• *ABSTRACT*

This paper describes the efforts of clinical scientists and computer experts to introduce computer diagnosis into the wards of a major Australian teaching hospital during the 1960s and 1970s. A logical-empiricist procedure construed as a 'scientific' model of medical diagnosis – and thus challenging traditional physicians' claims of 'craft knowledge' – had the potential to define a new social and institutional role for clinical research. In this account, the 'craft' and 'scientific' representations of diagnosis are treated symmetrically, as discursive resources used in a hospital context to legitimate the divergent competences of two competing occupational subgroups.

Neither 'skill' nor 'science' is privileged as an explanatory framework. Attributions of skill – as of rationality – may serve distinct social goals and institutional interests. In order to secure a place for this diagnostic technology clinical scientists appealed to a scientific method that physicians were prepared to use rhetorically to bolster their diagnoses – but not, in the end, to redefine the diagnostic process. The institutional authority of physicians in this case allowed them to ignore a model of diagnosis that would circumvent their control of a crucial aspect of medical work.

The Reasoning of the Strongest: The Polemics of Skill and Science in Medical Diagnosis

Warwick Anderson

'The reasoning of the strongest is always the best'
(Jean de la Fontaine, *Fables I*, 1668)

In 1971, Dr Ian Mackay, the director of the Clinical Research Unit (CRU) of the Royal Melbourne Hospital, wrote a review of recent advances in medical technology. He observed that 'there is some ill-founded opposition to the introduction of computers into medicine: the fears that computers will become automated diagnostic machines, and so replace the clinician, are reminiscent of the attitudes of

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artisans at the beginning of the Industrial Revolution'.[1] During the previous decade, biomedical computation had been an important part of the CRU's work. Inspired by the attempts of many North American academic medical centres to develop more rational methods of patient assessment,[2] a group of clinical scientists and computer experts had set out to apply what they called a 'scientific method' to medical decision-making. Like so many other doctors involved in clinical research, Mackay believed that a need existed, in the words of one of the American leaders of this enterprise, to promulgate 'rules and standards in terms of completeness and disciplined medical action'.[3]

The clinical scientists and computer experts who promoted biomedical computation argued that the crucial medical work of diagnosis, usually regarded by its practitioners as craft knowledge, could be reduced to discrete components and defined by precise, objective rules. This challenge to traditional medical decision-making began in the late 1960s, when many promoters of artificial intelligence were celebrating the 'scientific' advantages of their new endeavour. They tried to demonstrate that sequential logic and clinical science might replace the traditional practitioner's apparent 'art' or 'skill' of medical diagnosis – whatever it was about traditional diagnosis that could not be formulated in terms of rules ought to be discarded as redundant or obscurantist.[4] The CRU computer scientists presupposed a declarative model of diagnosis: that is to say, they assumed that general rules exist for drawing inferences, independent of context, and in this case these rules need only engage with the relevant facts derived from medical research. This was not a procedural model: the program didn't try to incorporate any special contingencies that a physician would claim to be aware of 'intuitively'.[5] The CRU scientists hoped to construct functional diagnostic hypotheses through logical means, not to represent physicians' descriptions of their own behaviour.

In this paper I suggest that a project to program computers for diagnosis derived from the efforts of clinical scientists and computer experts (both groups more laboratory oriented than ordinary hospital physicians) to attain a socially recognized capacity to make legitimate decisions on ordinary hospital diagnostic matters. Computer modelling of diagnosis meant intervening at the most sensitive point of current work arrangements: negotiation over the labelling of patients and their insertion into case categories and illness careers.[6] For many clinical scientists, a procedure that they construed as a 'scientific analysis' of the dubious 'craft knowledge' of diagnosis seemed thus to have unique potential to define a new social and institutional role for clinical research.[7] More than simply an effort to express adherence to a logical-empiricist method, this enterprise called for the actual reconstruction of diagnostic work along idealized 'scientific' lines. The project would, in effect, challenge the authority of medical 'artisans' in the hospital ward – it would not, like so many scientific innovations, be used simply to enhance or ratify the artisanal control of diagnosis.[8]

This paper treats the 'craft' and 'scientific' representations of diagnosis symmetrically, as discursive resources used in a hospital context to legitimate the divergent competences of the two occupational subgroups.[9] Since this approach stands in contrast with much of the recent work of sociologists of scientific knowledge on issues of skill and craft,[10] it calls for some explanation. I do not intend to engage in the attempt to establish whether medical diagnosis is indeed, as the traditional physicians argued, inarticulable in principle and practice: rather, what I do hope to show is that it served their interests to represent their work as such. Nor do I plan to pick apart the self-serving rhetoric of a 'rational scientific method': the exercise would be redundant, since Collins and others have ably performed that task on many previous occasions.[11] In this context, it is more productive to investigate the strategic value of the clinical scientists' self-presentation. My task is made easier by the astuteness with which each occupational group deconstructed the other's competence and recognized the interpretive flexibility of the other's representations of 'skill' or 'science' – if not their own. Traditional physicians could explain away attempts to develop computer diagnosis as inappropriate science, or pseudo-science. And the clinical scientists constructed a rather more rigorous and critical social analysis of physician 'skill' than many sociologists of scientific knowledge who continue to privilege this particular explanatory framework.[12]

To treat representations of 'skill' and 'science' simply as realistic descriptions or attainable goals would be to miss the point. Here they are better seen as examples of boundary work, stylistic resources designed to legitimate an expansion of expertise into other professional realms.[13] The descriptions of diagnostic work were nuanced to penetrate a perceived weakness in the opponents' vocabulary of practice. Latour has shown that in order to insinuate knowledge into new practical settings, experts must translate the practitioner's interests into the language of their expertise;[14] Mulkay, Pinch and Ashmore have called this process 'colonizing the mind'.[15] The outside experts must convince practitioners of some inadequacy of the conventional approach, without causing offence. In most accounts, the translators and colonizers are victorious, and 'scientization' marches on.[16] Here, though, they were resisted.

Traditional practitioners continued to decide the methods and the modes of expression that could be deemed diagnostic. What counted as a diagnosis had always been tentative and negotiable – even at autopsy. The 'correctness' of the answer depended on who had access to the diagnostic work process. The clinical scientists' expansionist claims threatened rather than complemented the investments of practitioners in diagnosis, and the scientists lacked the institutional authority to enforce their claims beyond the clinical research ward. Eventually, one diagnostic protocol was more persuasive than another not because it was a more 'realistic' description of the phenomenon, but because the dominant agents – traditional physicians – had shaped the language of diagnosis so that a diagnosis only made sense if the would-be diagnostician had done exactly what the physicians themselves thought they did.

The Hospital Context

To a stranger entering the hospital, many occupational distinctions may have appeared blurred. But with more familiarity, one might begin to observe distinctive encounters between scientists and traditional physicians structured by disparate institutional affiliations and different investments in practice and ideology. The CRU scientists, physicians by training, pursued their interests in clinical epidemiology and immunology both in the laboratories of the Hall Institute and in the 27-bed ward of the Royal Melbourne Hospital (RMH) that they supervised. They regarded themselves as clinical researchers, developing a 'basic science of patient care' derived from their study of sick people, using statistical methods and clinical trials.[17] The CRU scientist-physicians distinguished themselves from their more traditional hospital colleagues, who were prepared to embrace new scientific knowledge when it increased the cognitive legitimacy of medical expertise, but who made sure that their professional practices evaded scientific scrutiny. Doctors were ready to take up new diagnostic and therapeutic technologies, but then would use them only in conventional ways.[18] Decisions on individual patient care – on particular cases – still depended on what the practitioners called clinical 'experience', the practical knowledge gained on the ward from apprenticeship and from discussion with peers.[19]

The connections between the CRU, the RMH and the Hall Institute were unusual, but they serve to reveal all the more vividly the contrasting representations of scientific and medical work. The intimate association of the hospital's CRU with the Hall Institute, an independent organization dedicated to what it regarded as fundamental scientific research, allowed many clinical researchers to present themselves as a scientific bridgehead to a relatively untutored hospital staff. In Britain, on the other hand, the clinical research units were contained independently on separate wards within the hospitals; while in the USA, departments of medicine controlled the research and educational activities of the whole hospital, though often one ward was specially given over to research. The arrangements in Melbourne thus displayed more clearly the disparate commitments of clinical scientists and physicians.

As a medical student and young doctor I experienced both domains, and gained a sense of each of them.[20] In moving between the CRU ward and the more traditional wards, I had to negotiate the different investments in technique and self-presentation. Even five years after the computers had been withdrawn, I found that a junior doctor on CRU ward rounds was still expected to adapt to an unusual style of case documentation, and to a strong emphasis on 'scientific justification' of 'diagnostic hypotheses'.[21] There was an obvious contrast between the representation of clinical judgement in the traditional written record as a matter of personal interpretation, and the representation of clinical science in the computer print-out as a rule-based, statistically governed model of patient care. Like many of the junior doctors, I tried to make sense of the tension. In this paper, I try to explain it more formally.

Traditional Diagnosis and Documentation on the Ward

The admission and supervision of patients in all the wards of the Royal Melbourne Hospital (RMH) – as in other large teaching hospitals – has for the last century been entrusted to the 'resident' medical staff, usually an intern and the more senior registrar. After the final medical examinations, the graduates who intend to pursue specialist training apply for intern positions at the major teaching hospitals; and there they will stay, often for eight or more years, until they have gained their specialist fellowships or drifted off into other courses or toward general practice. The consultant physicians attend the medical ward rounds when they are 'on call' for their unit, review the work of their intern and registrar, and arbitrate on patient diagnosis, investigation and management, as well as instructing more formally the group of medical students attached to the ward. The hospital itself employs the junior medical staff, and they follow a roster drawn up by the medical administration.

Under this tutelage, and with clinical experience, junior doctors were expected gradually to learn how to piece together the clues observed in an individual patient, and thus to assemble an acceptable explanation of the case. The clues were not organized according to precise rules; their very designation as evidence depended on the idiosyncrasies of the patient's presentation, and their association into a diagnosis was a result of skilful selection and interpretation. Diagnosis was supposed to be a perceptual skill: one had to discover for oneself the right feel of the accomplishment. The traditional record, which by the 1960s and 1970s appeared natural and compelling, represented the traces of this common understanding of diagnosis: a few salient symptoms and signs, *ad hoc* reasoning, and tentative schemes of action. For the more junior staff, who sought as much practical endorsement as they could get, reproducing a creditable record seemed to substantiate skill in diagnosis.

Diagnosis is traditionally a personal encounter between doctor and patient involving questioning and interpretation, and occurring in a specially designated setting. More important, from the doctor's point of view, diagnosis has to be documented. At the RMH – as elsewhere – the medical record was generally written down, after the encounter with the patient, in a looseleaf binder which may contain anything from four standardized pages to hundreds of them. Occasionally there was a separate volume of investigation results from previous admissions. If a new patient arrived on the ward, the folder included an Emergency Room note (sometimes with the results of the initial diagnostic investigations), or a referring letter. The admitting doctor would usually wait until a nurse completed the initial admission notes (traditionally a social, family and dietary history, with routine observations of temperature, blood pressure, pulse and urinalysis). The medical notes were colour coded and consisted of two history sheets (virtually blank) and two examination sheets which had a checklist on the top that was often passed over, only to be partially reconstructed below in the resident's own handwriting. The checklist could not convey the practical knowledge that was supposed to inform this diagnostic process.

At the end of the physical examination sheets, or at the beginning of the progress sheet, the admitting doctor was expected to write the tentative differential diagnosis. Sometimes one clinical problem could be identified unequivocally, and tests suggested to gauge its extent and prognosis; other times five or more 'differential diagnoses' might be recorded, along with the tests needed to settle the matter. (As a clinician it was difficult to work with more than five diagnostic hypotheses at any one time.) The admitting doctor discussed the findings with the registrar soon after the record was complete; revisions might be suggested, or more questions and further tests; the physical examination would perhaps be reassessed. This revision, the new results and the progress of the patient were looked at again the next day on the consultant's ward round. The intern always presented the findings verbally to the registrar and the consultant, and always in the order: initial impression, history of the present complaint, general history, relevant examination, results, diagnosis, progress. Answers were often tentative, and sometimes no final decision could be made.

The sense one had on the ward of the importance of documentation of diagnostic work is confirmed by a number of outside observers. One computer scientist, assailing the conservatism of doctors, has lamented that the modern medical record, an unwieldy array of medical writing, laboratory reports, photographs, diagrams, nurses' notes and social workers' advice, is 'not just a repository of information: it often forms part of the doctor's thought processes'.[22] But the precise significance of the medical record in patient care depends on the institutional context. Eliot Freidson concluded that doctors in a large group practice regarded medical documentation as a 'natural accretion of everyday work'.[23] Their notes revealed only the more salient aspects of medical care, as the exigencies of practice apparently permitted only cursory record-keeping. In order to infer the details and design of what actually happened, one depended on the physicians' personal knowledge of the patient. This relatively autonomous approach to medical practice was less marked in teaching hospitals. Emily Mumford found that doctors in a university hospital more often expected the medical record to document carefully the details of daily care than their colleagues in a community hospital:

> The [record] is the means through which values are affirmed and articulated in the modern hospital. It is an instrument for socialization of newcomers. It can confirm the group and its medical standards.[24]

Those of us who, during our time as medical students or young doctors, were rostered on rotation to the CRU ward were usually committed to physician training. The three to six months spent there on the CRU ward exposed us to a sample of patients that reflected the unit's interests in autoimmunity and liver disease, as well as to patients with the more common medical disorders; increasingly it also meant we had to adapt, however crudely, to a different style of work and record-keeping. But it was usually a brief sojourn, and so it seemed in the long-term interests of trainee physicians to preserve previously acquired practices of diagnosis and documentation. After all, most of us wanted to make a career as hospital physicians, not as general practitioners or as clinical scientists at the Hall Institute.

Clinical Research and the Hospital

The clinical scientists of the CRU were a relatively isolated segment of the RMH medical staff. Although all had access to a ward of the hospital, many preferred to work in the laboratories of the adjacent Hall Institute. Though all physicians, they owed their appointments as much to proven research ability as to hospital seniority or reputations for diagnostic acumen. Their scientific status ultimately depended on their laboratory-related work. Much of their time was spent trying to understand disease through the techniques of an immunology laboratory, often requiring them to express their findings in terms of statistical relationships. They generally expected that the precise application of an objective scientific method would lead to the solution of most biomedical problems. The everyday ward procedures were often left to the junior ward doctors (a registrar and two residents), trainee physicians on rotation from the hospital rather than would-be scientists. At the regular ward audits, though, the CRU physicians carefully scrutinized the records, and junior doctors had to justify to their seniors the general direction of patient management.

In the hospital, the CRU physician-scientists joined the ward's junior medical officers on rounds and encountered patients either directly through questioning and examination, or through the rituals of case presentation, or through reading the traditional medical history. In association with traditional physicians on the other wards, the junior staff had come to emphasize the importance of the individual case history, to look for those idiosyncratic signs that indicated the course of each patient's illness.[25] But to the CRU consultants it seemed that many elements of these traditional activities could be organized in a far more orderly and 'scientific' manner.

Clinical scientists and trainee physicians had first become juxtaposed in this way when the Hall Institute, then dedicated to virus research, set up a clinical unit in 1946 to confirm what had been an informal connection with the RMH.[26] The Hall Institute derived part of its funding from the RMH, and most of its recruits had been medical students or junior doctors there. It seemed, at the time, that an affirmation of the value of applying scientific techniques generally to ward medicine and therapeutics, and of organizing clinical trials, would help the institute in a number of ways: the political and financial alliance of the institute and the hospital could more easily be maintained; the institute would continue to have access to the clinical material that it required; and it could recruit from a new generation of doctors who had returned from the war eager to substantiate their practice with research on patients.

Agreement was reached with the hospital's committee of management for the new clinical research unit to occupy ward 3E of the hospital, and to conduct an outpatient clinic.[27] Although patients with conditions of special interest to the CRU, such as liver disease and immune dysfunction, could be transferred there with the consent of their hospital consultant, it was expected that most of the patients would have ordinary medical complaints and simply be allocated there from the Emergency Room on receiving days.

The first appointments to the CRU were from the hospital's senior medical staff and so gave the impression of a compatibility in interests and values between the clinical research work and the more traditional diagnostic work. The director, Ian Wood, was a respected hospital gastroenterologist who investigated alcoholic liver disease and gastritis (and invented the peroral gastric biopsy).[28] His research dealt with quite common medical conditions, those obviously relevant to the practices of other hospital physicians, though with little to contribute to the institute's long-established commitment to virus research, or to its emerging interest in immunology. But in 1955, with the appointment of Ian Mackay, an expert in macroglobulinaemia and chronic liver disease, the research orientation of the CRU began to shift. With Macfarlane Burnet, the Hall Institute's director, Mackay began the study of autoimmunity, a complicated subject whose clinical relevance was not immediately obvious.[29] The CRU under Mackay continued to emphasize the study of the immunological basis of many diseases – some of them uncommon – not the problems that the hospital physicians encountered regularly in their everyday practices. Increasingly, the reference group of Mackay and other senior CRU staff consisted of Hall Institute scientists – a group that never had any significance either for the junior ward doctors who aspired to complete physician training, or for the more senior hospital physicians.[30] The CRU was gradually drifting away from the mainstream of hospital practice.

Another problem for the CRU scientist-physicians during the 1960s was that they could no longer claim to be the sole representatives of science in the hospital. The new Melbourne University Department of Medicine under Richard Lovell contrasted in many ways with the CRU, though both ultimately saw their mission as the application of science to patient care. Lovell's goal was principally to provide the opportunity for any of the hospital's medical staff to undertake research in whatever style they liked, without requiring a commitment to the Hall Institute or the abandonment of a private consultation practice.[31] The department was from the start oriented more toward reinforcing traditional notions of clinical practice and physician autonomy; it was more an integral part of the hospital, and its informal associations with other hospital doctors fostered a sense that they were all in the same boat; and it was less hierarchical than the CRU, so research did not tend to be subordinated to the particular interests of its leadership.[32]

The existence of two clinical units in the one hospital increasingly appeared untenable, especially in a place where many administrators and most of the older physicians could not understand the need for any local clinical research. According to Priscilla Kincaid-Smith, the first director of the RMH renal unit and later a professor of medicine:

> The word 'research' was frequently pencilled out of draft documents, but luckily the view of those who reinserted it each time prevailed. A 'service role' was the exclusive order of the day for hospital departments. Research was regarded as something carried out in institutions and universities . . . .[33]

But the hospital's staff and administrators were not the only ones to question the need for two clinical units: in 1962, the National Health and Medical Research Council was initially reluctant to continue to fund the CRU, but finally agreed to do so only 'according to its effectiveness or promise of its immunological work, not simply as a clinical research unit'.[34] The CRU needed a research programme that could legitimate its activities and illustrate a difference of mission, one that would show a distinctive but still a relevant competence, some method that could translate those medical practices that an increasingly distracted group of ordinary hospital physicians deemed important into a language that the CRU staff alone could speak. It seemed that the rational reconstruction of documentation and diagnosis was a project of special relevance to hospital practice.

Computer or Physician?

The 1965/66 Hall Institute *Annual Report* records an intention 'eventually to develop [computer] programmes for the analysis of problems that are especially relevant to clinical immunology: one of these is the significance of associations of disease entities occurring in one patient and his [sic] related family members'.[35] The proposal followed the predictions of Burnet, who though recently retired as institute director, still exerted a tremendous influence on institute policy. In 1964 Burnet had written: 'I can see no escape from the contention that if judgement is to be based on experience, then a machine that can give accurate weight to all the relevant information in terms of a qualitative probability will give a more acceptable answer than any clinician'. He saw the need for an 'army of specially trained technicians' with 'the same responsibility and prestige as used to be reserved for operating surgeons alone'.[36]

Mackay and T.A. McPherson (the CRU's assistant physician, and a recent medical graduate from Alberta, Canada) enthusiastically took up the idea of computer analysis. In contrast to the hospital's Department of Medicine which followed English models, the CRU generally looked more to America and thus often reflected American medical concerns. Burnet, Mackay and McPherson all were well aware of the interest of many North American academic medical centres in developing more rational and efficient methods of documentation. During the 1960s, in the USA, Lawrence Weed was vigorously promoting his problem-oriented medical record (POMR), arguing that it provided a more systematic means of recording and communicating clinical information and, eventually, of making sure that clinical findings were anchored to relevant scientific knowledge.[37] Weed argued that his project meant that, finally, a 'scientific method' might transform clinical medicine. To correct the shortcomings of 'unscientific' practices it was necessary to record, evaluate and audit the logic of physician decision-making. Physicians had to realize that they were deluded in thinking they could master the masses of scientific information on disease: in practice the medical system demanded explicit logical connections between all the available clinical and scientific knowledge and the individual patient's diagnosis. While the POMR in its early stages was, in effect, a consolidation of the manual record system, Weed went on to develop a problem-oriented diagnostic computer program (PROMIS) to guide even more formally the physicians' clinical and therapeutic actions.[38]

The conference on diagnostic data processing held at Rockefeller University during January 1959 had been the first meeting to discuss the prospects of using computers to organize medical care. At the end of their deliberations, the participants, mostly computer scientists, affirmed that electronic data processing in medicine was 'ready to assume the attributes and responsibilities of a mature field of endeavour'.[39] The proliferation of information on disease and the growing demand for medical services had caused traditional medical arrangements to become too fragmented and disorganized. In the early twentieth century, the growth in medical knowledge had been controlled by specialization and an enlargement of the medical record: now the restoration of coherence and completeness in diagnosis and management would require the intervention of electronic data processing.[40]

To the promoters of 'artificial intelligence', record-keeping and diagnosis were potentially observable patterns of behaviour with definite outcomes, and thus potentially amenable to computer modelling. For at least a century clinical scientists had applied statistical analysis to patient responses and behaviour; now, with the help of computer scientists ready to colonize another institutional niche, they had the opportunity to analyze their fellow physicians' behaviour. The clinical researchers might, in principle, improve the reliability of the medical system by reconstructing the behaviour of its more unreliable components, the junior medical staff.

The CRU computer project initially was restricted to the design of a program that would generate more systematic medical records and provide a more uniform database. If this could be achieved, then it might be possible to discern more clearly the statistical patterns of autoimmune disease. When the scheme was first announced in the *Medical Journal of Australia*, it was in terms so precise as now to seem pedantic: the character and capacity of computers were still mysteries for most medical practitioners.[41] The project used a digital computer similar, it was noted, to the ones then in use at the Tulane and Johns Hopkins medical schools. The IBM 7044 was based across the road from the hospital at the University of Melbourne.

The programmer used Fortran to write the necessary routines and instructions. It was an 'open-ended' system, but designed to include initially 'the following information: identification; primary and secondary diagnosis; histopathological and cytological diagnoses; autopsy diagnoses; past, social and family histories; results of pathology tests; and an inpatient summary'.[42] Mainly fixed-field format was used for the input, which meant that a checklist had to be filled out. At first it seemed likely that information as variable as symptoms and signs would require a variable-field format to cover it adequately. But this considerably increased the difficulty of programming the computer, as a dictionary of words had to be provided to analyze the stored free English data. When this narrative format repeatedly defied systematic analysis, the CRU abandoned the idea.

The first tentative efforts at biomedical computation were supposed 'to be used for research rather than the day to day care of patients'. However, as the system developed, the objectives moved 'towards the design of a computer-based medical record that would eventually replace the existing medical record'.[43] Significantly, it was noted that this system might turn out to be 'suitable for the needs of other hospital units'.[44] In 1968, the CRU signalled an expansion of its work on record-keeping when it formally established a biomedical computation unit under the direction of Vance Gledhill, a computer programmer who had spent the last six years at IBM. Gledhill would work toward a PhD, supervised by Ian Mackay and funded by Roche Pharmaceuticals. Within a year he was joined by John Mathews, a physician attached to the CRU. Mathews' interest in epidemiology was channelled into the development of programs for recording large numbers of medical histories, physical examination checklists and laboratory results. He also proceeded to write a dissertation on biomedical computation.[45]

By 1970, Gledhill and Mathews had constructed a self-administered symptom history (SASH) in a fixed-field format.[46] It required the patient to answer 'yes' or 'no' to 284 primary questions, printed in any one of eight languages. From these answers a secondary questionnaire would be generated. For example, if the patient answered 'yes' to the question: 'Do you suffer from headaches?', then the computer would print out further questions relating to the headaches'

FIGURE 1A
A Self-Administered Symptom History Printout

NAME L NUMBER DATE 10/8/70

SELF-ADMINISTERED SYMPTOM HISTORY

MAJOR COMPLAINT
PALPITATIONS, FIRST NOTICED SOME MONTHS BEFORE CONSULTATION, EPISODIC IN NATURE, LESS THAN 5 EPISODIC ATTACKS, EACH ATTACK LASTS SOME MINUTES, THE LAST ATTACK STARTED SOME DAYS AGO, THE SYMPTOM WAS PRESENT AT TIME OF CONSULTATION.
OVERWEIGHT, FIRST NOTICED SOME YEARS BEFORE CONSULTATION RECENTLY BECOME WORSE.

HEAD-ENT
SEVERE HEADACHE IN THE PAST YEAR, OCCURS RARELY, LASTS LESS THAN 2 HOURS, DULL ACHE, FRONTAL, OCCURS AT NO REGULAR TIME, WORSE WITH MOVEMENT OF THE HEAD, RELIEVED BY ASPIRIN, THE SYMPTOM IS NOT CONSIDERED TO BE SERIOUS.

GASTROINTESTINAL
HEARTBURN, FOR MORE THAN A YEAR, COMES AND GOES FOR WEEKS AT A TIME, COMES ON STRAIGHT AFTER MEALS, BROUGHT ON BY PARTICULAR FOODS, RELIEVED BY ANTACIDS, DISCOMFORT IS EPIGASTRIC.

CARDIORESPIRATORY
HAS HAD COUGH NEARLY EVERY DAY, FOR MORE THAN 2 YEARS, FEELS IT IN THE CHEST, WORSE AT NIGHT, COUGHS A LOT DURING THE DAY, COUGH IS PRODUCTIVE, SPUTUM IS BLOODSTAINED, TAKES PROPRIETARY COUGH MIXTURE, THE SYMPTOM IS WORRYING.
HAS HAD A BAD COUGH IN LAST YEAR, WITH PRODUCTION OF SPUTUM, THE SYMPTOM IS WORRYING.
HAS HAD DYSPNOEA, WHEN WALKING QUICKLY, MORE THAN ONE ATTACK IN THE LAST YEAR, ASSOCIATED WITH CHEST PAIN, HAS A WHEEZE WITH THE DYSPNOEA, BROUGHT ON BY COLDS, CONSIDERED TO BE A SERIOUS PROBLEM SOMETIMES.
HAS HAD NOCTURNAL DYSPNOEA, RELIEVED BY SITTING UP.
HAS HAD CHEST DISCOMFORT, SPECIFICALLY TIGHTNESS, WHICH IS INTERMITTENT, ATTACKS LAST A FEW MINUTES, PAIN IS DIFFICULT TO DESCRIBE, RETROSTERNAL, BROUGHT ON BY CLIMBING STAIRS, MADE WORSE BY WALKING QUICKLY, MADE WORSE BY ANXIETY OR WORRY, HAS HAD PAIN ON MOST DAYS, NO RADIATION, RELIEVED BY SITTING UP.
PALPITATIONS, DESCRIBED AS RAPID BEATING AFTER HURRYING, USUALLY IRREGULAR HEARTBEAT, THE SYMPTOM IS WORRYING.

FIGURE 1A cont.

PSYCHIATRIC
THINGS GET ON HIS NERVES,
SOMETIMES DEPRESSED, THIS IS WORST IN THE MORNING,
LIFE SEEMS HOPELESS, CONTEMPLATED SUICIDE.

GENITOURINARY
NOCTURIA, FOR MORE THAN 2 YEARS, TWICE A NIGHT,
THE SYMPTOM WAS NOT CONSIDERED TO BE SERIOUS.

PAST HISTORY
RHEUMATIC FEVER—FIRST OCCURRED, MORE THAN 10 YEARS AGO.
HYPERTENSION—FIRST OCCURRED, MORE THAN 10 YEARS AGO, NO CAUSE FOR HYPERTENSION KNOWN.
HIATUS HERNIA—FIRST OCCURRED, MORE THAN 10 YEARS AGO.
BROKEN BONES—, UPPER ARM, FEMUR, LOWER LEG, DUE TO BEING RUN OVER BY CAR.
TONSILLECTOMY, 5–10 YEARS OLD.
MAJOR OPERATION, HERNIA, BROKEN BONE, HYDROCELE.
CURRENT OR RECENT MEDICATION, HEART TABLETS (DIGOXIN), VITAMIN INJECTIONS.
TAKES RELAXA TABS, ON MOST DAYS OF THE WEEK, TAKING MORE THAN 2 TABLETS A DAY, HAS DONE SO FOR 3 TO 5 YEARS.

FAMILY HISTORY
HEART DISEASE, BROTHER.
DIABETES, FATHER, ONSET BEFORE THE AGE OF 50.
CANCER, SISTER, ONSET BEFORE THE AGE OF 50.

SOCIAL HABITS
WORKS AS A MANAGER.
SMOKES, HAS BEEN SMOKING LESS IN THE LAST YEAR, BEGAN MORE THAN 20 YEARS AGO, MORE THAN 40 CIGARETTES A DAY.
PRESENTLY MARRIED, LIVING WITH SPOUSE, LIVES IN OWN HOUSE OR FLAT.
EDUCATION—FINISHED INTERMEDIATE, CONSIDERS HEALTH TO BE USUALLY GOOD.

FIGURE 1B
A Physical Examination Checklist Printout

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UNIT NO. 398765 **PHYSICAL EXAMINATION** DATE 20/2/71
AGE 49 SEX M TEMP. 36.8 P. 104 RESP. 18 B.P. 170/110

  APPROACH TO PATIENT*******
    * OBESE.
    * UNSHAVEN (2 DAYS GROWTH).
    * ALERT.
    * MOOD, ANXIOUS.
  APPEARANCE—NORMAL

  SKIN AND BODY SURFACE—NORMAL

  EYES—NORMAL

  MOUTH AND PHARYNX—NORMAL

  LARYNX—NOT EXAMINED

  NOSE, SINUSES, EARS—NORMAL

  LYMPHOID SYSTEM—NORMAL

  THYROID—NORMAL

  BREASTS—NORMAL

  RESPIRATORY SYSTEM—NORMAL

  CARDIOVASCULAR SYSTEM*******
    * PULSE RHYTHM, IRREGULAR, PROBABLY DUE TO V.E.S.
    * CARDIAC APEX, SLIGHT CARDIOMEGALY, (APEX BEAT 13
  CM FROM MIDLINE IN 5TH ICS), HEAVING (L.V.).
    * HEART SOUNDS, SOFT S3 AT APEX.
    * PERIPHERAL BRUIT, OVER R CAROTID, (SYSTOLIC, HARSH)

  ABDOMEN—NORMAL

  ANO-RECTAL—NORMAL

  GENITALIA—NORMAL

  SKELETAL SYSTEM—NORMAL

  NEUROPSYCHIATRIC EXAMINATION*******
    * 7TH NERVE* L SIDED WEAKNESS OF MOUTH.
    * MUSCLE WEAKNESS* L SIDED, GENERALISED.
    * GRADING OF WEAKNESS, GRIP (L), (STRENGTH GRADE 4/5)
  ANTERIOR TIBIAL MUSCLES (L), (STRENGTH GRADE 4/5).
    * WEAKNESS ATTRIBUTED TO, PYRAMIDAL LESION.
    * REFLEXES, INCREASED KNEE JERK (L).
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Source: V.X. Gledhill and J.D. Mathews, 'Acquisition and Storage of Clinical Data by Computer', *British Journal of Hospital Medicine*, Equipment Supplement (November 1971), 17, 22. Reproduced by kind permission.

location, severity and duration. A secretary would feed the punch cards for each series of questions into the computer, where the coded set of numbers was converted into sentences of medical English and printed out in narrative form (see Figure 1A). Clearly this procedure, potentially illuminating if the patient was an elective admission with a chronic autoimmune disease, was inappropriate for many acutely ill, distressed or incompetent patients. But most ward patients were stable enough to give the questionnaires proper attention. Gustav Nossal, Burnet's successor as director of the Hall Institute, observed that the 'big advantage of SASH, was that it can be generated in the absence of the doctor . . . '.[47] He had heard some physicians argue that SASH emphasized trivial or irrelevant symptoms, that it could not convey the shades of meaning expressed in a personal interview. These criticisms made him wonder 'whether there is not an exaggerated respect for the subtleties of medical history that has grown out of tradition rather than hard scientific fact'.[48]

Gledhill and Mathews also devised a physical examination checklist (PECL), on which the junior medical staff would record their abnormal findings (see Figure 1B).[49] The physical examination for many years had been more rigorously structured than the history; like many hospitals, the RMH had standard requirements for this task, though in the manual record they could safely be ignored. In the course of the traditional admission of a patient, the intern would report findings to the registrar, who might then check any doubtful observations. But now the intern filled out a checklist which went straight to the computer. Nossal felt sure that 'when one knows that every statement, opinion, test request or treatment suggestion must be defended and justified as it will form part of the computer's learning process, there is a great pressure to conform to the highest scientific standards'.[50]

Special investigations traditionally have been ordered on the basis of a working hypothesis that the doctor reaches after consideration of the history and examination. The computer was thus programmed to recognize which group of signs and symptoms required particular tests. The numerical results of the tests could then be codified and entered into the computer's memory. Programs had already been devised in the United States to interpret ECGs and x-rays, so even the more qualitative results ultimately seemed to be accessible.

The information from all the assembled forms had to be punched on to cards and taken across to the computer. This took about forty minutes for each patient, but secretarial staff were employed to undertake this task.

Toward the end of 1970, Mackay was confident that 'computers will be used for clinical diagnosis in the near future'.[51] The CRU project seemed to attract increasing interest. IBM had recently made available a terminal attached to an IBM 360/67 computer installed in its Systems Development Institute in Canberra. Gledhill and Mathews had recently won the 1970 Karger Foundation prize for their early efforts to develop actual diagnostic programs. Using a computer-aided learning model (CALM), they had determined the diagnostic significance, for all of fifteen specified diseases commonly found in the CRU, of a positive or negative response to each SASH question.[52] They derived this statistically from the questionnaire responses of 95 patients with diagnoses established by agreement on the ward. When the responses of another 484 patients were compared to the responses of the patients with the fifteen specified diagnoses, a weighting was found for each symptom, creating fifteen diagnostic profiles. The diseases were not peculiar immunological disturbances: they included chronic bronchitis, myocardial infarction and alcoholism.

The CALM was put through a carefully limited trial, using questionnaire responses from 45 patients with one or more of the fifteen diagnoses that the program should recognize. The diagnoses were first ascertained by a consensus of the senior CRU staff. CALM came to the agreed answer in 69% of cases, while a method based on Bayes' theorem[53] was 'correct' in 36%, and eight independent clinicians, referring to a list of the fifteen diagnoses and reading the patient responses, were 'correct' in 42% of cases.[54] The physicians did not get to talk to the patients directly, or to examine them. While such a demonstration was hardly likely to convince even the most generous of traditional physicians of the superiority of computer diagnosis in normal conditions of practice, it did at least serve to draw the attention of many hospital doctors to the start of what the CRU hoped to present as a promising diagnostic project.

Quitting the System

In 1969, the hospital administration, responding to a severe nursing shortage, closed the CRU ward for three weeks and distributed the patients elsewhere in the hospital. The decision, taken without consultation with the Hall Institute, incensed the CRU staff. On their behalf, Dr Don Metcalf, the acting director of the institute, wrote to

Dr Hughes-Jones, the chairman of the committee of management of the hospital, pointing out that it was 'imperative that the operation of this highly specialized unit be conducted within the ward especially constructed for the purpose'.[55] In a letter to Sir Colin Syme, the chairman of the institute's board of directors, Metcalf was more explicit: he described specifically the dangers to 'an advanced computer program in operation', and the disruption of connections between the computation lab and the medical and secretarial staff that 'can be expected to cause ultimately a breakdown in this costly project' which was, after all, designed to 'facilitate and improve the treatment of patients'.[56] His point was taken, and the CRU ward was re-opened.

But despite the evidence that the biomedical computation could be deployed with some institutional effect within the hospital, by 1971 the enthusiasm of the CRU physician-scientists had begun to wane. The hospital's general physicians, who had not been formally consulted during the development of the project, showed no sign of interest in the new record-keeping arrangements, and there was no prospect that any pilot projects would be tried on their wards. Indeed, many pointed to the limits of clinical science. Ian Stahle, a consultant physician, observed that 'the rat race of intensive research can so dazzle the vision of those who are overtrained at a scientific level that they fail to see the suffering patient behind the disease'.[57] Richard Lovell was never convinced that computer diagnosis could be practical.[58] 'Let us have more science', declared Sir Albert Coates, a consulting surgeon, 'but still retain the art'.[59] The promoters of the CRU scheme cast about for an explanation of this apparent indifference, if not hostility. Nossal, while ascribing the failure of biomedical computation to spread to other wards in part to problems of cost, complexity, poor communication and conservatism, went on to say that 'many medical men fear automation and computers, either sensing rivals to their own self-assumed omniscience, or harbouring doubts about depersonalisation of medical practice'.[60]

As early as 1970, Gledhill had told a reporter from *The Age* newspaper that: 'There are a lot who don't believe it. Others are disinterested. There's quite a bit of antagonism underneath it all. . .'.[61] Two years later, Mathews noted that while the medical staff tolerated the PECL, which used a format not unlike the orthodox examination record, the SASH, in his opinion, 'proved to be of limited acceptability to inpatients in a public hospital, and also of limited acceptability to doctors'.[62] Since the early days of the project, much time had been spent attempting to 'reconcile "physician acceptability" and "computer acceptability" '. But if the computer was eventually to replace the traditional methods, as Mackay claimed, increasingly it seemed that 'some changes will need to be accepted by clinicians if they are to obtain the full benefit of data processing techniques'.[63]

Yet physicians gave no indication that they were prepared to let clinical scientists restructure the clinical encounter in a 'scientific' way, though they remained keen to have their interpretation of illness scientifically ratified when possible. Nossal reported to the Hall Institute board of directors that, even though 'results have shown that this method of computer-aided diagnosis [was] feasible', the project appeared 'too unconventional to receive support'.[64] During the early 1970s, the biomedical computation unit became ever less salient a feature of the CRU's activities, though some minor alteration of the programs continued until the end of the decade. Social work diagnoses were defined and codified in a social work indices check list (SWIC).[65] In the late 1970s, Dr Jon Buckley developed a program that permitted limited interaction between codes and produced a more flowing narrative report.[66] The open-heart surgery unit at St Vincent's Hospital, Melbourne, took up the SASH, PECL and data retrieval programs for a short time.[67] Dr Rod Lambert codified details of the surgical procedure (Details of Cardiac Surgery, DOCS), including 1680 items, and was able to generate a narrative report.[68] It is significant that surgeons, whose prestige is to a large degree invested in manual dexterity ('surgical skill') rather than 'diagnostic acumen', should accept the computer more willingly than physicians.

In the 1974/75 *Annual Report*, Mackay signalled a tactical withdrawal from the active promotion of biomedical computation. 'Whatever computers may or may not do for the processing of medical records, or for diagnosis making,' he wrote, 'they certainly encourage new and critical attitudes towards data handling and documentation, and decisions in medicine'.[69] This was a departure from the days when the CRU's ambitions had 'increased toward a computer record replacing entirely the conventional record and an on-line computer-based "patient information centre" replacing the conventional medical records library'.[70] In Melbourne at least, the prospect that 'a handful of individuals, drawn largely from university centres and knowledgeable in the arcane arts of computer science as well as medicine, might thus emerge as a new elite', came to naught.[71] During 1975, it was obvious that Nossal had shifted ground and was no longer seeking support for the project, having decided that diagnosis no longer seemed an appropriate Hall Institute research problem.[72] He had come to believe that 'the limiting factor in outcomes of medical interventions rarely relates to diagnosis'.[73] With all the tests available, it was not terribly difficult to make a diagnosis: and when the diagnosis was made, more often than not it was the natural history of the disease, not the treatment offered, that determined the outcome. Improving treatment interventions seemed a more useful activity.

The project was not abandoned on the basis of a crucial experimental result, or any other unassailable proof of its failure or irrelevance. Rather, it was put on hold by a redefinition of what constituted a legitimate problem for clinical research, a redefinition that was more in accord with the interests of traditional practitioners in decision-aiding rather than decision-making technology. Clinical scientists never mastered the 'clever rhetorical footwork' required to convince practitioners – without causing offence – of a deficiency in current diagnostic practices that further computer research might remedy.[74] And yet, CRU physician-scientists remained convinced that conventional approaches were inadequate, and that further development of computer diagnosis was technically feasible, though institutionally indefensible. A strategic withdrawal occurred; there was no consensus on closure. The weaker group of contestants had been driven from the field, unable to muster the institutional strength to continue their challenge to the conventional understanding of diagnostic work.[75]

Although the CRU no longer expected the traditional physicians in the rest of the hospital to be interested in its project, it never gave up entirely its efforts to apply a 'scientific method' to record-keeping on its own ward. The computer itself was removed, but a manual system derived from Weed's POMR remained.[76] For every admission, the intern had to write out the formal history and then fill out the problem-oriented medical synopsis (POMS), a bedside worksheet that was supposed to contain a complete problem list along with the essential features of investigation and management.[77] Clinical criteria, set down plainly on the sheet, had to justify each problem (see Figure 2). All investigation and treatment were 'logically' connected to the proposed problem. When the patient was discharged from hospital, the synopsis became a problem-oriented summary of the case, with vestigial codes from the CRU's intervention attached to each diagnosis. The clinical synopsis was designed 'to introduce a new model of "scientific accountability" to medical information

FIGURE 2
The Clinical Synopsis

DIAGNOSIS/PROBLEM	STATUS	HISTORY	SIGNS	INVESTIGATIONS: ORDERED	INVESTIGATIONS: RESULTS	CONSULTATIONS	TREATMENT: DRUGS	TREATMENT: SURGICAL/OTHER	COURSE	RECOMMENDED MANAGEMENT
~~abdominal pain~~ — ?	1	12 hours ureteric colic on	tender over L side	serum urea	√	DR. JONES	pethidine	5/7/71 LEFT	4	X-RAY ABDOMEN IN 2 WEEKS
ureteric calculus		L with macroscopic	of abdomen	X-ray abdomen	1X2CM CALCULUS L URETER	(UROLOGY)		URETEROLITHOTOMY		
723–915		haematuria		REPEAT X-RAY	CALCULUS NOT MOVED	SURGERY				
				POST OR X-RAY	CALCULUS GONE	ADVISED				
bilateral pyelolithiasis	2	chronic backache, X ray in	both kidneys just						⊥	
		1967 showed bilateral	palpable							
722–915		staghorn calculi								
medullary sponge	9	radiological diagnosis							⊥	
kidney 710–010		1967								
chronic pyelonephritis	3	persistent urinary frequency		urine micro/culture,	PYURIA, 50,000 PROTEUS/ML		cephalexin		⊥	CEPHALEXIN 250MG ORALLY 6
		since 1967, proteus is the		sensitivity	CEPHALEXIN, GENTAMYCIN		GENTAMYCIN			HOURLY URINE CULTURE IN 2
719–127		predominant organism, on		white cell count	WCC = 14,000, NEUTROPHILIA					WEEKS
		long term cephalexin		haemoglobin	√					
systemic lupus	2	diagnosed in 1964, no		anti nuclear antibody	POSITIVE, TITRE $^1/_{10}$		prednisolone		⊥	PREDNISOLONE 5MG B.D.
erythematosus		recent symptoms, on long		LE cells	NEGATIVE					INDEFINITELY
013–955		term prednisolone.		serum iron	22					
				E.S.R.	12					
iatrogenic hypercorticism		prednisolone since 1964,	plethoric facies,						⊥	
881–781	2	dosage now 5mg b.d.	cervical fat pad							
			B.P. $^{150}/_{100}$							
POSSIBLE ADRENAL	7	LEFT URETEROLITHOTOMY	P. 120	serum electrolytes	√		HYDROCORTISONE		2	INCREASED STEROID DOSAGE IS
CORTICAL HYPOFUNCTION		SLIGHT OPERATIVE BLOOD	B.P. $^{120}/_{70}$							ADVISABLE BEFORE ANY FUTURE
(SECONDARY) 881–788		LOSS, TACHYCARDIA POST								SURGERY
		OPERATIVELY								

Source: V.X. Gledhill and J.D. Mathews, 'The Clinical Synopsis', *Australian and New Zealand Journal of Medicine* Vol. 2, No. 2 (1972), 134–41. Reproduced by kind permission.

recording'.[78] But Gledhill felt that even the POMS met resistance from the junior medical staff 'because it differs from traditional practice'.[79] As residents, many of us did appreciate the sense of orderliness that mastering the POMS often provided, but at the same time we criticized the difficulties in explaining the timecourse of different problems, and the vastly greater amount of paperwork we seemed burdened with. We often felt that the CRU staff made unrealistic demands on the capacity for – or interest in – adaptation of junior physicians who were just passing through the ward.

The CRU continued to emphasize its immunological work, and drifted further away from the clinical concerns of the rest of the hospital. When Mackay retired in 1987, the unit was reorganized. Many of its laboratory workers joined other Hall Institute units with compatible research programmes. A re-formed clinical unit was set up in an eight-bed ward of the RMH to conduct research initially on diabetes and the immune response, but this was far from the scale of the old CRU, and appeared more akin to specialist units like cardiology than to a general medical research ward.

Conclusion

In presenting a social explanation of the fortunes of the CRU computer-diagnosis project, I have tried to avoid resorting to assertions about a necessary cognitive form of diagnosis. Whether diagnosis is, in fact, a skill or a science was very much at stake in these institutional manoeuvres. I have therefore attempted to assume here an agnosticism toward the cognitive issues – though I readily admit that when faced with a patient in ordinary medical practice I retain a faith in conventional diagnostic procedures. But even though I am a traditional diagnostician in the clinic, I see no cognitive or fixed psychological objection to computer diagnosis: the problem, I suggest, lies more in reconstituting the social world of medicine so that a non-human actor performing a diagnostic function will be institutionally acceptable. This would imply the acceptance – or enforcement – of a different diagnostic process or grammar (and thus a new definition of diagnostic accuracy); but then diagnosis has not been an invariant category, socially (in terms of the work process) or intellectually.[80] In other words, when one asks 'Can this be a diagnosis?', it has not usually been a matter of establishing the proof or otherwise of diagnostic propositions, but rather it is a question of whether the would-be diagnostician has understood the currently acceptable grammar of diagnosis, and thus is making sense within the dominant culture of diagnosticians. Considered in this way, to look for *necessary* skill or rationality in diagnosis is to reveal an ignorance of the contingency of the procedures. The question, therefore, that I have asked in this paper is not whether a legitimate computer diagnosis *could* be achieved (for obviously I think the eventual answer is 'yes'), but what is necessary socially in order to do it.

The CRU computer project tackled the problems of medical documentation and decision-making, issues of abiding interest to ordinary physicians, but did so in a way that challenged the professionals' image of themselves as skilled diagnosticians – and thus created, in Nossal's words, 'interesting if sometimes difficult and awkward interfaces'.[81] The strength of the clinical researchers' occupational strategy lay in its appeal to a scientific method that physicians were prepared to use rhetorically to bolster diagnosis – but not, in the end, allow to redefine it. Computer diagnosis appeared to threaten the physicians' own sense of themselves as professionals; in particular, it appeared to degrade craft knowledge and clinical memory; and potentially it allowed staff without medical training access to the patient's record.[82] In the RMH, the traditional physicians' definitions of 'diagnostic accuracy' and 'clinical acumen' were what counted in the end. The more traditional physicians had the institutional authority, in this case, to argue that one could never represent diagnosis legitimately in the form of a computer program because their act of diagnosis must always include unaccountable elements acquired only through physician training. The clinical scientists were too socially and intellectually marginal in the hospital to succeed in redefining diagnostic practices in terms more consistent with their own cultural assumptions and institutional interests.

This is part of a larger discourse on physician competence that has been expressed historically in terms of the tension between clinical science and clinical experience.[83] For the past fifty years or more, clinical scientists have attempted to provide formal statistical models to supplement or supplant the reasoning processes of physicians. Their assumption that physicians should be able to explain medical decisions, and permit inspection of medical records, has presented a threat to the individual clinician's sense of competence, though one that the profession has for the most part been able to resist. My analysis of one instance of this discourse on physician competence has treated the languages of 'science' and 'skill' in terms of their representative function: it is not the point of this paper to settle which of these discourses more accurately reflected medical realities. I have focused instead on the role of a struggle for institutional control in generating this discourse, and on the way social authority can be legitimated in the technical language of 'skill' and 'science'. My emphasis has been on studying the use of 'skill' and 'science' as fairly versatile discursive resources and strategies, rather than on analyzing them as non-transferable possessions. I leave to scholars such as Freidson the slippery issue of the actual competence of physicians; I am more interested here in what Good has called the 'symbolic domain of competence'.[84]

One can gain access to this discourse on competence through analysis of what counts as a creditable record of diagnosis: in this case, either the computer print-out or the traditional written record. Within the cultures of clinical science and conventional medicine, each of these documents achieved a compelling symbolic and constitutive status. Each record, in order to be understood, had to be read in the context of the conventions of what each occupation regarded as its typical work: in the context, that is, of what Lynch and Woolgar have referred to as 'the socially distributed competencies which establish the theoretic sense and import of any representational device'.[85] Furthermore, as Garfinkel has suggested, there are good organizational reasons for the apparently 'bad' or 'unmethodical' clinical records of traditional physicians.[86] Hospital staff resist attempts to standardize record-keeping since they use records for local purposes, such as protecting their work from supervisory control. But just as there were good organizational reasons for RMH physicians to resist any attempt to establish a more formal and explicit record of diagnostic reasoning, so too were there good organizational reasons for the CRU clinical scientists to argue that the derivation and documentation of diagnosis should be standardized and controlled, using their computer programs as representations of rational decision-making. Ian Mackay had wanted an accessible record, one 'far more structured and formalized, and devoid of ambiguities'[87] – and thus more open to intervention and supervision – similar to what Garfinkel calls the 'actuarial' record. The traditional physicians' record that it would replace was, in contrast, relatively *ad hoc*, allowing more professional autonomy and opportunity for imaginative play.[88]

Physicians and clinical scientists were enmeshed in their distinct subcultures. Differences in training, commitments and goals meant that each group spoke – as it were – a different dialect; each would often find it was talking past the other. The goal of each group was to protect or consolidate the investment it had in a specific grammar of competence. But, in this case, the more traditional physicians at the RMH had the institutional authority to reject the symbolic framework that the clinical scientists had constructed for their computer. Even as junior doctors, we knew only too well that physicians, not clinical scientists, ultimately controlled the assessment of competence in the hospital – though not in the laboratory – and thus would determine the sense and nonsense, the 'skill' or 'science', of diagnostic method.[89]

•*NOTES*

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1. Ian R. Mackay, 'Developments in Medical Science and Technology', *Postgraduate Medical Journal*, Vol. 46 (1970), 182–90, at 182.
2. The most relevant early studies were: R.S. Ledley and L.E. Lusted, 'Reasoning Foundations of Medical Diagnosis', *Science*, Vol. 130 (3 July 1959), 9–21; M. Lipkin et al., 'Digital Computer as an Aid to Differential Diagnosis', *Archives of Internal Medicine*, Vol. 108, No. 1 (July 1961), 56–72; J.E. Overall and C.M. Williams, 'Conditional Probability Program for Diagnosis of Thyroid Function', *Journal of the American Medical Association* [hereafter *JAMA*], Vol. 183 (2 February 1963), 307–13; H.R. Warner, A.F. Toronto and L.G. Veasey, 'Experience with Bayes' Theorem for Comparative Diagnosis of Congenital Heart Disease', *Annals of the New York Academy of Sciences*, Vol. 115 (July 1964), 558–67; and T.D. Sterling, J. Nickson and S.V. Pollack, 'Is Medical Diagnosis a General Computer Problem?', *JAMA*, Vol. 198 (17 October 1966), 281–86.
3. L.L. Weed, *Automation of the Problem-Oriented Medical Record* (Washington, DC: National Center for Health Services Research, 1977), 15. There are a number of reviews of the enterprise. Those of most relevance to the 1960s and 1970s are: R.A. Shegog, 'Reviewing Some Applications of Computers to Medicine: A Tale of Two Philosophies', in G. McLachlan (ed.), *Problems and Progress in Medical Care* (Oxford: Oxford University Press, 1968); McLachlan and Shegog (eds), *Computers in the Service of Medicine: Essays on Current Research and Applications* (London: Oxford University Press, 1968); W.B. Schwartz, 'Medicine and the Computer: The Promise and Problems of Change', *New England Journal of Medicine* [hereafter *NEJM*], Vol.

283 (3 December 1970), 1257-64; S.J. Reiser, *Medicine and the Reign of Technology* (Cambridge: Cambridge University Press, 1978); D.W. Young, 'A Survey of Decision Aids for Clinicians', *British Medical Journal* [hereafter *BMJ*], Vol. 285 (6 November 1982), 1332–36; and B.M. Kaplan, *Computers and Medicine 1950–80: The Relationship between Policy and History* (unpublished PhD dissertation, University of Chicago, 1983). On the more recent developments, see J.A. Reggia and S. Tuhrim, *Computer-Assisted Medical Decision Making*, 2 Vols (New York: Springer-Verlag, 1985); M.-O. Houziaux and P.J. Lefebre, 'Historical and Methodological Aspects of Computer-Assisted Medical History Taking', *Medical Informatics* (London), Vol. 11 (1986), 129–43; J.G. Anderson and S.J. Jay (eds), *The Use and Impact of Computers in Clinical Medicine* (New York: Springer-Verlag, 1987); and Mike Pringle, 'Using Computers to Take Histories', *BMJ*, Vol. 297 (17 September 1988), 697–98.

4. 'Traditional' was the term used to refer to the established system of record-keeping, while 'rational' was the term the clinical scientists used most frequently to describe their new systems of record-keeping. For the sake of simplicity, I refer to those physicians who were not clinical scientists as being 'traditional'.

5. T. Winograd, 'Frame Representations and the Declarative/Procedural Controversy', in D.G. Bobrow and A.M. Collins (eds), *Representation and Understanding: Studies in Cognitive Science* (New York: Academic Press, 1975), 185–210; A.V. Cicourel, 'The Reproduction of Objective Knowledge: Common Sense Reasoning in Medical Decision-Making', in G. Böhme and N. Stehr (eds), *The Knowledge Society: The Growing Impact of Scientific Knowledge on Social Relations* (Dordrecht: Reidel, 1986), 87–122.

6. Eliot Freidson, *Profession of Medicine: A Study of the Sociology of Applied Knowledge* (New York: Dodd, Mead, 1970); Anselm Strauss et al., *Social Organization of Medical Work* (Chicago, IL: The University of Chicago Press, 1985).

7. On the history of the disparate commitments of clinical scientists and ordinary physicians, see Joseph Ben-David, 'Scientific Productivity and Academic Organization in Nineteenth-Century Medicine', in B. Barber and W. Hirsch (eds), *The Sociology of Science* (New York: Free Press & London: Collier-Macmillan, 1962), 305–28; R.C. Maulitz, ' "Physician versus Bacteriologist": The Ideology of Science in Clinical Medicine', in M.J. Vogel and Charles Rosenberg (eds), *The Therapeutic Revolution* (Philadelphia, PA: University of Pennsylvania Press, 1979), 91–108; S.E.D. Shortt, 'Physicians, Science and Status: Issues in the Professionalization of Anglo-American Medicine in the Nineteenth Century', *Medical History*, Vol. 27 (1983), 51–68; David Armstrong, 'Clinical Sense and Clinical Science', *Social Science and Medicine*, Vol. 11 (1977), 599–601; J.V. Pickstone et al., 'Exploring Clinical Research: Academic Medicine and Clinicians in Early Twentieth-Century Britain', *Bulletin of the Society for the Social History of Medicine*, Vol. 37 (1985), 79–81; Christopher Lawrence, 'Incommunicable Knowledge: Science, Technology and the Clinical Art in Britain 1850–1914', *Journal of Contemporary History*, Vol. 20 (1985), 503–20; and Harry Marks, 'Notes from the Underground: The Social Organization of Therapeutic Research', in Russell Maulitz and Diana Long (eds), *Grand Rounds: One Hundred Years of Internal Medicine* (Philadelphia, PA: University of Pennsylvania Press, 1988), 299–338.

8. Judith Perolle, 'Expert Enhancement and Replacement in Computerized Mental Labor', *Science, Technology, & Human Values*, Vol. 16, No. 1 (Spring 1991), 195–207.

9. For 'symmetry', see David Bloor, *Knowledge and Social Imagery* (London:

Routledge & Kegan Paul, 1976). My concern with orientations toward practice, investments in competences, the constructive activity of scientists and doctors, the inseparability of technical capacity and social power, and with exchange systems, owes much to the work of Pierre Bourdieu, especially his 'The Specificity of the Scientific Field and the Social Conditions for the Progress of Reason', *Social Science Information* , Vol. 14 (1985), 19–47, and 'The Peculiar History of Scientific Reason', *Sociological Forum*, Vol. 6 (1991), 3–26.

10. Particularly Harry Collins, *Artificial Experts: Social Knowledge and Intelligent Machines* (Cambridge, MA: MIT Press, 1990). Many participants at the 1990 Bath Conference on 'Rediscovering Skill in Science, Medicine and Technology' seemed preoccupied with identifying the characteristics of skill, treating it as a self-evident non-transferable possession.

11. H.M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (London & Beverly Hills, CA: Sage, 1985); J.A. Schuster and Richard Yeo (eds), *The Politics and Rhetoric of Scientific Method* (Dordrecht: Reidel, 1986); see also Lucy Suchman, 'Representing Practice in Cognitive Science', in Michael Lynch and Steve Woolgar (eds), 'Representation in Scientific Practice', Special Issue, *Human Studies*, Vol. 11, Nos 2–3 (April/July 1988), 305–25, republished as *Representation in Scientific Practice* (Cambridge, MA: MIT Press, 1990), at 302–21.

12. See Collins, op. cit. note 10. Collins emphasizes the social dimensions of even the most formal activities, and argues that behaviour cannot be explained by, or reduced to, explicit rules, unless our actions have (like production-line work) become machine-like, or unless we simply become more charitable to machines. In a sense, what I study here is a failed effort to encourage such charity. The diagnosis that scientists modelled is not a reduction of the diagnostic behaviour of physicians as much as an attempt to design a medical procedure that will be deemed to make sense as a diagnosis, given a restructured grammar of medical activities. I argue that all method talk – whether emphasizing formal rules or craft knowledge – is a social activity, and should be studied sociologically.

13. T.F. Gieryn, 'Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in the Professional Ideologies of Scientists', *American Sociological Review*, Vol. 48 (1983), 781–95.

14. Bruno Latour, 'Give Me a Laboratory and I Will Raise the World', in Karin Knorr-Cetina and Michael Mulkay (eds), *Science Observed: Perspectives in the Social Study of Science* (London & Beverly Hills, CA: Sage, 1983), 141–70; Latour, *Science in Action* (Milton Keynes, Bucks.: Open University Press, 1987).

15. Michael Mulkay, Trevor Pinch and Malcolm Ashmore, 'Colonizing the Mind: Dilemmas in the Application of Social Science', *Social Studies of Science*, Vol. 17 (1987), 231–56.

16. For example, Latour (1983), op. cit. note 14, recounts the relentless advance of bacteriology.

17. Alvan Feinstein, *Clinical Judgement* (Baltimore, MD: Williams & Wilkins, 1967), and his 'An Additional Basic Science for Clinical Medicine: I. The Constraining Fundamental Paradigms', *Annals of Internal Medicine*, Vol. 99 (1983), 393–97; 'II. The Limitations of Randomized Trials', ibid., 544–50; and Arthur Elstein, 'Clinical Judgement, Psychological Research and Medical Practice', *Science*, Vol. 194 (12 November 1976), 696–700.

18. Charles Bosk, *Forgive and Remember* (Chicago, IL: The University of Chicago Press, 1979); Friedson, op. cit. note 6; and D.R. Gordon, 'Clinical Science and Clinical

Expertise: Changing Boundaries between Art and Science in Medicine', in M. Lock and Gordon (eds), *Biomedicine Examined* (Dordrecht: Kluwer, 1988), 257–98.

19. For similar arguments, see T.A. Parrino and R. Mitchell, 'Diagnosis as a Skill: A Clinical Perspective', *Perspectives in Biology and Medicine*, Vol. 33 (1989), 18–44. Medical diagnosis has frequently been used as an example of a skilful activity not amenable to logical–empiricist renderings: see, for example, Michael Polanyi, *Personal Knowledge* (London: Routledge & Kegan Paul, 1958); and H.L. Dreyfus and S.E. Dreyfus, *Mind over Machine: The Power of Human Intuition and Expertise in the Era of the Computer* (New York: Free Press, 1986).

20. I was a medical student on the CRU ward in 1980, and an intern there in 1984.

21. L.L. Weed, 'Medical Records that Guide and Teach', *NEJM*, Vol. 278 (14 March 1968), 593–600.

22. D.W. Young, 'What Makes Doctors Use Computers?: Discussion Paper', *Journal of the Royal Society of Medicine*, Vol. 77 (August 1984), 663–67, at 664.

23. E. Freidson, *Doctoring Together: A Study in Professional Social Control* (New York: Elsevier, 1975), 167. On the history of the medical record, see S.J. Reiser, 'Creating Form out of Mass: The Development of the Medical Record', in E. Mendelsohn (ed.), *Transformation and Tradition in the Sciences: Essays in Honor of I.B. Cohen* (Cambridge: Cambridge University Press, 1984), 303–16; also C.A. Nathanson and M.H. Becker, 'Doctors, Nurses and Clinical Records', *Medical Care*, Vol. 11 (1973), 214–23.

24. E. Mumford, *Interns: From Students to Physicians* (Cambridge, MA: Harvard University Press, 1970), 141.

25. More generally on this, see: Everett Hughes, 'The Making of a Physician: General Statement of Ideas and Problems', *Human Organization*, Vol. 14 (1956), 21–24; Strauss et al., op. cit. note 6; and Paul Atkinson, *The Clinical Experience: The Construction and Reconstruction of Reality* (Farnborough, Hants.: Gower, 1981).

26. The best account of clinical science in Melbourne, and of the Walter and Eliza Hall Institute (WEHI), is Macfarlane Burnet, *Walter and Eliza Hall Institute 1915–1965* (Melbourne: Melbourne University Press, 1971); see also G.J.V. Nossal, *Medical Science and Human Goals* (Melbourne: Edward Arnold, 1975), and his 'The Responsibilities of Scientists and Government for Research within a Medium-Sized Country', in M. Bennett (ed.), *Science and Society in Australia* (Canberra: AAS, 1986), 95–100. V. deV. Davis, *A History of the Walter and Eliza Hall Institute of Medical Research* (unpublished PhD thesis, University of New South Wales, 1979) is useful, but concerned predominantly with Hall Institute administration.

27. Joint meeting of RMH and WEHI, *Minutes* (1 April 1946), 1.

28. Sir Ian Wood, *Autobiography* (Melbourne: Lothian, 1972).

29. Burnet, op. cit. note 26, 78. Mackay had been a medical student at RMH, but then worked in an Australian country hospital before obtaining a research position in Seattle. In Melbourne, with Carleton Gajdusek, he developed the autoimmune complement fixation test, and with Burnet he helped to define the new field of autoimmunity. Mackay wrote a number of important papers on chronic active hepatitis and SLE. With Burnet he wrote the classic *Autoimmune Diseases: Pathogenesis, Chemistry and Therapy* (Springfield, IL: Thomas, 1963). Burnet later won a Nobel Prize in Physiology and Medicine for his work on immunological tolerance.

30. See Hughes, op. cit. note 25, for remarks on reference groups.

31. Interview with Professor R.R.H. Lovell, October 1987. I was a medical student in Lovell's Department in 1981. Lovell discusses his ideas of clinical research in 'The

University Medical Presence in Hospitals Seventy Years after Flexner – and a Look Ahead' [Stawell Oration 1982], *Australian and New Zealand Journal of Medicine* [hereafter *ANZJM*], Vol. 13 (1983), 187–94.

32. Interview with Dr Robert Fraser, July 1989.

33. Professor Priscilla Kincaid-Smith, in the *Report of the Board of Medical Research of the RMH* (Melbourne, 1987), 3.

34. Burnet, op. cit. note 26, 76.

35. WEHI *Annual Report* (1965–66), 58.

36. F.M. Burnet, 'Fifty Years On', *BMJ* (1964), Vol. 2 (31 October), 1091–93, at 1092.

37. Weed, op. cit. note 21; L.L. Weed, *Medical Records, Medical Education and Patient Care: The Problem-Oriented Record as Basic Tool* (Cleveland, OH: Case Western Reserve University Press, 1971). Lawrence Weed was a senior physician at Cleveland Metropolitan General Hospital, Western Reserve School of Medicine, in Ohio.

38. The best account of this is H.P. Lundsgaarde et al., *Human Problems in Computerized Medicine* (Lawrence, KA: University of Kansas Press, 1981); see also P.J. Fischer et al., 'User Reaction to PROMIS: Issues Related to the Acceptability of Medical Innovations', in Anderson & Jay (eds), op. cit. note 3, 286–302.

39. Reiser, op. cit. note 3, 202.

40. Lee Lusted, 'Computer Techniques in Medical Diagnosis', in R.W. Stacy and B.D. Waxman (eds), *Computers in Biomedical Research*, Vol. 1 (New York: Academic Press, 1965), 319–38.

41. T.A. McPherson and I.R. Mackay, 'A Computer Project in Clinical Immunology', *Medical Journal of Australia* [hereafter *MJA*] (1967), Vol. 1 (6 May), 924–27.

42. Ibid.

43. V.X. Gledhill, T.A. McPherson and I.R. Mackay, 'The Application of Computers to Medical Records: A Computer System for the Storage and Retrieval of Medical Records', *Australasian Annals of Medicine*, Vol. 19 (1970), 1–8, at 1.

44. WEHI *Annual Record* (1966–67), 27. By 1968, Mackay was confident enough to tell a reporter that: 'We see this system being fully established in every hospital in Victoria and then Australia wide, with the central data bank probably in Canberra. . . ': Lennard Bickel, 'Computer will be Used for Diagnosis', *The Australian* (11 March 1968).

45. J.D. Mathews, *Morbidity: A Computational Approach* (unpublished PhD dissertation, University of Melbourne, 1972). Mathews is the director of the Menzies Medical College, Darwin.

46. V.X. Gledhill, J.D. Mathews and I.R. Mackay, 'Computers in the Service of Medicine', *MJA* (1970), Vol. 2 (19 December), 1200–04, at 1202.

47. Nossal (1975), op. cit. note 26, 165.

48. Ibid., 166; interview with Sir Gustav Nossal, October 1987.

49. Gledhill, Mathews & Mackay, op. cit. note 46.

50. Nossal (1975), op. cit. note 26, 170.

51. WEHI *Annual Report* (1970–71), 56.

52. Ibid.; Gledhill, Mathews & Mackay, op. cit. note 46.

53. Bayes' theorem states that the probability of any disease in a given patient depends on: (1) the prevalence of that disease in the population; (2) the finding of a particular symptom of that disease; and (3) the likelihood of the symptom being associated with that disease.

54. WEHI, op. cit. note 51. Nossal reported to the WEHI Board that the test indicated that the computer 'outshines physicians in diagnostic ability': 'Director's Bimonthly Report to the Board, 17 November 1969', 5, WEHI Archives: 0010, 'WEHI Board'.

55. D. Metcalf to E. Hughes-Jones, 29 April 1969, WEHI Archives: 0052, 'CRU'.

56. D. Metcalf to C. Syme, 29 April 1969, WEHI Archives: 0010, 'WEHI Board'.

57. I.O. Stahle, 'Medicine at the Hospital', *RMH Quarterly* (Autumn 1969), 18–19, at 19.

58. Interview with Professor R.R. Lovell, October 1987.

59. Sir A. Coates, 'The Hospital over the Last 50 Years', *RMH Quarterly* (Spring 1969), 13–15, at 13.

60. Nossal (1975), op. cit. note 26, 161. He continues: 'The case memory of a given doctor is limited and fallible. The case memory of the computer is, in principle, virtually infinitely expandable, and certainly not susceptible to the sorts of errors that human memories exhibit', 168. For another account of contemporary physician suspicions, see R.A. Thatcher, 'The "Take-over Bid" by Computers in Medicine', *MJA* (1969), Vol. 1 (14 June), 1258–59.

61. V.X. Gledhill, quoted in 'Diagnosis by Machine', *The Age* (18 November 1970).

62. Mathews, op. cit. note 45, 1.

63. WEHI *Annual Report* (1968–69), 57.

64. 'Director's Bimonthly Report to the Board, 21 September 1970', WEHI Archives: 0010, 'WEHI Board'.

65. WEHI *Annual Report* (1976–77), 72.

66. WEHI *Annual Report* (1974–75), 86.

67. Ibid., 73.

68. More generally on the CRU's work on narrative reports, see J.D. Buckley, 'Narrative Report Generation from Numerically Coded Data', *Computers and Biomedical Research*, Vol. 11 (1978), 525–36.

69. WEHI, op. cit. note 67, 73.

70. Gledhill, Mathews & Mackay, op. cit. note 46, 1202.

71. R.L. Teach and E.H. Shortliffe, 'An Analysis of Physician Attitudes Regarding Computer-Based Clinical Consultation Systems', in Anderson & Jay (eds), op. cit. note 3, 80.

72. Interview with Dr I.R. Mackay, September 1987.

73. G. Nossal to W. Anderson, personal communication, 14 August 1991.

74. On another group who did not step lightly, see Mulkay, Pinch & Ashmore, op. cit. note 15.

75. On this form of 'closure', see Everett Mendelsohn, 'Political Anatomy of Controversy in the Sciences', in H. Tristram Englehardt, Jr & Arthur L. Caplan (eds), *Scientific Controversies* (Cambridge: Cambridge University Press, 1987), 93–124. Also on methods of closure, or stabilization, see T. Pinch and W. Bijker, 'The Social Construction of Facts and Artefacts, or How the Sociology of Science and the Sociology of Technology might Benefit Each Other', *Social Studies of Science*, Vol. 14 (1984), 399–441, and Sharon Beder, 'Controversy and Closure: Sydney's Beaches in Crisis', ibid., Vol. 21 (1991), 223–56.

76. V. Gledhill and J. Mathews, 'The Clinical Synopsis', *ANZJM*, Vol. 2 (1974), 131–41.

77. V.X. Gledhill et al., 'The Problem-Orientated Medical Synopsis', *Annals of Internal Medicine*, Vol. 78 (1973), 685–91, at 690.

78. WEHI *Annual Record* (1971–72), 88.
79. Gledhill et al., op. cit. note 77, 690.
80. Indeed, it could be argued that the physicians' understanding of diagnosis is already undergoing a transformation as medical culture itself is altering. Much of the traditional medical diagnosis of 'easy' cases could be characterized as machine-like, to use Collins' term, and the practitioner's craft knowledge is redundant – while diagnosis of 'hard' cases is frequently never resolved through the current procedures, in any case.
81. G. Nossal to W. Anderson, personal communication, 14 August 1991.
82. Indeed, they went as far as to suggest that patients might check their own records: D.P. Stevens, R. Stagg and I.R. Mackay, 'What Happens when Hospitalized Patients See Their Own Records', *Annals of Internal Medicine*, Vol. 86, No. 4 (April 1977), 474–77. Of course, one of the striking features of this dispute is that neither group contesting the means of diagnosis seriously expected the opinions of patients to influence the debate.
83. For an illuminating discussion of approaches to the study of physician competence, see Mary-Jo D. Good, 'Discourses on Physician Competence', in R.A. Hahn and A.D. Gaines (eds), *Physicians of Western Medicine: Anthropological Approaches to Theory and Practice* (Dordrecht: Reidel, 1985), 247–67; Armstrong, op. cit. note 7; Shortt, op. cit. note 7; Lawrence, op. cit. note 7.
84. Freidson, op. cit. note 6.
85. Michael Lynch and Steve Woolgar, 'Sociological Orientations to Representational Practice in Science', in Lynch & Woolgar (eds), op. cit. note 11 (1988), 99–116, at 110, (1990) 1–18, at 13.
86. Harold Garfinkel, *Studies in Ethnomethodology* (Englewood Cliffs, NJ: Prentice-Hall, 1967), esp. 186–207. In particular, there is an interest in avoiding specifics in the record.
87. Mackay, op. cit. note 1, 182.
88. Garfinkel, op. cit. note 86, 200.
89. For another perspective on the lack of inevitability in the 'scientification' of skill, see Richard Whitley, 'The Transformation of Expertise by New Knowledge: Contingencies and Limits to Skill Scientification', *Social Science Information*, Vol. 27 (1988), 391–420.

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