sampling and were deemed to be at increased risk of relapse by virtue of tumour size (T3–4) or clinically node positive (N+) were randomised to receive axillary radiation or no further local treatment [8].

Premenopausal patients (1473) were all given adjuvant CMF and postmenopausal cases (1202) received tamoxifen. In both pre- and postmenopausal cases, there was a significantly increased axillary relapse rate in the non-irradiated group. Furthermore, the overall survival was significantly increased in the premenopausal irradiated group (68 versus 63%). It was concluded that adjuvant systemic treatment alone did not prevent locoregional relapse in high-risk cases.

Use of radiotherapy rather than surgery than surgery to treat the axilla can lead to an increase in morbidity. In the Edinburgh trial, 417 patients undergoing total mastectomy were randomised to have either an axillary clearance or sampling [9]. Those in the sampled group who had histological evidence of nodal involvement were given axillary radiotherapy. When this group was compared with the node positive group treated by clearance, there was a significant increase in reduced arm mobility, severe interference with daily activity and lymphoedema in the irradiated group.

AXILLARY NODAL STATUS AND PROGNOSIS

Despite a multinational billion dollar industry which has tried to replace pathological nodal status by other prognostic factors, usually related to the behaviour of the primary tumour, the pathologist still remains the most significant prognostic indicator. This information is now known by an increasing number of patients. Thus, the majority wish to know their likely prognosis and, if axillary staging has been inadequate, this will be a mission impossible. This latter aspect is often forgotten by doctors who have not appreciated that patients will have opinions and needs with regards to their future prognosis and treatment.

Finally, it may be argued that as more cases are diagnosed as a result of mammographic screening so they will have small (<1 cm) tumours with a good prognosis. A recent study from Guy's Hospital of 336 women with tumours up to 1 cm revealed that 31% had axillary nodal metastases [10]. Furthermore, patients with impalpable tumours and nodal involvement had a significantly worse prognosis than those who had palpable lumps with nodal involvement.

Of course, it has to be accepted that approximately half of the patients undergoing axillary surgery will have pathologically negative nodes. This does not mean that the operation was unnecessary, since the patient and her doctor will be better informed. Naturally, the procedure will be replaced as imaging techniques are able to indicate true negativity.

However, it will still be necessary to treat the axilla when there is pre-operative evidence of nodal metastases. Whether this should be by surgery or radiotherapy remains a moot point. What is most important is a multidisciplinary approach to treatment, with the surgeon playing an important role in both local treatment and staging.

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PII: S0959-8049(96)00202-X

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When a treatment has been painstakingly validated over many years—and when even longer has been required to convert practitioners at large to its use—there is understandable reluctance to welcome criticism of the approach in question. Times change, however, and the conventional wisdom of 1986 or 1991 may not prove equally valid in 1996. As suggested in the accompanying commentary (Fentiman, pages 1460–1461), multidisciplinary decision-making is an appropriate policy for the modern age; it is therefore entirely appropriate for medical oncologists to discuss the utility of a surgical procedure justified in large part by its impact on medical decision-making. Here I address the concerns expressed above in response to my recent review of routine axillary nodal dissection (RAND) [1].

The key point of the original article was not that RAND has become less effective as a prognostic exercise, but that the clinical decision-making value of nodal staging has declined substantially over the last decade. To reiterate the evidence for this view [1] here would be futile; suffice to say, however, that the simplistic 1980s rule-of-thumb "no nodes, no adjuvant therapy" is now virtually defunct. This partly reflects the growing realisation that such patients remain at high risk of premature death, and is especially relevant to older women in whom tamoxifen is likely to be prescribed whatever their nodal status. Even for younger women in whom chemotherapy is being considered, substantive adjuvant chemotherapeutic benefit is now recognised in node-negative cohorts [2]; preliminary results from the updated Early Breast Cancer Trial-

ists' Collaborative Group overview are believed to strengthen this conclusion. Despite this point, many of the prognostic studies cited in the commentary (e.g. the Danish) have minimal relevance to therapeutic outcomes. For example, much is made of a new study in which impalpable node-positive tumours are associated with a worse prognosis than palpable node-positive tumours [3], even though this finding indicates less about the clinical utility of nodal status than it does about that of tumour size [1].

A further weakness of the pro-dissection arguement lies in its failure to acknowledge the morbidity of RAND. The ultimate question is not whether RAND is associated with any benefits, after all, but whether these benefits are of sufficient magnitude to justify the surgery. There would be little reason to forgo RAND if the morbidity and costs were agreed to be trivial, but this is not the case [1]. Proof of RAND's value thus mandates a comparison of its positive and negative effects; the latter may be less dramatic than the former, but are also far more widely distributed. (This raises larger issues beyond this discussion: is 'best' treatment always synonymous with 'most' treatment? And in a world of dwindling resources, is the notion of 'good-enough' treatment valid when it comes to health care?)

Several claims in the commentary are overstated. The declaration is made that 'inadequate axillary treatment' leads to increased distant recurrence, for example, and a study is cited in which such treatment was associated with increased mortality [4]. However, the implication that untreated nodal involvement *per se* predisposes to lethal downstream metastatic events is not generally accepted, differing as it does from the Fisherian paradigm of nodal disease as a marker of systemic disease [5]. Both randomised [6] and retrospective [7] studies have associated improved survival with more aggressive axillary surgery, but such studies have been confounded by failure to control for differing frequencies of adjuvant drug administration; as modest as the benefits of the latter may be, it remains the only intervention reproducibly shown to improve survival in breast cancer. Irrespective of which individual studies may be cited to support aggressive local treatment, the overwhelming weight of randomised evidence now confirms that survival is broadly similar with or without RAND [8].

With respect to local control, it is true that the efficacy of adjuvant drug treatment in preventing axillary relapse remains unclear, but the literature includes many studies at variance with the single negative reference quoted [1]. A recent study has shown that the 5-year axillary recurrence rate in undissected clinical node-negative patients receiving adjuvant tamoxifen is approximately 4% [9]; as explained previously, approximately half of such recurrences occur in the context of disseminated disease, while half the remaining 'isolated' recurrences are controllable with salvage interventions [1]. The 5-year incidence of uncontrolled isolated relapses in this cohort thus seems likely to be around 1%—raising the question as to whether one prevented recurrence justifies 99 pure 'diagnostic' dissections. Similarly, the caveats cited concerning the NSABP protocol are valid, but do not thereby overturn the conclusions of a unique study [10].

The final section of the commentary betrays a flaw in its logic: namely, the notion that doctors' (or patients') curiosity about breast cancer prognosis in itself constitutes ample reason for RAND. While such thinking is doubtless popular—and in all probability afflicts medical oncologists as often as surgeons—it in no way justifies the routine execution of an invasive and morbid procedure. Every medical student is taught that investigations are justifiable exclusively on the basis of potential management impact; however, reasonable decisions about adjuvant drug therapy can often be made without nodal staging. This does not imply that nodal staging provides trivial information, but rather reflects the fact that the variety of adjuvant therapeutic interventions is strictly limited. By admitting that curiosity is indeed a major justification for RAND, the credibility of the latter policy is undermined.

The Janus-like status of RAND as half-investigation/half-therapy has long deflected criticism on either front. However, dogmatism regarding the value of RAND will remain inappropriate until and unless randomised studies are undertaken in cohorts defined by impalpable nodes, routine adjuvant systemic therapy, and regular monitoring of the axilla for prompt and expert salvage intervention. Endpoints of such a study would be limited to survival, quality of life, and frequency of uncontrolled isolated axillary recurrences. An interesting exercise for RAND proponents would be to propose an appropriate sample size for such a study. This calculation would demand a quantitative estimate of the procedure's benefits or, at the very least, a consensus definition of 'minimum worthwhile benefit'.

To represent the current uncertainties over RAND as a demarcation dispute between surgeons and oncologists is unconstructive. Notwithstanding the collapse of Halstedian theory [5], surgeons in 1996 play a central role in breast cancer management—as, indeed, do radiotherapists and medical oncologists. My view is not that axillary dissection should be abandoned forthwith for all patients, but rather that there has emerged a valid and important question as to whether patient subsets are identifiable in which the probability of benefit from RAND differs substantially. The charge of 'therapeutic nihilism' is a potentially damaging one in this era of frustratingly slow clinical progress; however, the siren-like voices of comfortable habit, morbid curiosity and iatrogenic denial pose a far greater hazard to therapeutic progress than do those of continuing experimentation and verification, no matter how threateningly 'nihilistic' the latter may seem to some. Reasoned discussion—combined with a willingness to submit one's convictions to the acid test of prospective randomised studies—is surely the way forward.

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Acknowledgement—The author thanks the Cancer Research Campaign for salary support.

PII: S0959-8049(96)00237-7

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IAN FENTIMAN and Richard Epstein's commentaries on routine axillary node dissection (RAND) make excellent reading. I found myself totally convinced by the arguments first of one, then of the other, and was left wondering whose point of view would get my 'vote' at the end of a well argued debate. The arguments in favour of RAND are that it is a simple surgical procedure which provides excellent local tumour control with minimal morbidity, that it provides important staging and prognostic information, and that the alternative 'watch policy' may increase the risk of uncontrollable axillary recurrence and possibly even have an effect on overall prognosis. The arguments against are that axillary irradiation provides comparable rates of local tumour control to axillary surgery, and that decisions regarding adjuvant systemic therapy can be made without a knowledge of axillary node status. What are busy clinicians reading this Journal going to do with the conflicting information and views given to us?

The most contentious area in this debate is whether or not untreated nodal involvement might have an adverse effect on survival. The general consensus from randomised studies is that survival is broadly similar with or without RAND. Nevertheless, I think that this question is one about which there is still lingering doubt. It may well be that there is a subgroup of patients for whom ineffective local treatment may result in long-term survival disadvantage as argued by Harris and Osteen [1], Stotter and colleagues [2], and in Haagensen's experience of Halsted mastectomy [3] showing long-term survival of patients with multiple involved nodes despite the absence of any systemic therapy at all. This issue becomes increasingly important given the rising number of screen-detected cancers being treated by Specialist Breast Units. The rate of axillary node positivity for incident round screen-detected invasive cancers is little different to that for symptomatic breast cancers, and in aiming to give these patients long-term survival, it is important that we are not tempted into under treatment.

Regardless of any effect on survival, an important role of the team treating the primary breast cancer must be to achieve local control—either by surgery or radiotherapy to the axilla. A 'watch policy' reserving treatment in the event of axillary recurrence is, in my view, not an acceptable option because of the devastating effect that this event can have on a woman suffering recurrence, even if you reassure her that it may not have any adverse impact on survival.

The current debate for and against RAND is reminiscent of the debates in the surgical and oncology literature of the 1970s concerning staging laparotomy for Hodgkin's disease. With improving non-surgical staging techniques (such as CT scanning of the abdomen) and increasing efficacy of systemic therapies, the need for staging laparotomy was lost. Hopefully the same will happen for axillary surgery in breast cancer. However, we do not yet have a staging investigation which will accurately predict the negative axilla. We do not yet have systemic therapy which will consistently control local disease. Until we have developed these improvements, I will stick with Ian Fentiman's view that RAND should remain part of the primary management of breast cancer (my non-surgical colleagues agree!).

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