critique. The most important new element in the re-analysis of General Household Survey data carried out by Collins and Klein was ensuring that there was cor-

respondence between the two populations constituting the enumerator and denominator of the use/needs ratio. The absence of this correspondence invaliated the previous analyses by Brotherston,[8] Forster[9] and le Grand[10] and this was agreed by Scott-Samuel in his paper.

Scott-Samuel identified three defects in Collins and Klein's presentation. The first criticism concerned the use of patient consulting data without any measurement of the frequency of consultation. This seems to me a very spurious argument indeed for the 'need' element of the use/need ratio was also based on patients' reporting illness and not on the number of problems or illnessses reported. It seems to be therefore perfectly valid to use this measure of need when 'use' is also based on a patient consulting rate and not on total consultation rates, particularly when the main aim of this analysis was to throw light on *access* to primary care and not total usage. Data now presented by myself and by Blaxter,[4] using both patient consulting data and total consultations and contacts, makes it clear that both measures produce complementary conclusions. It so happens that standardized patient consulting ratios in the Second National Morbidity Survey, which reflect access rather than total usage, also show a small excess for social classes 4 and 5 compared with social classes 1 and 2. This data matches that of Collins and Klein for patients consulting their general practitioner. However, episodes of illness and consultations per patient at risk (and also per patient consulting) in the Second National Morbidity Survey show stronger gradients favouring social classes 4 and 5. This was of course the basis for my own conclusions. Blaxter also found a similar gradient for consultation rates using National Morbidity Survey data. In the sense that patient consulting rates hide a gradient *favouring* social classes 4 and 5, Scott-Samuel was partly correct. Analysis of the 1977 General Household Survey data[11] using this approach also showed a gradient favouring social classes 4 and 5.

Scott-Samuel and McPherson and colleagues must know that there are no usable or generally accepted absolute measurements of health status, so relative or indirect measures of attributes which are proxies for absolute measures must suffice. This makes Scott-Samuel's striving for precision rather artificial.

The second criticism concerned the classification of 'need' by the GHS question used by Collins and Klein to define the acutely sick. This was, 'during the two weeks ending last Sunday, did you have to cut down on any of the things you usually do because of this illness, disability or some other illness or injury'. Scott-Samuel commented here 'clearly a positive answer to this question cannot necessarily imply the need for a visit to the general practitioner and cannot therefore be related with any validity to the "use" data'. On the contrary, it seems to me that the extreme inclusiveness of this definition is itself a virtue. It must include almost any other definition of need except perhaps the need which is unperceived in the form of pre-symptomatic morbidity. Any bias must be from non-reporting and therefore of rates which are too low. In the event the relative rates for need as numbers of patients reporting exactly matched the relative measure of use by numbers of patients attending their general practitioner.

If different morbidities tend to cluster, then the use of a patient reporting rate could hide this fact. However this is crudely counterbalanced by the use of patient consulting rates instead of total consultation rates in any comparative statistic.

As I mentioned earlier, both sets of measurements were used in the NMS publications and in my paper.

The third criticism is one which is also made about my paper, namely that differentials have been shown to exist with respect to the length and quality of the consultation and that there may be systematic social class differentials in the 'opportunity cost' that is the benefit foregone in making a visit to the doctor. Whether or not there are systematic social class differentials in this opportunity cost, social class 5 still managed to consult their doctor more frequently than social class 1.

It should now be clear that there is no substance in their suggestion that Scott-Samuel's critique stands as a valid criticism of my analysis and in particular that 'the use of general practitioner services is taken as serving equal needs'. McPherson and colleagues suggest that there is no reason to suppose that such an assumption is remotely justified. On the contrary apart from the data of Cartwright and O'Brien[12] and Richardson[13] there is no evidence to suppose that they do *not* do so. General practitioners are beholden to Ann Cartwright not only for the particular contribution which the authors mention but for much else that she has contributed to our understanding of primary care problems . . . Indeed general practice has taken to heart this possible major criticism by including in both undergraduate and postgraduate training the teaching of communication skills in the context of increased awareness of possible problems in this difficult area. I am not sure what else McPherson and colleagues think we should do about this problem. I leave others to judge how important these final elements may be in this whole argument.

That part of Le Grand's analysis which deals with primary care has been totally invalidated by the Collins and Klein reanalysis of data even if we accepted Scott-Samuel's criticisms of the ways in which Collins and Klein went on to use that reanalysed data.

Scott-Samuel's explicit conclusions, and implicitly also those of the writers of these two letters are remarkable examples of the perverted logic that is all too common in this context today. The whole hypothesis of social inequalities in health care was first set-up around dubious data which bit by bit has been discredited, in particular the original form of the use/need ratio. However, as it has been discredited, the defenders of this hypothesis stick to the battle cry:[7] 'Such conclusions (such as those of Collins and Klein) can in no sense be said with any certainty to alter the existing state of knowledge on social class

equality in access to primary care. What knowledge can we be talking about when we are back to square one? The only certain 'knowledge' at this point in time can be summarized as 'There is no evidence now and there never has been of social inequality in *opportunities* for *access* to primary care' other than that previously mentioned.

I was accused of being extremely selective in my review of literature. On the contrary, it seems to me that this largely spurious debate about social class inequalities has only been kept going by the selective use of convenient statistics and the ignoring of the inconvenient facts.

D. L. CROMBIE
Director, RCGP Birmingham Research Unit
Lordswood House
54 Lordswood Road
Harborne
Birmingham B17 9DB

References

1. Black Report. *Inequalities in health.* Townsend P, Davidson N (Eds). Harmondworth: Penguin, 1982.
2. McKeown I. *The role of medicine. Dream, mirage or nemesis?* Rock Carling Lecture. London: Nuffield Provincial Hospital Trust, 1976.
3. RCGP, OPCS, DHSS. *Morbidity statistics from general practice 1970–71 Socio-economic analyses. Studies on medical and population subjects no. 46.* London: HMSO, 1982.
4. Blaxter M. Equity and consultation rates in general practice. *Br Med J* 1984; **288:** 1963-67.
5. Marmot MG, Shipley MJ, Rose G. Inequalities in death–specific explanations of a general pattern. *Lancet* 1984; **1:** 1003-1006.
6. Collins E, Klein R. Equity and the National Health Service. Self reported morbidity access and primary care. *Br Med J* 1980; **281:** 1111-1115.
7. Scott-Samuel A. Social class inequality in access to primary care: a critique of recent research. *Br Med J* 1981; **283:** 510-511.
8. Brotherston J. Inequality. Is it inevitable? In: *Equalities and inequalities in health.* Carter CO, Peel J (Eds). London: Academic Press, 1975.
9. Forster DP. Social class differences in sickness and general practitioner consultations. 1976; **8:** 29-32.
10. le Grand J. The distribution of public expenditure: the case of health care economics. 1978; **45:** 125-142.
11. Office of Population Censuses and Surveys. *General household survey 1977.* London: HMSO, 1979.
12. Cartwright A, O'Brien M. Social class variations in health care and in the nature of general practitioner consultations. In: *The sociology of the National Health Service.* Stacey M (Ed). Sociological Review Monograph no. 22. Keele University Press, 1976.
13. Buchan J, Richardson I. *Time study of consultations in general practice.* Scottish Home and Health Department, 1973.

New RCGP classification

Sir,

The British, especially British general practitioners, often overlook the extent of their influence in the world of ideas. I recall a sign displayed in an important North American teaching institution. 'We have a choice. We can either do this work, or try to keep up with how far the British are ahead of us.'

The presentation of a new British *Classification of diseases, problems and procedures* in general practice[1,2] is an event of supreme importance and interest to the primary care taxonomers of the world. I have no doubt that the concepts embodied in the work will prove to be very influential on the international scene.

What is mystifying to the friends and admirers of the Royal College is why such worthy work should be conducted in such Byzantine secrecy. The Classification Committee of WONCA was founded by your Robin Pinsent; Donald Crombie, the chairman of the Committee reponsible for the 1984 RCGP Classification was for many years a member of the WONCA Committee, and Clifford Kay has corresponded often with them. Foreign taxonomers were never told about the developing ideas of Great Britain. Apparently even the RCGP representative to the International Committee was not told about the classification work taking place in Manchester!

Openess and internationality are two important features of science; I wish the 'caring scientists' of the RCGP would try to bear this in mind as they push forward with their great works.

ROBERT WESTBURY
Department of Family Medicine
Ohio State University
Columbus
Ohio
USA

References

1. Gray DP. To classify is to clarify. *J R Coll Gen Pract* 1984; **34:** 306-308.
2. RCGP Research Division Executive. New classification of diseases and problems. *J R Coll Gen Pract* 1984; **34:** 125-127.

Sir,

The advent of a new RCGP classification of morbidity (which does not yet appear to have a name)[1,2] rouses mixed feelings in many who are interested in general practice morbidity recording at an international level. I would not presume to comment on the need or otherwise for a special RCGP classification, but a number of related issues need discussion.

Dr Kay's statement 'there is never any difficulty in accommodating long lists in shorter lists'[1] is surely an excellent argument for using ICD itself in general practice, since it is apparently essential to be entirely compatible with it. Why have a short list of any sort if 'longer lists and more specific terms are much easier to use than short lists'? We have wrestled with this problem long enough in the International Classification Committee of WONCA to know that this statement does not stand up to scrutiny. The question is, easier for what?

Classifications need to be used for both the input and output phases of data management. Unfortunately a classification which allows automatic coding so that there