[4] Meiselman MS, Cello JP, Margaretten W. Cytomegalovirus colitis. Report of the clinical, endoscopic and pathologic findings in two patients with the acquired immune deficiency syndrome. Gastroenterology 1985; 88: 171-5.
[5] Collaborative DHPG treatment study group. Treatment of serious cytomegalovirus infections with 9-(1,3-dihydroxy-2-propoxymethyl)-guanine in patients with AIDS and other immunodeficiencies. N Engl J Med 1986; 314: 801-5.
[6] Masur H, Lane HC, Palestine A, et al. Effect of 9-(1,3-dihydroxy-2-propoxymethyl)guanine on serious cytomegalovirus disease in eight immunosuppressed homosexual men. Ann Intern Med 1986; 104: 41-4.

S.J.H.VAN DEVENTER

Huid- en geslachtsziekten

Malathion als scabicide middel

Ondanks het feit dat er verschillende werkzame middelen zijn tegen scabiës, komt deze aandoening nog steeds veel voor. De meest gebruikte middelen zijn hexachloorcyclohexaan, benzylbenzoaat en crotamiton. Gelijktijdige behandeling van gezinsleden en goede instructies over de wijze van applicatie en over de hygiënische maatregelen zijn van groot belang voor het welslagen van de therapie, vooral bij epidemieën. Een ander middel waarmee in het verleden goede resultaten zijn geboekt,[1] maar dat desondanks niet of nauwelijks bij scabiës wordt toegepast, is malathion. Burgess et al. vergeleken de werkzaamheid van malathion 0,5% met benzylbenzoaat 25% na eenmalige applicatie bij 120 kinderen en twee volwassenen met scabiës, die afkomstig waren van een weeshuis en twee scholen in Dhaka, Bangladesh.[2]

Bij hun conclusie dat malathion een sterker scabicide middel is dan benzylbenzoaat kunnen enkele kritische kanttekeningen worden geplaatst. Het is jammer dat de onderzoekers malathion niet hebben vergeleken met hexachloorcyclohexaan of crotamiton waarvan bekend is dat een eenmalige behandeling meestal afdoende is. Andere bezwaren tegen dit onderzoek zijn dat de groepen niet homogeen zijn samengesteld en niet gelijk van grootte en dat ook het aantal patiënten dat na 1 en 2 weken niet voor controle kwam voor beide groepen verschillend is. Dit neemt echter niet weg dat malathion aandacht verdient als scabicide middel.

LITERATUUR
[1] Hanna NF, Clay JC, Harris JRW. Sarcoptes scabiei infestation treated with malathion liquid. Br J Vener Dis 1978; 54: 354.
[2] Burgess I, Robinson RJF, Robinson J, Maunder JW. Aqueous malathion 0,5% as a scabicide: clinical trial. Br Med J 1986; 292: 1172.

J.J.E. VAN EVERDINGEN

Heelkunde

Bij messteken in de buik is röntgenonderzoek overbodig

Klinisch onderzoek is veruit het belangrijkste voor de indicatie tot laparotomie bij messteken in de buik; aanvullende onderzoeken als laparoscopie, contrastonderzoek van het steekkanaal en peritoneale spoeling zijn omstreden en dragen weinig bij. Een aanvullend onderzoek, dat nauwelijks als zodanig door de meeste medewerkers van eerste-hulpafdelingen zal worden aangemerkt door de hoge frequentie waarmee het als traditie wordt uitgevoerd, is het röntgenonderzoek van de buik. De Amerikaanse chirurgen Kester et al. verrichtten een retrospectief onderzoek naar nut en kosten van de buikoverzichtsfoto's, die routinematig waren vervaardigd bij 94 patiënten die tussen 1 september 1981 en 1 maart 1984 een laparotomie ondergingen wegens steekwonden in de buik.[1] Bij 86 van hen waren er röntgenologisch geen afwijkingen. Ongeveer de helft van hen had intra-abdominale verwondingen die chirurgisch moesten worden verholpen en van hen had weer de helft darmperforaties, die kennelijk niet tot een zichtbaar pneumoperitoneum hadden geleid. Acht van de 94 patiënten hadden afwijkingen: bij 5 was vrije lucht onder het diafragma zichtbaar; bij 2 een verdichting, passend bij een bloeding en één had lokale ileus. Zeven van deze acht werden geopereerd op grond van peritoneale prikkelingsverschijnselen. De enige patiënt zonder symptomen had wèl vrije lucht onder het diafragma, doch bij laparotomie geen intra-abdominale afwijkingen. Van het totaal van 26 patiënten met aangesneden darmen hadden er slechts vier een pneumoperitoneum.

Uiteraard achten de auteurs op grond van dit onderzoek de buikoverzichtsfoto bij messteken volslagen nutteloos. Dat was het allang, wanneer een chirurgische kliniek als regel exploratieve laparotomieën uitvoert bij slachtoffers met steekwonden in de buik. Maar ook bij een meer selectieve benadering van deze verwonding geven normale beelden bij de 'blanco buik' géén informatie (de helft is fout-negatief); is het röntgenbeeld daarentegen afwijkend, dan wordt het therapeutische beleid er niet door beïnvloed. De kosten van dit overbodige onderzoek waren bovendien niet onaanzienlijk: per afwijkende foto 1400 dollar!

De conclusie van de schrijvers dat de buikoverzichtsfoto geen kosteneffectieve methode is, kan gevoeglijk als een 'understatement' worden beschouwd; het onderzoek kan zonder meer achterwege blijven.

LITERATUUR
[1] Kester DE, Andrassy RJ, Aust JB. The value and cost effectiveness of abdominal roentgenograms in the evaluation of stab wounds to the abdomen. Surg Gynecol Obstet 1986; 162: 337-9.

J.BENDER

Ingezonden

(Buiten verantwoordelijkheid van de redactie; deze behoudt zich het recht voor de stukken te bekorten; stukken die langer zijn dan 1 kolom druks komen niet voor plaatsing in aanmerking)

De filosofische basis van de geneeskunde

As scholars we recognize the debt we owe our colleagues who take our work seriously enough to offer carefully developed criticism. We are therefore delighted that Dutch physicians and philosophers would offer their criticism of our work in the philosophy of medicine (1985; 2512-7). Their comments will help us to clarify our position, to consider new questions and to tighten the lines of our arguments. Even more, we are encouraged about the still-budding discipline itself to see that all of our critics agree that there is some basis for a philosophy of medicine that deserves exploration. Nevertheless, some points require comment from us. We wish to respond first by making some general points, and second by addressing specific topics.

General comments. Our critics appreciate that we have undertaken a serious, important, but very difficult task. We were under no illusion, however, as Dr. Verbrugh suggests we might have been, that our book might heal the rift between philosophy and medicine on its own, or that we have exhaustively covered all possible topics in the philosophy of medicine, or that our work would please those scholars who do not share the same philosophical suppositions and methodology. Further we thought we had made clear that ours was not the first systematic American effort to address a philosophy of medicine (we think that honour belongs to F.Scott Buchanan).[1] Note, too, that Dr.Endtz thought we presumed uniqueness by placing the clinical event at the center of medicine. Our references during the discussion leading to that placement should disabuse anyone from that notion.

We can't refute the objections to our philosophical method at this time, nor try to resolve difference in our presuppositions between ourselves and our critics. They obviously approach philosophy differently than we do. Nonetheless much of the criticism stems from our clear intention to find the roots of a philosophy of medicine in the actual phenomena of medicine itself, and from those roots, to explore health and illness, and indeed, ethical axioms and consequences of these axioms. Prof.Broekman seems sympathetic with this enterprise, but quite concerned that axioms leave no room for discussion. Here a problem with language occurs. We regard axioms not as ultimate principles to which one must always adhere, but rather, as context-derived guidelines for action. We argue that such axioms are dependent upon the healing aims of medicine and are required to reach that aim. They do not rule out discussion between doctor and patient at all. Rather dialogue about the healing aim, specifically what counts as healing for the patient, is necessary to accomplish the moral task of medicine.

In this effort, we do not deny the importance of other dimensions of medicine, especially such dimensions as prevention, public health, the issue of resource allocation, and the role of social values in the practice of medicine. Indeed, each of us, in several places, has discussed both the ethics and the philosophy of many of these other dimensions of medicine. As we said in the beginning of the book, our intention was to initiate the discussion needed to develop a new philosophy of medicine for our day. In particular, this development would be a 'systematic' study of medicine. That is to say, philosophy of medicine includes its logic, epistemology, ontology, aesthetics, axiology, sociology and anthropology, to name just a few of the components needed.

Specific responses. First it puzzles us that the same book would provoke such widely divergent response, from finding much merit (Prof.Van der Meer and Dr.Endtz) to little merit for Dutch readers (Dr.Verbrugh). The reason seems to lie in expectations more than anything else. We did not set out to write a philosophy of epidemiology or prevention, but rather a philosophy of medical practice. Given our approach toward clinical practice (recall that our title emphasized practice), we took great pains in the book to demonstrate that the clinical event (the doctor-patient relationship) was the distinguishing feature of the discipline of medicine, and not just the result of a personal bias of ours due to training, experience, and proclivities. One could write a philosophy of epidemiology or prevention, but such a philosophy could not be extrapolated to the clinical event. However, a philosophy of the clinical event makes possible other extrapolations, for example the social nature of medicine as a profession.

Second, Prof.Broekman fears that the philosophy of medicine as we have developed it is in danger of being skewed toward intervention and the medical model. We tried to avoid such skewing by emphasizing decisions made for and with the patient (a point missed by Prof.Broekman). We realize that much more needs to be written about the exchange of values in the relationship, and beyond it, in society itself. This concern prompted by criticism that our views lacked a social perspective, has led us to adumbrate these features in detail in our second work to be titled, 'For the good of the patient: the restoration of beneficence in medicine', which will soon appear from Oxford University Press.

Third, a fundamental disagreement about a starting point is present. Should the discipline start with the social reality of medicine or the individual and his diseases? Which has metaphysical priority: the individual or society? We are undeniably social beings, but also individuals. The question is like asking which came first, the chicken or the egg? Hence, we sought a compromise between the study of the individual and the study of medicine as a social reality – a tertium quid, if you will, the relationship of the doctor and patient. From this vantage point, one may address individual, social, professional, economic, scientific, and institutional concerns.

Fourth, Dr.Verbrugh takes us to task for equating theory and philosophy throughout our book. In his words: 'Filosofie is niet hetzelfde als theorie'. He is right, of course. Philosophy is not equivalent to theory. But the obverse is true. Theory in medicine is equivalent to a philosophy. As our historical development in the book demonstrated, thinking in medicine about the craft, disease entities, and all other concerns, always betrays a philosophical basis. This point is very well articulated by Prof.Broekman at the end of his critique. Medicine rests on a philosophical and ethical base prior to any particular philosophical analysis. While Dr.Verbrugh is correct, then, in tracing the divergence between philosophy and medicine since 1840, we should not uncritically accept his implication that no philosophical thinking occurred in the development of the theory of medicine over the past century. We liked his analogy between a chronically ill patient and the often troubled relation of philosophy and medicine – many efforts will be necessary to rejoin the two in dialogue. Further, he is correct to argue that rejoining the two disciplines on the basis of ethical and economic concerns is not sufficient. That is why we set a modest goal of taking a 'first step' toward developing a philosophy of medicine.

Finally, a word about methodology. We perceive some exasperation with our realistic, empirical, analytic, phenomenologic, and eclectic method. Further, Dr.Verbrugh and Dr.Endtz seem puzzled by a personal, 'first person' approach in a scholarly work. We are not the first, nor the last, to note a fundamental rift between Anglo-Saxon and continental approaches in philosophy. Points made in the latter method appear to English-speaking analysts as hopelessly vague and obtuse. Further, the modes of 'detached' analysis in continental philosophy do not properly portray the personal engagement of the thinker thinking that our first-person plural style does portray. Further still, practical conclusions and implications are essential in any philosophy of medicine, which is perforce, a practical discipline. As H.Tristram Engelhardt notes: 'Medicine approaches the world not simply to understand and explain it but to master it'.[2]

A more intensive cross-cultural dialogue is needed to correct cultural bias if a truly authentic philosophy of medicine is to emerge. We are grateful to our Dutch colleagues, then, for contributing to this effort despite their disagreements with us.

REFERENCES

1 Buchanan FS. The doctrine of signatures. London: Kegan, Trench, Trubner and Co., 1938.

2 Engelhardt HT. The social meanings of illness. Second opinion 1986; 1: 26-39.

Pellegrino ED. Health promotion as public policy: the need for moral groundings (Editorial). Prev Med 1981; 10: 3.

Pellegrino ED. Philosophy of medicine: towards a definition. J Med Philos 1986; 11: 1.
Pellegrino ED, Thomasma DC. Toward an axiology for medicine: a response to Kazem Sagegh-Zadeh. Metamedicine 1981; 2: 331-41.
Thomasma D. The philosophy of medicine in Europe: challenges for the future. D. Reidel Publishing Company, 1985: 115-23. (Theoretical medicine; vol 6).
Pellegrino ED. Rationing health care: the ethics of medical gatekeeping. Journal of contemporary health law and policy 1986. Ter perse.

D.C.THOMASMA
E.D.PELLEGRINO

Maywood, Illinois, Washington, D.C., USA, May 1986

We thank Prof.Thomasma and Prof.Pellegrino for their serious attempt to bridge cultural and linguistic rifts. I have asked the writers of the bookreview to comment on the remarks of our American colleagues.

Prof.Broekman is of the opinion that a decision made by a doctor for and with the patient is already characterized by the medical model. He upholds his objection against the equalization of that decision with other decisions pertaining to life events. He objects to the individualistic starting point of the authors in their discussion of the doctor-patient relationship, again an expression of the medical model. It is wrong to make a choice between 'metaphysical priority' of individual and society; this would suggest that an individual might exist prior to a society.

Dr.Endtz and Dr.Verbrugh wrote long replies, from which I select the following remarks.

Dr.Endtz: my cry for definitions remains unanswered. Since medicine is universal and medical practice ideally is tending to be so, the first thing to do is to define the words for use. As for me, I am not against a personal approach and I do not object to changing our axioms, but if we want to be understood, we have to know what we are talking about. I am still puzzled by the easiness shown by Prof.Thomasma and Prof.Pellegrino in their use of the terms 'medicine' and 'medical practice'. I eagerly try to follow their discourse but they do not make my trying easier in this way.

Much remains to be said about the supposed identity between 'medical practice' and the 'healing aims of medicine'. Times have changed since Hippocrates stressed the importance of prognosis and we are better armed to intervene in prognosis; but is not this intervention, and thus the healing aim, only part of medical practice?

One word on psychiatry. I am aware that American psychiatrists today are ready to bridge the rift between psychiatry and the rest of medicine.

Dr.Verbrugh: I still have great misgivings about a venture in 'a philosophy and ethic of the healing professions' presented under the title of 'A philosophical basis of medical practice'. One can describe how people go about setting medical matters, particularly how they deal with situations in which there are conflicts of interests and other 'ethical' issues. This is not a 'philosophical' ethics, however, it is sociology or journalism, not philosophical reasoning. This is my main objection to the book.

The authors see 'the actual phenomena of medicine primarily in the clinical event' (page 4), and although this is historically certainly a valid procedure, I would argue that many, perhaps even most issues which are medically-philosophically controversial and relevant have their roots outside the phenomena of the clinical event. The roots of the reasons why people think and feel and act in the way that they do about such issues as euthanasia, abortion, artificial procreation, psychiatric/psychosomatic treatment, the coping ability in the case of cancer and other serious diseases, etc., are not to be found in the phenomena. It can be fruitful to start the discussion on the basis of the phenomena, but I would also want many more examples taken from practice than Thomasma and Pellegrino provide in their book. Indeed, having a second (or, rather a third or fourth) look at their book while writing this reply to their comment, I am again impressed by the paucity of actual medical cases and phenomena. It is from such case-studies that the reader is incited to discover for himself the actual phenomena of philosophy.

This brings me to another point. I 'seem puzzled by a personal, "first person" approach in a scholarly work'. On the contrary, I dearly miss the 'first person' where it would be appropriate, that is to say, in the narration of personal experiences and in the explicit taking of sides in issues. What I consider to be a defect in the book, is the lack of equilibrium between (subjective) personal experience and engagement on the part of the authors, and the absence of any objective philosophy, in the sense that the great lines of philosophical thought as it has developed in the last 25 centuries are not summarized with reference to medicine today.

I like to close this reply by quoting the last section of Dr.Endtz's letter: 'Finally I want to thank Prof.Thomasma and Prof.Pellegrino for having formulated their ideas first in their book, now in their reply. They have been of great help for me in clarifying my own ideas and, I am sure those of many others. The last word will never be said but I hope that the dialogue will continue'.

J.VAN DER MEER
on behalf of
J.M.BROEKMAN,
L.J.ENDTZ and
H.S.VERBRUGH

Amsterdam, July 1986

Zuur en zuur is twee

Het is van belang er steeds weer op te wijzen dat thiaminedeficiëntie vaak miskend wordt. De ziektegeschiedenis die in de klinische les van Strack van Schijndel en Bronsveld (1986; 993-4) gebruikt wordt om dit te illustreren, lijkt echter geen gelukkige keuze te zijn. De schrijvers veronderstellen dat er aanvankelijk een keto-acidose bestond, die na toediening van glucose overging in een lactaatacidose. Het is echter onzeker of de lactaatacidose niet reeds bij eerste opneming bestond, aangezien in deze fase het lactaatgehalte niet bepaald werd. Het verlaagde hartminuutvolume van patiënte pleit tegen de diagnose beriberi. Bij de cardiale vorm van thiaminedeficiëntie wordt doorgaans een verhoogde waarde gevonden. Aangenomen wordt dat voor het ontstaan van de cardiomyopathie een thiaminedeficiëntie met een duur van ten minste 3 maanden noodzakelijk is. Bij patiënten met hyperemesis gravidarum kan weliswaar encefalopathie van Wernicke ontstaan als gevolg van thiaminedeficiëntie, maar een echte cardiale beriberi is naar mijn weten bij deze toestand nooit beschreven.

Belangrijker is echter dat de interpretatie van de zuurbasetoestand van patiënte onjuist is. Aanvankelijk bestond er een metabole acidose. Na toediening van 800 ml natriumbicarbonaat bedroeg tijdens beademing van patiënte de pH 7,29, de P_{CO_2} 50 mmHg en het bicarbonaatgehalte 21,5 mmol/l. (Bij de berekening van deze laatste waarde moet overigens een kleine fout gemaakt zijn omdat volgens de formule van Henderson Hasselbalch bij de gegeven pH- en P_{CO_2}-waarden een bicarbonaatgehalte van 24 mmol/l past.) Op grond van deze tweede bepaling werd de diagnose metabole acidose gesteld, terwijl de verhoogde P_{CO_2} bij verlaagde pH tot de diagnose respiratoire acidose had moeten leiden. Op zijn hoogst is er sprake van een