A randomized trial of preexercise stretching for prevention of lower-limb injury

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ABSTRACT

POPE, R. P., R. D. HERBERT, J. D. KIRWAN, and B. J. GRAHAM. A randomized trial of preexercise stretching for prevention of lower-limb injury. *Med. Sci. Sports Exerc.,* Vol. 32, No. 2, pp. 271–277, 2000. **Purpose:** This study investigated the effect of muscle stretching during warm-up on the risk of exercise-related injury. **Methods:** 1538 male army recruits were randomly allocated to stretch or control groups. During the ensuing 12 wk of training, both groups performed active warm-up exercises before physical training sessions. In addition, the stretch group performed one 20-s static stretch under supervision for each of six major leg muscle groups during every warm-up. The control group did not stretch. **Results:** 333 lower-limb injuries were recorded during the training period, including 214 soft-tissue injuries. There were 158 injuries in the stretch group and 175 in the control group. There was no significant effect of preexercise stretching on all-injuries risk (hazard ratio [HR] = 0.95, 95% CI 0.77–1.18), soft-tissue injury risk (HR = 0.83, 95% CI 0.63–1.09), or bone injury risk (HR = 1.22, 95% CI 0.86–1.76). Fitness (20-m progressive shuttle run test score), age, and enlistment date all significantly predicted injury risk ($P < 0.01$ for each), but height, weight, and body mass index did not. **Conclusion:** A typical muscle stretching protocol performed during preexercise warm-ups does not produce clinically meaningful reductions in risk of exercise-related injury in army recruits. Fitness may be an important, modifiable risk factor. **Key Words:** RANDOMIZED CONTROLLED TRIAL, STRETCHING, INJURY

Many athletes stretch their muscles before exercise because they believe this reduces the risk of injury. However, there is no evidence that preexercise stretching does, in fact, reduce injury risk.

Only one randomized controlled trial has specifically examined the effects of preexercise stretching on injury risk (26). In that study, 1093 male army recruits were randomly assigned to either a stretch or control group. Subjects in the stretch group stretched their calf muscles during warm-ups, whereas control group subjects did not. The incidence of five selected lower leg injuries in the stretch group (4.2%) did not differ significantly from that of the control group (4.6%). However, because the statistical power was low, the authors could not rule out the possibility that a small but worthwhile effect of stretching did exist. A less rigorous way to determine the effects of stretching is to compare the incidence of injury in naturally occurring cohorts who do and do not stretch. To our knowledge, four such cohort studies (5,12,18,37) have been performed, and none have found any reduction in the incidence of injury for those that stretch. As cohort studies provide a potentially biased estimate of the effects of therapeutic interventions, and as the only randomized controlled trial did not have sufficient statistical power to definitively rule out an effect of stretching, we conducted a randomized controlled trial to determine whether preexercise stretching, performed as part of the preexercise warm-up, reduces injury risk.

The study was performed on army recruits undergoing basic training, because recruits undertake a rigidly controlled program of exercise and sustain a high incidence of lower-limb injury (26,27). The stretching protocol (one 20-s static stretch of each of six key lower-limb muscle groups) was chosen because it was similar to those typically advocated and practiced (2,3,11,29–31,34), because it complied with recommendations based on basic muscle research (9,19) and because it was acceptable to the physical training instructors. Static stretching was employed because it is easily performed, is thought to be relatively safe, and is just as effective as alternative means of stretching at increasing musculotendinous compliance (2,3,7,11,20,29,30). A multivariate analysis was performed to examine interactions of stretching with fitness, age, height, weight, body mass index (BMI), and day of enlistment. These variables were nominated before the conduct of the study. The first five variables were chosen on the basis of research that suggested an association with injury risk (5,13,14,35). The relationship

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TABLE 1. Physical training content of the recruit training course.

Activity	Description	Total Hours
Route marching	Marching at speeds of 5–7 km·h^{-1} over specified routes of 2–12 km carrying a backpack and rifle	10
Running	Running distances of 4–8 km over specified routes	10.5
Obstacles	Running, jumping, scaling walls, vaulting, and negotiating other obstacles	12.5
Circuit training	Running, situps, pushups, weights	7.5
Swim/swim circuit	Swimming, pool entry and exit, poolside sit-ups/push-ups	4
Battle training	Wrestling, log lifts, fireman's carry training, shoulder rolls	5.5
Total		50

between day of enlistment and injury risk was of interest because it was popular belief in the Australian army that injury rates rise later in the year. This was thought to be related to systematic changes in the pool of recruits over the course of the year.

METHODS

Subjects. About 1800 male recruits are trained each year at the Australian Army's 1st Recruit Training Battalion, situated at Kapooka, in rural New South Wales. At the time of this study, enlistment criteria included absence of any history of significant injury, good general health (both assessed by preenlistment medical screening), age between 17 and 35 yr, and psychological suitability (assessed by paper-and-pencil aptitude tests). Usually two or more platoons, each of about 40 recruits, went through the 12-wk (80-d) training program simultaneously. The training included an 11-wk period (40 sessions totaling 50 h) of intense physical exercise, closely supervised by physical training instructors. A summary of the content of this physical training program is given in Table 1.

All 1589 male recruits entering the training program between January and December 1994 were invited to participate in the study; 1538 recruits aged between 17 and 35 yr gave written, informed consent to participate. On the basis of power calculations performed before the study, this was expected to provide a high probability of detecting a 25% reduction in injury risk, if such an effect existed. All procedures were approved by the Australian Defence Medical Ethics Committee and by the Ethics Committees of Charles Sturt University and the University of Sydney.

Allocation. Those recruits who consented to participate were allocated to stretch or control groups using a blocked, stratified, random allocation procedure. As the male recruits arrived at Kapooka, they were assigned to platoons on the basis of surnames, by army administrative staff. Recruits with surnames commencing with the same letter were equally split between platoons. In addition, where possible, recruits with the same surname were allocated to different platoons. No other conditions influenced allocation to platoons. All allocation procedures to this point were conducted by administrative staff at Kapooka, without regard for the research to be conducted.

Pairs of platoons, formed as described above, were then randomly allocated to stretch or control groups (stretch group 19 platoons, 735 subjects; control group 20 platoons, 803 subjects), so that one platoon from every pair was allocated to each group. Physical Training Instructors (PTI) were assigned to platoon pairs, so that the platoons allocated to each group were matched for PTI. There were an odd number of platoons because the last platoon intake within the study period was not matched with an accompanying platoon. The initial procedure used to allocate recruits to platoons was unlikely to produce systematic differences between control and stretch groups, and systematic differences were made even more unlikely by random allocation of platoons to control or stretch groups.

Procedures. Before this study, army protocol dictated that, during the training program, physical training instructors were to ensure that all recruits performed stretches before they undertook strenuous physical exercise. Such exercise occurred, on average, once every second day, and the exact method of stretching and the muscle groups to be stretched varied according to instructors' preferences. For this study, subjects in the stretch group received self-administered stretches designed to lengthen the gastrocnemius, soleus, hamstring, quadriceps, hip adductor, and hip flexor muscle groups (33) during the warm-up before all physical training sessions. One 20-s stretch, timed by a stopwatch, was performed in the manner described by St. George (33) for each specified muscle group in each leg. Stretches were interspersed with 4 min of warm-up activities such as jogging and side-stepping. Subjects in the control group performed only warm-up activities and did not stretch. The small number of recruits who chose not to participate in the study were permitted to perform stretches of their choice during the warm-up period. The warm-up and stretching protocol was taught, directed, and closely supervised by the physical training instructors. Instructors ensured that subjects in the stretch group performed stretches in the correct manner and for the prescribed duration. The authors ensured that this protocol was followed by conducting regular checks at lessons and by discussing the protocol being performed with individual recruits and platoon staff outside physical training sessions.

The stretching protocol of one 20-s static stretch for each muscle group conformed with typically advocated protocols (1,11,22,28,36) and would be expected to produce a significant increase in joint range of motion (9,19) and a significant reduction in resistance to applied stretch (21). A common set of instructions always preceded stretching routines. These included a demonstration of the specific stretch technique, initial fault correction, and advice regarding the stretch sensation that should be experienced by the recruit. The stretch sensation sought was described as "stretch, not pain; pain means danger."

On arrival at the training facility, data regarding the height, weight, BMI, and age of each recruit were recorded. Initial fitness assessment included a 20-meter progressive shuttle run test (20mSRT; 15). The 20mSRT has been shown to be a valid and reliable indicator of both maximum aerobic capacity and running ability (16,17,23). 20mSRT scores were obtained for 1317 (86%) of the 1538 subjects, but, for administrative reasons, scores were not available for the other 221 subjects.

Injury data collection. Throughout the recruit training program, all injuries were reported to medical assistants or nursing staff. When the injury was more than trivial, or when the recruit was unable to resume full duties without signs or symptoms within 3 d, these staff referred injured recruits to the regimental medical officer (RMO) as standard procedure. The RMO was asked to make a diagnosis and refer all of these recruits to the researchers. The researchers regularly liaised with medical assistants, nursing staff, and the RMO to ensure referral protocols were followed.

Injury was defined as any lower-limb injury that prevented the subject from resuming full duties, free of signs or symptoms, within 3 d. The RMO, who was masked to patient allocation, categorized all injuries by area and type:

1. Area: foot, ankle joint, tibia, fibula, tibiofemoral joint, patellofemoral joint, femur, hip joint, sacroiliac joint, pubic ramus (superior or inferior), pubis, ilium, ischium, pubic symphysis, lower leg (nonbone), or thigh (nonbone).
2. Type: Bone (traumatic, periostitis, or stress fracture; all confirmed by radiographs, CT scan, or bone scan), ligament sprain, muscle strain, joint pain/dysfunction (articular), tendonitis, muscle compartment pressure syndrome (compartment pressures greater than 15 mm Hg), meniscal lesion, or other soft-tissue injury (for example, bursitis).

Data analysis. Survival analysis (24) was used to examine the effects of stretching on injury incidence and the predictive value of other variables for injury. To test the effects of stretching, a univariate Cox regression model was used (24). We separately considered effects of stretching on all injuries combined, all bone injuries, and all soft-tissue injuries. Effects were analyzed in terms of likelihood ratios (LR), which provide an indication of the extent to which group membership accounts for differences in incidence of injury (24). A multivariate Cox regression model (24) was then used to examine the effects of stretching while controlling for 20mSRT score, age, height, weight, BMI, and day of enlistment. Multivariate analyses were again conducted for all injuries combined, for soft-tissue injuries alone, and for bone injuries alone. A step-wise elimination procedure (24) was used to remove variables that did not contribute significantly to explaining the risk of injury. Similar multivariate analyses were performed *post hoc* for specific types of injury where more than 30 of the specific injuries were recorded. A probability of less than 5% was considered significant for *a priori* hypotheses, and a probability of less than 1% was considered significant for hypotheses formulated *post hoc*. All analyses were conducted with SPSS software (SPSS Inc., Chicago, IL).

Once a subject had presented with a lower-limb injury his survival time was considered to be terminated. This prevented any bias related to further presentation with the same injury, or with an injury secondary to the first. If a recruit presented with two or more lower-limb injuries simultaneously, only the primary injury was recorded. Recruits discharged from the army or transferred to officer training before completion of recruit training had their survival times censored accordingly. Thus only recruit training days actually completed before injury were taken into account in the final analysis. Survival analysis does not assume equal time at risk for each subject but instead uses total time at risk to determine relative risk of injury (24). Data from recruits who were "backsquadded" (reassigned to a later platoon because they were having difficulty with some aspect of training) and recruits who withdrew consent to participate in the study were analyzed by intention to treat (6,25,32). This meant that monitoring of these recruits continued after backsquadding or withdrawal, so that injury and time in training data were included in the statistical analyses as if the recruits continued with their allocated treatment.

RESULTS

Sample attrition. Of the 1538 participating recruits, 170 (11%; 69 from the stretch group, and 101 from the control group) were discharged or transferred to officer training before the end of the training program and without suffering a lower-limb injury. Censored training times for each of these 170 subjects were included in the overall analysis. In survival analysis, each subject's result is weighted by the number of days he was at risk, and thus this censoring was unlikely to have a confounding effect.

Eighty-nine subjects (5.8%; 46 control subjects and 43 subjects from the stretch group) were backsquadded to another platoon during the course of training. A further 94 subjects (6.1%), all from the control group, withdrew from the study. These subjects reported that they wished to perform lower-limb muscle stretches before exercise and withdrew within the first 3 wk of training. At the time of withdrawal or backsquadding, none had suffered a lower-limb injury. Injury data were still available for these subjects, so their data were analyzed by intention to treat (6,25,32).

Injury record. Altogether 333 lower-limb injuries were recorded: 175 in the control group and 158 in the stretch group. A total of 96,021 training days was recorded for all subjects combined, equating to 60,013 h of physical training. The incidence of lower-limb injury was therefore 3.5 injuries per 1000 training days, or 5.5 injuries per 1000 h of physical training (note that subjects who sustained an injury were thereafter removed from the analysis). A summary of injury types, sites, and frequencies is provided in Table 2.

Effect of stretching. The univariate Cox regression model revealed no significant effect of stretching on all-injuries risk (likelihood ratio [LR] = 0.18 for 1 *df*, $P = 0.67$; Fig. 1). The hazard ratio (HR), which is equal to the injury rate in the stretch group divided by the injury rate in the control group, was 0.95 (95% CI 0.77–1.18). This HR is close to 1.00, which would indicate no difference in injury rates between groups. No effect of stretching on injury risk was observed when soft-tissue injuries were examined separately (LR = 1.86 for 1 *df*, $P = 0.17$, HR = 0.83, 95% CI 0.63–1.09; Fig. 1), or when bone injuries were examined

TABLE 2. Summary of injuries recorded.

Lower-Limb Injury Type	Injury Site	Treatment Group		
		Control	Stretch	Total
Bone				
Stress fracture	Tibia	24	32	56
	Foot	10	11	21
	Femur	4	0	4
	Fibula	1	3	4
	Ilium	2	0	2
	Pubic rami	1	1	2
Acute fracture	Patella	1	0	1
Periostitis	Tibia	10	15	25
Stress changes	Foot	2	2	4
Soft-tissue				
Joint (articular)	Patellofemoral joint	40	27	67
Ligament sprain	Ankle joint	27	19	46
	Tibiofemoral joint	6	2	8
	Foot	5	7	12
Muscle strain	Shank	9	10	19
	Thigh	10	2	12
	Hip rotators	2	2	4
Tendonitis	Thigh	9	10	19
	Shank	7	10	17
	Hip rotators	1	0	1
Meniscal lesion	Tibiofemoral joint	1	2	3
Compartment syndrome	Shank	1	1	2
Other (e.g., bursitis)	Thigh	1	0	1
	Tibiofemoral joint	0	1	1
	Shank	1	1	2
Totals		175	158	333

separately (LR = 1.24 for 1 *df*, $P = 0.27$, HR = 1.23, 95% CI 0.86–1.76; Fig. 1).

Multivariate model for prediction of injury risk. Multivariate analysis involved the 1317 subjects for whom 20mSRT scores were obtained. The distribution of 20mSRT scores was close to normal, with a mean of 8.7 (SD ± 1.6; Fig. 2). The stepwise regression model for prediction of risk of lower-limb injury retained only 20mSRT score (LR = 47.3 for 1 *df*, $P < 0.001$), age (LR = 7.1 for 1 *df*, $P = 0.008$) and day of enlistment (LR = 16.1 for 1 *df*, $P < 0.001$). As expected on the basis of the univariate analysis, there was no significant effect of muscle stretching on injury risk (LR = 0.02 for 1 *df*, $P = 0.89$, HR = 1.04 with 95% CI 0.82–1.33). Height did not contribute significantly to the final model for prediction of risk of lower-limb injury (LR = 1.1 for 1 *df*, $P = 0.30$) and neither did weight (LR = 1.9 for 1 *df*, $P = 0.17$) or BMI (LR = 0.8 for 1 *df*, $P = 0.38$). Models for prediction of lower-limb injury risk from each significantly contributing variable are depicted in Figure 3 and are derived from the final multivariate model:

$$\mathrm{RIR} = e^{-0.2656\times(20\mathrm{mSRT}-8.68)+0.0534\times(\mathrm{age}-19.44)+0.0028\times(\mathrm{DE}-122.59)}$$

where relative injury risk (RIR) is calculated relative to the risk of injury associated with average fitness (20mSRT score), age (yr), and day of enlistment (numbered day of the year).

Separate multivariate analyses were performed for soft-tissue injuries and bone injuries. Significant predictive variables for soft-tissue injuries included 20mSRT score (LR = 21.7, $P < 0.001$), age (LR = 5.7, $P = 0.02$) and day of enlistment (LR = 11.7, $P < 0.001$). For bone injuries, significant predictive variables included 20mSRT score (LR = 28.5, $P < 0.001$) and day of enlistment (LR = 6.4, $P = 0.01$). There was no significant effect of stretching on injury risk in either case. The relationships between 20mSRT score and risk of injury for bone injuries and for soft-tissue injuries are depicted in Figure 4. These relationships are similar to that between 20mSRT score and overall lower-limb injury risk. *Post hoc* analyses of injury subgroups suggested that 20mSRT score was a significant ($P < 0.01$) predictive factor for risk of stress fracture, tibial stress fracture, patello-femoral injury, and muscle strains, but not for risk of ankle sprain ($P = 0.52$).

DISCUSSION

The aim of the present study was to examine whether a typical program of muscle stretching performed during warm-up before exercise affects the risk of exercise-related injury. No effect was detected, despite good statistical power. These results confirm and extend the findings of a smaller randomized trial which we performed on the same population (26). In that study we found that calf muscle stretching did not reduce the risk of selected lower-limb injuries, but we were unable to rule out small but clinically worthwhile effects.

The findings of the present study clearly indicate that a typical preexercise stretching protocol does not produce a

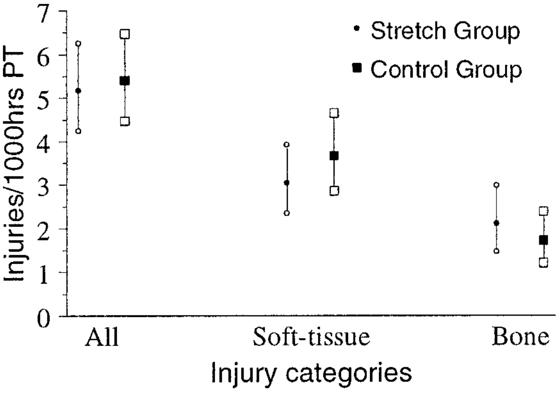

Figure 1—Effect of preexercise stretching on injury risk, with 95% confidence intervals.

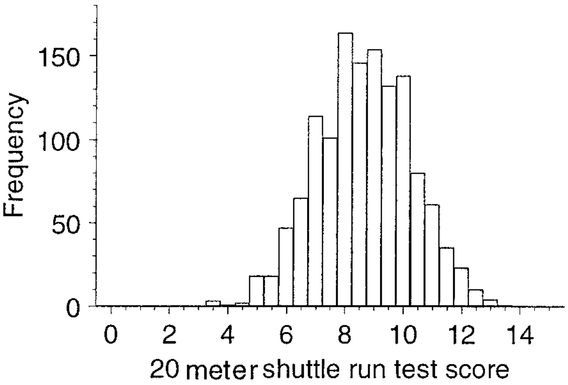

Figure 2—Frequency distribution histogram for recorded 20mSRT scores.

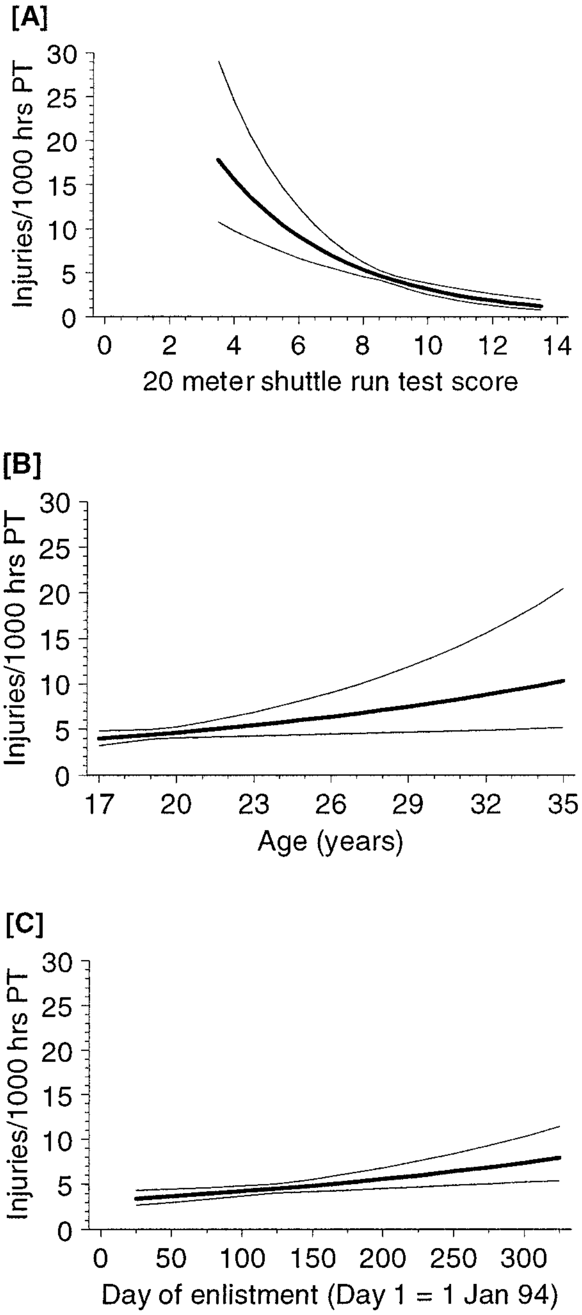

Figure 3—Injury risk vs (A) 20mSRT score, (B) age, and (C) day of enlistment; 95% CI for the population estimates of injury risk are also shown.

clinically useful reduction in injury risk. Our best estimate of the effect of stretching is that it reduces all-injury risk by 5%, and we are able to rule out a 23% or greater reduction in injury risk with 95% certainty. When these results are expressed in absolute terms, the futility of stretching becomes apparent. Recruits stretched for 40 sessions over the course of training, and so, on average, each recruit would need to stretch for 3100 physical training sessions to prevent one injury. As it took 5 min to complete the stretches, an average of 260 h of stretching would be required to prevent one injury (95% CI 50 h to prevent one injury to 65 h to produce one injury). Clearly, even the most optimistic effects consistent with our data are of dubious clinical significance. Most populations are at lower risk of injury than the army recruits investigated in the present study, and so it is probable that the value of stretching is even less in those populations (10).

It is possible that the muscle stretches employed in the current study were not sustained for long enough to produce sufficient physiological changes in the musculotendinous unit to reduce injury risk (21). This possibility needs to be tested with a randomized controlled trial. However, the potential benefits of more prolonged stretching need to be weighed against the time spent stretching. Athletes may be reluctant to stretch if the time spent stretching detracts significantly from the available training time or exceeds the expected reduction in time away from sport due to injury.

Fitness, which was assessed using the 20mSRT, proved to be a consistent and strong predictor of injury risk across all soft-tissue and bone injury categories except ankle sprain. The least fit subjects were 14 times more likely to sustain a lower-limb injury than the fittest subjects (Fig. 3A). This finding is consistent with the results of studies by Jones et al. (13) and Blair et al. (5), but we believe that the 20mSRT is a more efficient screening test than the 1-mile runs (13) and treadmill testing (5) used in these other studies. It is a simple, quick, reliable, and valid indicator of the aerobic capacity and running ability of each subject (16,17,23).

Although a strong and consistent relationship was demonstrated between 20mSRT score and lower-limb injury risk, it has not been established that the relationship is a causal one. Further research is required to determine specific correlates of the 20mSRT score that afford protection from injury. It is possible that aerobic fitness (the construct that the 20mSRT purports to measure) is such a correlate, but it is also possible that running ability (skill and coordination), bone mass, psychological attributes, or the tensile strength of contractile and noncontractile soft-tissues play some role. The findings of the present study suggest that it may be worthwhile instituting fitness training programs for army recruits scoring poorly on the 20mSRT in an attempt to reduce risk of injury, but properly designed clinical trials will be required to determine whether this prevents injury.

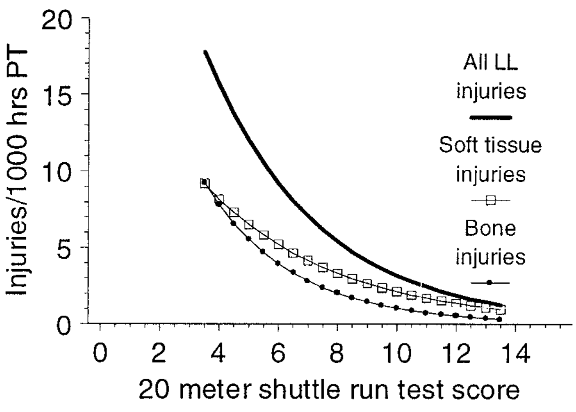

Figure 4—Injury risk vs 20mSRT score for bone injuries, soft-tissue injuries, and all injuries analyzed together, based on the Cox regression models for each.

The greatest value of the 20mSRT lies in its potential to identify those individuals at highest risk of injury. Training can be adjusted to avoid overtaxing these individuals, or they can be excluded from intense training programs.

Analysis of the association between age and risk of injury was limited by the fact that ages of recruits included in this study were restricted to 17–35 yr by the army enlistment criteria. Nonetheless, age was significantly associated with overall risk of injury (Fig. 3B) and risk of soft-tissue injury, so that injury was more likely in older recruits. This finding conflicts with those of Walter et al. (37) and Macera et al. (18), who investigated cohorts of runners and found no association between age and injury risk. In contrast, Blair et al. (5) found age was positively related to injury risk in a cohort commencing new run or walk programs. Given these results, and the fact that army-style marching and running is new to most recruits at Kapooka, it is possible that age is a greater risk factor among individuals commencing a novel walk or run program than among habitual runners and walkers. Further research may clarify this issue.

Recruits enlisting late in the year had more than twice the injury risk of recruits enlisting early in the year (Fig. 3C). Further monitoring of injury rates across the calendar year over several years is required to establish the consistency of the phenomenon. This factor added significantly to the multivariate predictive model for injury risk, indicating that the phenomenon was not explained by differences in fitness or age of recruits enlisting in each time period. Confounding variables such as weather, environmental conditions, and psychological aptitudes of enlistees might deserve investigation if the phenomenon is replicated in other studies.

We found no association between injury risk and the height and weight of subjects. This is consistent with the findings of Finestone et al. (8), who found no association between height or weight and incidence of stress fractures. Our findings suggest also that height and weight do not predict soft-tissue injuries. Based on these findings, there is little evidence to suggest that subject height or weight influence risk of injury independently of fitness, age, and date of enlistment.

An association between BMI and injury risk was not detected in the current study. This is in contrast to the findings of several previous studies (4,5,13) but consistent with the findings of Macera et al. (18) in a cohort of habitual runners. A possible explanation is that the range of BMI scores in this study was relatively narrow because of enlistment criteria. These criteria demanded a BMI between 20 and 25 $kg \cdot m^{-2}$ unless evidence of good health and fitness was available, which may have obscured an association between BMI and injury risk in training. This limitation did not exist in the study by Macera et al. (18), as these investigators reported a wide range of BMI scores in their 583 subjects.

The results of this randomized, controlled trial indicate that preexercise muscle stretching does not produce a clinically worthwhile reduction in the risk of lower-limb injury. Injury risk is strongly associated with age and 20mSRT scores. This suggests that fitness may be a modifiable risk factor for injury.

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