those rural migrants who moved to the cities constitute a group with a high potential to prevent disability adjusted life years (DALY) lost. That is particularly important for a disease whose incidence rarely occurs after the fifth decade, such as CHAD. Provided the success of vector control remains, this potential tends to grow in the cities, while decreasing in the areas of former active transmission.

Let us take now the natural history of CHAD. From the Figures provided by Lima e Costa et al., one can confirm the expected reduction of the burden for CHAD in Brazil in the past decades, but also notice how the shape of the curves of mortality and hospitalization has remained unchanged. Those Figures strongly suggest that the natural history of the disease has been basically the same. Virtually no incidence of symptomatic CHAD occurs after the fifth decade of life. Indeed, most of the sudden, unexpected death in CHAD occurs before, whereas most deaths after the 50s occur in previously symptomatic individuals. Those lower mortality rates before the 60s are much more important in terms of DALY lost. The same argument applies for hospitalizations. For a chronic cardiomyopathy that is highly lethal once it becomes symptomatic, the rise in the number of deaths and hospitalizations after the fifth decade is not surprising. I would therefore interpret the excess of mortality at older ages as worsening of previously symptomatic disease. Likewise, the excess of hospitalizations after the 50s would be mostly due to re-admission of previously symptomatic patients. This information leads us to see CHAD as an entity with an unchanged, particularly high impact on premature death or disability.

I believe patient-based research on CHAD should emerge. Studies like Lima e Costa’s make us aware that a substantial burden of disease remains in spite of the interruption of the transmission. On the basis of a continued vector control programme, I see a large common ground for answering research questions highly relevant for both infected individuals and their health care givers. Most of them would apply for both young and old patients living in either rural or urban settings. However, focusing on people over 60 living in towns of former active transmission would either miss most of the high-risk asymptomatic population, or tackle a burden of a disease that has already started. For a disease whose risk factors, or interventions with preventive capabilities have not been identified yet, including the infected, urban population in their productive years to be included in the control programmes would attain a more comprehensive impact.

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Jerry Morris and Health Services Research in the USA

From KERR L WHITE

Sir—The International Journal of Epidemiology published a superb set of papers to celebrate Jerry Morris’s enduring contributions to the ever-broadening applications of epidemiological concepts. To the authors’ thoughtful comments may be added his role in initiating and shaping Health Services Research in the US and beyond. George Davey Smith has already mentioned the 1952 University of North Carolina’s Chapel Hill conference on research requirements for health and medical care. That conference, and especially Jerry’s keynote address, helped to establish a major research agenda at what was the first new medical school in the US after World War II. I was attracted from McGill University to join the founding faculty of the Department of Internal Medicine in 1953, as were Frank Williams, Bob Huntley, and Dan Martin. John Cassell and Sydney Kark from South Africa and Bernie Greenberg were similarly attracted to the also new School of Public Health. We were friends, taught in each other’s classes in the two schools, and initiated studies under the rubric of Medical Care Research. Jerry’s landmark 1955 article in the British Medical Journal on the Uses of Epidemiology, the prelude to his classic volume, intrigued me further. The result was a sabbatical year (1959–1960) in London spent jointly with Jerry at the London Hospital where he was then based and at the London School of Hygiene and Tropical Medicine. Whatever I was able to pass on later to students and colleagues was learned from Jerry Morris.
Here are several examples.

Jerry had introduced me to the UK's Hospital In-Patient Enquiry (HIPE) and the Register General's fascinating but little analysed reports. Indeed this enterprise embodied an idea that Florence Nightingale had urged upon the medical fraternity a century earlier. Jerry also emphasized the still startling message in Table 3 of Davey Smith's article summarizing the 'Glover Phenomenon'. On return to Chapel Hill, I had hoped to use Glover's model to examine hospital and clinical performance both locally and eventually more extensively. Accordingly, I attempted to introduce into the North Carolina Memorial Hospital a discharge abstracting system, the Professional Activities System (PAS), which Vergil Slee was developing with support from the Kellogg Foundation. My biomedical colleagues at the University of North Carolina informed me that such ideas were unwelcome and that I obviously did not know I was working in a university hospital whose activities required no monitoring but rather set the gold standard for all hospitals in its region.

Two years later in 1962, the University of Vermont invited me to head what we called the Department of Epidemiology and Community Medicine (the first US medical school to use the term 'epidemiology' as a department label). One major attraction to Vermont was its relatively small population (then about 400,000), compact geography, and the prospect of installing a hospital discharge abstract system (PAS) in all that state's hospitals. The goal, as in North Carolina, was to examine the 'Glover' and related phenomena by comparing population-based rates for hospital admissions (or discharges), procedures, and outcomes. With the help of John Last, whom I had recruited from Australia to our department in Vermont, we did just that. We went about installing PAS in all Vermont hospitals for what would be the first population-based hospital discharge abstract system in the US. It was not easy! There were endless meetings with hospital administrators, their boards and lawyers, as well as with health officers and local politicians; most required intense persuasion. Even the Vermont Medical Society paraded me with the charge of mounting a 'communist plot', and so forth.

Before we could generate adequate data or attempt analyses, I was offered the job of founding a new department at Johns Hopkins (initially the Department of Medical Care and Hospitals, later Health Care Organization, and now the Department of Health Policy and Management). Somewhat reluctantly, I left Vermont for Baltimore with the unwelcoming thought that a message from Hopkins might attract more attention than the same one from a provincial medical school. In addition, I was to have expansive real estate within the hospital and a large budget. Osler once remarked to his three fellow 'horsemen': 'It's lucky we have jobs in this place because we would never get in as students!' It is also said of Hopkins that it would not matter if the faculty were all idiots because the students are so good. One of our first such students in 1965, Jack Wennberg, was interested in what we still called Medical Care Research. I introduced him to Jerry's work, the 'Glover Phenomenon', and the prospects of small area comparisons, and told him about the wealth of unmined hospital discharge abstract data being generated by Vermont hospitals. We put him in touch with authorities in Vermont and he took it from there. His Center for Evaluative Clinical Science at Dartmouth is now a global pioneer and leader in pursuing Glover's and Jerry's visions with, of course, creative and greatly enhanced data and methods. We now have national atlases depicting the alarming variations in medical procedures and outcomes. Jerry should be delighted!

Convinced in 1968 that Florence Nightingale and the UK's more recent HIPE reports were the way to go, we organized from Hopkins an international conference on Hospital Discharge Abstract Systems, a number of which were evolving by this time, but none of which contained the same core data elements and none were population-based. From this conclave, Nightingale's notion of Minimum Uniform Data Sets re-emerged. Their adoption by the US National Center for Health Statistics (NCHS) followed, albeit gradually; 17 years to be exact! This set was followed by two additional conferences and promulgation of minimum data sets for ambulatory care and for 'long-term' care. Jerry Morris had introduced me to this relatively simple idea for generating the essential population-based data required to assess the use of a population's health services.

Although we had been barred from monitoring clinical performance for inpatients at the University of North Carolina Hospital, Bob Huntley, Frank Williams, and I pursued the same goals in a new educational venue we called a 'General Clinic', for which we were responsible. Among the several investigations we mounted was a systematic review of all our medical charts.

We thought we ran an unusually 'tight ship' but found to our dismay that we were experiencing unacceptably large 'error' rates. There were unexamined abnormal laboratory results and X-rays, broken return appointments, and failures to notify referring physicians of the results of their patients' studies. We published what seems to have been the first assessment of the 'quality' of medical care in an ambulatory setting. But that was not the end! When I arrived at Hopkins in 1965 I again tried to introduce a system of hospital discharge abstracts and met the same negative reaction; the gold standard, I was told, was of even higher quality than that in North Carolina. From the Johns Hopkins Hospital, we switched to the outpatient clinics of the Baltimore City Hospital, a teaching affiliate of the Hopkins Medical School. Here I persuaded Julie Krevans, the Chief of Medicine, to allow Bob Brook, recently the Chief Resident on the Osler Medical Service in the Johns Hopkins Hospital and now a graduate student in our department, to replicate the study we had done in the General Clinic at the University of North Carolina. Imbued with Jerry's thinking, especially in relation to outcome comparisons of teaching and non-teaching hospitals (Table 2 in Davey Smith's paper), Bob worked with John Williamson in our department to complete an investigation with results that were even worse than our original ones at Chapel Hill. His paper was published in the New England Journal of Medicine. Eventually the Johns Hopkins Hospital allowed us to replicate the study and again, the results were unacceptable. Bob went on from there to his current role as Vice-president of the Rand Corporation and Professor of Medicine at the University of California, Los Angeles. His work on outcomes and the 'appropriateness' of medical procedures has set new standards and constituted tangible testimony of Jerry's ubiquitous influence on Health Services Research in the US.

At the University of North Carolina, as I indicated above, our group included colleagues in both the new School of Medicine and the new School of Public Health (housed in the basement of the medical school building). At the time I questioned the wisdom of separating these two academic entities but accepted
the emerging realities since we had other fish to fry. When I arrived at Hopkins our new department was based organizationally in the School of Hygiene and Public Health but housed in the Johns Hopkins Hospital. Subsequently we were required to move to fancier quarters in a new addition to the old School of the Public Health building. Here I realized that two vastly different cultures were separated by Wolfe Street. Again, I was reminded that Jerry Morris argued that there are three venues for studying health and disease, the bedside or clinic, the laboratory, and the population. Why should the third dimension be separated academically from the first two? Given the 1965 advent of new publicly funded Medicare and Medicaid legislation in the US, it seemed to me, again reflecting Jerry’s vision, that the country’s health departments would eventually have responsibility for spending and/or monitoring the disbursement of these funds and for assessing their impact on the health of the populations served. This would require a new kind of physician and a new approach to managing health care arrangements.

It is true that US schools of public health had something called Departments of Public Health Administration but I could never find out what they administered, let alone what difference it made, and to whom. How could we stimulate discussion of these matters? Again, I turned to Jerry and invited him to be visiting professor at Hopkins. Here he gave his seminal talk on Tomorrow’s Community Physician. It was a great local success and drew a much larger crowd than Archie Cochrane’s talk a few years later. We debated whether he would submit it to the New England Journal of Medicine as Bradford Hill and Will Pickles years later. We debated whether he would submit it to the New England Journal of Medicine as Bradford Hill and Will Pickles.

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Author’s response

From NICK DAY

Professor Willett, in his editorial, dismisses the relevance of our paper, in which we compare the performance of a food frequency questionnaire (FFQ) with a 7-day diet diary in estimating nitrogen and potassium intake, using nitrogen and potassium measurements from six 24-h urine collections as independent biomarkers of intake. He claims we have been unfair to our FFQ. His main criticism is that we have not adjusted for energy intake. The principal reason for focussing on energy-adjusted intakes is that both in the epidemiological and the experimental setting, interest is primarily in comparing isocaloric diets. We would agree. Energy adjustment is highly desirable. The question is whether it can be done. Although Professor Willett has argued repeatedly for isocaloric comparisons, he has, never, to our knowledge, produced any evidence on the accuracy with which the FFQ can estimate energy intake, apart from one small (n = 20) study with doubly-labelled water in which it was concluded that ‘none of the methods (including the Willett FFQ) gave accurate estimates of the usual energy requirements of individual subjects’.

As part of the programme of dietary assessment validation associated with our EPIC-Norfolk cohort, we have estimated energy expenditure, which in the absence of changes in body weight, we take to be equivalent to energy intake. Using 4-day, individually calibrated, heart rate monitoring (HRM), it has been shown that this method agrees well with estimates of energy expenditure using doubly-labelled water. In the figure, we show the relationship between the FFQ estimate of energy intake with the HRM estimate of energy expenditure in 100 individuals. For comparison, we give the 7-day diary estimates of energy intake and also weight. As can be seen, the FFQ estimate of energy intake is almost independent of the HRM values. The diet diary does better, and weight is the most strongly associated with energy expenditure. Results from a larger study (n = 448) in the US using doubly-labelled water, the OPEN study, corroborate our finding, that a FFQ provides a very poor measure of energy intake (Kipnis, personal communication). Thus Willett’s principal reason for energy adjustment is void. Using FFQ, energy expenditure is associated with specific foods and dietary patterns.