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GROUPE  
D'ÉTUDE CANADIEN SUR  
LES SOINS DE SANTÉ PRÉVENTIFS

**The Use of Back Belts for Prevention of  
Occupational Low-Back Pain  
Systematic Review and Recommendations**

**December 2002**

# **TECHNICAL REPORT**

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**PREVENTIVE HEALTH CARE, 2002 UPDATE - THE USE OF BACK BELTS  
FOR PREVENTION OF OCCUPATIONAL LOW BACK PAIN:  
SYSTEMATIC REVIEW & RECOMMENDATIONS**

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## **ABSTRACT**

**Objective:** To systematically review the evidence and provide recommendations for primary health care providers on the use of back belts for primary prevention of low back pain at work.

**Options:** Combination of back belts, education and exercise for primary prevention of back pain.

**Outcomes:** The incidence of reported low back pain and the duration of lost time (absenteeism) from work for low back pain, as compared between initially asymptomatic workers with and without back belts.

**Evidence:** MEDLINE, CINAHL, EMBASE and HEALTHSTAR databases were searched for relevant articles published up to June 2002 using the following key words: back, lumbar, spine, belts, supports, braces, orthotic devices, prevention and occupational. Pertinent references from articles obtained from the above search were also reviewed.

**Benefits, Harms and Costs:** Back injuries account for over 25% of all lost time claims in Canada, making it the largest single category for all Workers' compensation claims. Low back pain is also estimated to be the most costly ailment of working age adults. Prevention programs directed at the worker, including education, exercise, pre-employment screening and the use of back belts, are by far the most common preventive strategy in industry. Potential negative effects of back belt use include rubbing, pinching or bruising of ribs, hampered sitting and driving, excessive sweating and a false sense of security. Other potential risks based on laboratory studies include cardiovascular strain, back muscle weakening and abdominal hernia.

**Values:** The strength of the evidence was evaluated using the methods of the Canadian Task Force for Preventive Health Care.

**Recommendations:** The CTF concludes that the existing evidence (Levels I, II-2) is conflicting and does not allow making a recommendation for or against the use of back belts to either prevent occupational low back pain or to reduce lost work time due to occupational low back pain (**C Recommendation**).

**Validation:** The findings of this analysis were reviewed through an iterative process by the members of the Canadian Task Force for Preventive Health Care.

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Back pain continues to be the leading overall cause of morbidity and lost productivity in the workplace. Recently there has been a renewed interest in the use of back belts by industry to reduce the incidence of occupational low back pain (LBP).<sup>1-4</sup> In this paper we systematically review the available evidence for the effectiveness of back belts in preventing occupational LBP.

In Canada, back injuries account for over 25% of all lost time claims, the largest single claims category in most workers' compensation jurisdictions.<sup>5</sup> Disability resulting from low back pain is the most common chronic health problem in adults under the age of 45 and second only to arthritis in those aged 45 to 65.<sup>6</sup> In terms of costs LBP is estimated to be the most costly ailment of working age adults.<sup>7,8</sup>

There are three main categories of potential risk factors for occupational LBP- individual, biomechanical and psychosocial. For a given individual, the strongest risk factor is a previous history of LBP, along with the severity of the previous episode.<sup>7</sup>

The most consistent associations among biomechanical risk factors have been exposure to lifting or carrying heavy loads, whole body vibration, and frequent bending and twisting.<sup>1,7,9-12</sup> Although the literature on psychosocial risk factors for LBP is less consistent,<sup>1,12-17</sup> there is growing empirical evidence linking psychosocial stressors, such as perceived high workload, time pressure, lack of intellectual discretion and job dissatisfaction, with an increased risk of occupational LBP.<sup>7,9,13,18-20</sup>

There are two main primary prevention strategies for occupational LBP: those directed at the individual worker; and those directed at the workplace. Workplace strategies involve modification of the work site to suit the worker, such as ergonomic job re-design.

Programs directed at the worker are by far the most common preventive strategy in industry,<sup>21</sup> and include: education (e.g., improving knowledge regarding lifting techniques and injury awareness); exercise (e.g., improving strength and overall fitness); pre-employment screening efforts to detect risk factors such as smoking, obesity, previous LBP and psychosocial factors; and the use of mechanical back supports (e.g., belts or corsets).<sup>11,15,21,22</sup>

## **METHODS**

The published English literature to June 2002 was identified with a computerized search of MEDLINE, CINAHL, EMBASE and HEALTHSTAR databases using the following keywords: back, lumbar, spine, belts, supports, braces, orthotic devices, prevention and occupational. Pertinent references from articles obtained from the above search were also reviewed. Studies were included

if the study participants were material handlers (i.e. exposed to lifting), and outcome measures included the incidence and/or duration of lost time of reported LBP among workers who wore back belts compared to those who did not. No restriction was made on the style of back belt used.

The evidence was systematically reviewed using the methodology of the Canadian Task Force on Preventive Health Care<sup>23</sup>, including review by the Task Force, and independent review by external experts (Appendix 1). The quality of the evidence was scored using a double rating method first according to research design, then by internal validity rating using design specific criteria described by Harris et al.<sup>24</sup> The internal validity of each study was assessed independently by two authors (CA, MK). The main internal validity items and levels of evidence are summarized in Table 1. Consensus was used to resolve disagreements in scoring.

## **RESULTS**

Ten epidemiologic studies meeting inclusion criteria were identified. These include five randomised controlled trials,<sup>25-29</sup> two non-randomised controlled trials,<sup>30,31</sup> two cohort studies<sup>32,33</sup> and one survey.<sup>34</sup> A summary of the study quality evaluations is shown in Table 2; study findings are summarised in Table 3.

### *Controlled Trials*

Walsh and Schwartz randomly assigned 90 warehouse workers into three equal groups to receive either no intervention, a one hour training session in lifting techniques and back pain prevention, or one hour of training and a back belt for use during working hours.<sup>28</sup> No group was assigned the back belt only.

The results revealed a significant decrease in lost time (2.5 days) in the group receiving training plus back belts ( $p=0.03$ ). In the sub-group analysis, the authors suggest the reduction in lost time seen in the group receiving training plus back belts was limited to workers with previous LBP ( $p=0.02$ ).

The main weaknesses of this study include the lack of adjustment for the apparent baseline differences in days lost among the groups and the failure to assess group similarity with respect to history of LBP at baseline.

Reddell *et al.* studied 642 out of the initial 896 selected airline baggage handlers who were randomly assigned to four groups: back belt only; one hour training class (on proper lifting) only; back belt and one hour training class; and no intervention.<sup>26</sup> There were no significant differences

in injury rates, lost workdays or WCB costs among the groups. However the results are inconclusive because: patients were analysed according to use of back belts, and not on group assignment; there was very high non-compliance (58% discontinued back belt use) and high non-participation rate (28%).

Alexander *et al.* randomly assigned 60 health care workers to either wear a belt or be in a control group and then followed them for three months.<sup>29</sup> There were only three self-reported low back injuries at follow-up, one in the belted group and two in the control. The difference was not statistically significant ( $p=0.53$ ). The small number of injuries in this study suggests the sample size may have been too small and/or follow-up period too short to detect any significant difference in injury rates between the two groups.

In another RCT, van Poppel *et al.* randomly assigned a total of 315 airline cargo workers (within pre-existing work groups) to one of four groups: education and lumbar support; education only; lumbar support only; or no intervention.<sup>27</sup> After six months, there was no significant difference in self report low back pain incidence (risk difference 1%, 95% CI, -10 to 13) or sick leave for LBP (risk difference 4%, 95% CI, -3 to 11) among workers in the groups assigned to wear lumbar supports compared to those who were not. In a small subgroup of workers with LBP at baseline (15%), the group with lumbar supports had fewer days with LBP per month than did the group without (mean of 3.1 vs. 8.4 days,  $p=0.03$ ). A main limitation in the study was the randomisation process, where assignment was made by workgroups instead of the individual worker, which may have potentially introduced confounding if there were undetected systematic differences between the work groups. There was also potential recall bias and selection bias due to high non-compliance.

In the most recent RCT, Kraus *et al.* randomised nine agencies, employing 12,772 home attendants, into three groups: back belts only, lifting advice only, and control.<sup>25</sup> Over a 28 month period the authors reported a marginally significant lower back injury rate among the back belt group when compared to the control group (rate ratio 1.36, 95% CI, 1.02 to 1.82). There was no significant difference when the back belt group was compared to the advice only group (rate ratio 1.22, 95% CI, 0.70 to 2.11). The main limitation to this study is the inability to adjust for cluster randomization and known risk factors when comparing the back belt and control groups since the control group data was based on crude (unadjusted) injury data only (due to a lack of baseline data). Therefore, it is not certain if the difference observed was due to confounding. Other weaknesses include no comparison of pre-intervention injury rates and no data on previous LBP.

In two non-randomised trials; Anderson *et al.*<sup>30</sup> found a reduction in back injury rates and Thompson *et al.*<sup>31</sup> found a decrease in back pain respectively among workers who wore a back support compared to those who did not.

The positive results found in the two non-randomised trials are questionable since the groups being compared were not assessed for factors associated with increased risk of LBP. Known and unknown confounding factors within work sites may have influenced the results.

### *Observational Studies*

In a retrospective survey among workers at an air force base Mitchell *et al.* reported a marginally significant protective effect with back belt use (OR 0.60, 95% CI, 0.36 - 1.00) for the first low back injury.<sup>34</sup> The main limitations in this study include the lack of randomization, back belt users and non-users were not matched in time and the use of self report data on back belt use and injuries.

In a historical cohort study, Kraus *et al.* compared the low back injury rates of 36,000 retail workers during pre and post mandatory back support use policy periods implemented over a six-year period.<sup>32</sup> The results indicated an incidence density rate ratio (ratio of the number of low back injury claims per million working hours before and after back support policy implementation) of 1.52 (95% CI, 1.36 - 1.69). The authors reported that 34% of injuries could have been prevented if all subjects had worn a back support.

The conclusions of this study must be interpreted with caution. Because the comparison groups were not matched in time, it was not possible to control for unknown confounders such as a change in hiring practices, job duties, claims handling, safety regulations and workers' compensation claims policies. In addition, the gradual increase in the use of forklifts and pallets during the six-year study could have been an important co-intervention contributing to the significant decrease in the IDR.

The largest prospective study on back belt use was conducted by Wassel and colleagues who compared the incidence of low back injuries and self-report low back pain among a sample of 13,873 material handlers employed at either one of 89 retail stores who had a mandatory back belt policy or one of 71 stores that had a voluntary policy.<sup>33</sup> When controlling for potential confounding factors the authors found no difference in back injury claims (OR 1.22, 95% CI, 0.87 to 1.70) or self-report episodes of LBP (OR 0.97, 95% CI, 0.83 to 1.13) among the stores with mandatory or voluntary back belt use policy.

The main limitations of this study were the high non-participation rate (32%) and the high number of workers who did not complete follow-up interviews (33%). Lack of randomisation with the inability to control for unknown confounders, and recall bias from self-report interviews may have been other potential sources of bias.

### **Potential Harms and Costs**

Potential negative effects of back belt use were discussed in most studies that were reviewed. In one trial 20% of workers felt that the belt rubbed, pinched or bruised their ribs; 15% stated the belt caused problems during sitting or driving; and 20% said that it was too hot or caused excessive sweating.<sup>26</sup> Reduced movement, uncomfortable sitting and excessive heat were complaints expressed by workers in another study.<sup>27</sup> The negative comments may have contributed to the high non-compliance rate for back belt use in these two studies.<sup>26,27</sup> *Post hoc* analysis of non-compliant back belt users in one study<sup>26</sup> indicated a significantly higher number of lost workdays ( $p < 0.0181$ ) when compared to compliant back belt or control groups. However, a similar finding was not observed in the compliance subgroup analysis from another study.<sup>27</sup> Two studies evaluating abdominal strength change found no significant loss of abdominal strength among back belt users.<sup>27,28</sup>

The United States National Institute for Occupational Safety and Health, (NIOSH) has suggested that a false sense of security may accompany back belt use which may lead to increased risk taking behaviours (such as excessive lifting).<sup>35</sup> Other potential risks include cardiovascular strain,<sup>36,37</sup> back muscle weakening<sup>38</sup> and abdominal hernia.<sup>39</sup> However, these possible risks have been extrapolated from studies evaluating various physiological parameters such as intra-abdominal pressure, EMG, heart rate, and blood pressure, and we were unable to find any epidemiological evidence to support these possible adverse effects of back belt use.

## **INTERPRETATION**

### **Summary of Key Evidence**

Three out of five randomised controlled trials reviewed failed to show positive results with back belt use.<sup>26,27,29</sup> One RCT showed decreased time loss in workers who received both training and used a back belt.<sup>28</sup> A review of their subgroup analysis suggests this effect is seen only among workers with a previous history of LBP. Another RCT reported a marginally significant decrease in low back injury rates among employees receiving a back belt compared to the control group.<sup>25</sup>

However, this analysis was based on unadjusted data not controlling potential confounders. All RCTs reviewed suffered from methodological flaws, some of which, such as lack of blinding, are inherent to workplace studies and the type of intervention used, while others, like inappropriate randomisation, lack of intention to treat analysis and inadequate follow-up times are related more to the study designs. *A priori* sample size calculation for lost time was conducted in only one RCT.<sup>27</sup> Based on this calculation, only two RCTs appeared to have had a sufficient sample size.<sup>26,27</sup> Only one RCT performed a sample size calculation for the incidence of low back injuries.<sup>25</sup> This RCT had an 80% power to detect a 30% or greater decline in the low back injury rate. No sample size calculations were performed for assessing change in the incidence of LBP in any of the reviewed RCTs. Sample size estimates for this outcome, based on mean incidence values found in one trial,<sup>27</sup> suggests only two trials<sup>26,27</sup> had sufficient sample size to detect a reduction in the incidence rate of less than 70% (using a power of 80%).

None of the RCTs reviewed were considered to be of “good” quality. Four were considered of “fair” quality<sup>25,27-29</sup> and one<sup>26</sup> was considered “poor”.

The only study with a “good” quality rating was the recent large cohort.<sup>33</sup> This study failed to show any benefit to back belt use for the main outcomes or in the subgroup analysis. Lost time in this study was not assessed.

The remaining studies found positive results with back belt use.<sup>30-32,34</sup> However, these studies were found to have significant weaknesses in both methods and analysis, as reflected in their “poor” quality scores.

The studies reviewed used diverse styles of back belts. Since no one style produced beneficial results it is unlikely that any subtle differences due to design were a factor. The lack of consistent conclusions from the studies included for this review are not surprising given the conflicting laboratory evidence on how back belts are thought to prevent LBP.<sup>22,39-44</sup>

Controversy over back belt use also extends into the area of treatment, where results from randomised controlled trials are also conflicting.<sup>3,45</sup> However, the positive results demonstrated in two randomised controlled trials<sup>46,47</sup> and in the subgroup analysis in two trials reviewed here<sup>27,28</sup> suggest the need for further study focused specifically on this important subgroup.

In summary, evidence for the effectiveness of back belt use in preventing the incidence or reducing lost time (absenteeism) for occupational LBP among material handlers is limited in both quantity and quality. Conclusions from studies reporting adverse effects were limited by the

methods used while potential risks from back belt use, theorized from inconclusive laboratory data, have not been proven in epidemiological studies.

### **Canadian Task Force Recommendations (Table 4)**

To date, evidence for the effectiveness of back belt use in preventing the incidence or reducing lost time (absenteeism) for occupational LBP among material handlers is limited in both quantity and quality. Conclusions from studies reporting adverse effects were limited by the methods used while potential risks from back belt use, theorized from inconclusive laboratory data, have not been proven in epidemiological studies. Therefore, the CTFPHC concludes that the existing evidence is conflicting and does not allow making a recommendation for or against the use of back belts to prevent occupational low back pain or to reduce lost work time due to LBP (**C Recommendation**).

### **Recommendations of Others**

NIOSH, following the results of their most recent back belt study, concluded there was no evidence to support the use of back belts as a preventive measure.<sup>33</sup> This further supports their previous review and recommendations on back belt use.<sup>40</sup>

The Canadian Centre for Occupational Health and Safety's (CCOHS) publication on back belt use,<sup>48</sup> refers to the NIOSH review<sup>40</sup> and recommends that back belts should not be used as a primary workplace prevention approach.

In contrast, OSHA's recent ergonomics regulation classified lumbar supports as personal protective equipment and suggests they may prevent back injuries in certain industrial settings.<sup>49</sup>

In two recent literature reviews of back belt clinical trials, one concluded there was moderate evidence<sup>3</sup> and the other strong and consistent evidence<sup>50</sup> that lumbar supports are not effective for primary prevention. In one of these reviews<sup>3</sup> only two of the seven trials included<sup>27,28</sup> were considered to be of high quality. Our review was not concordant with these two high quality designations. Both these trials were downgraded to "fair" quality, one<sup>27</sup> due to inappropriate randomisation (ie. work-group level only) and high non-compliance and the other<sup>28</sup> because of the lack of comparability of the groups at baseline.

Earlier reviews on the topic concluded there was insufficient evidence to make recommendation for or against the use of back belts for the prevention of occupational low back injuries.<sup>22,42,51,52</sup>

## **Implications for Clinical Practice**

In general, the majority of the evidence presented in our review, and the evidence presented in earlier reviews of the topic, indicates that individual workers presenting with no prior history of low back pain are unlikely to benefit from the use of a back belt. Those with a previous history of LBP may experience some potential benefit from back belt use. However, prior to back belt prescription, individuals should be screened for cardiovascular risk and receive training on lifting mechanics.<sup>37,53</sup> Although there is some laboratory evidence suggesting possible concern for the adverse effects of long-term use,<sup>53</sup> these possible risks have not been proven. However, given the combination of questionable benefits and potential for negative effects, if back belts are to be prescribed, it should only be for short-term use.<sup>53</sup>

## **Research Agenda**

The overall level and quality of this evidence on the topic remains limited and conflicting. Well-conducted randomised controlled trials into the efficacy of back belts are still needed. While it is recognized that rigorous workplace effectiveness trials are logistically very difficult to conduct, it is recommended that future studies should include: a large ‘at risk’ population; individual worker randomisation; appropriate control groups; long follow-up; high compliance rate; and the use of validated outcome measures, including a special focus on those with a prior LBP history.

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**Table 1. Main Methodologic Quality Criteria & Levels of Evidence**

<b>Internal Validity Criteria</b>	
1.	Was there appropriate assembly and maintenance of comparable groups?
2.	Was there adequate follow-up?
3.	Were interventions clearly defined?
4.	Were equal, reliable and valid outcome measures used?
5.	Were the analyses/ sample size appropriate and was intention to treat analysis used?
<b>Levels of evidence</b>	
I	Evidence from at least one well-designed randomized controlled trial
II-1	Evidence from well-designed controlled trials without randomization
II-2	Evidence from well-designed cohort or case-control analytic studies, preferably from more than one centre or research group
II-3	Evidence from comparisons between times and places with or without the intervention; dramatic results from uncontrolled studies could also be included here.
III	Opinions of respected authorities, based on clinical experience; descriptive studies or reports of expert committees

**Table 2. Methodologic Quality of Studies on the effectiveness of Back Belts**

<b>Study</b>	<b>Internal Validity Criterion*</b>					<b>Study design</b>	<b>Overall rating</b>
	<b>1</b>	<b>2</b>	<b>3</b>	<b>4</b>	<b>5</b>		
Walsh, 1990 <sup>28</sup>	-	+	?	+	?	I	fair
Reddell, 1992 <sup>26</sup>	+	+	+	-	-	I	poor
Alexander, 1995 <sup>29</sup>	+	-	+	?	?	I	fair
Van Poppel, 1998 <sup>27</sup>	?	+	+	?	+	I	fair
Kraus, 2002 <sup>25</sup>	?	+	?	?	+	I	fair
Anderson, 1993 <sup>30</sup>	-	+	+	+	?	I-2	poor
Thompson, 1994 <sup>31</sup>	-	-	+	?	?	I-2	poor
Wassel, 2000 <sup>33</sup>	?	+	+	?	+	II-2	good
Kraus, 1996 <sup>32</sup>	-	+	+	-	?	II-3	poor
Mitchell, 1994 <sup>34</sup>	-	+	+	-	?	III	poor

\* + - criterion met; - = criterion not met; ? = not reported or unclear

**Table 3. The Effect of Back Belt Use on the Incidence of Occupational Low Back Pain**

<b>Study (Design)</b>	<b>Target Population</b>	<b>Intervention / Exposure</b>	<b>Follow Up</b>	<b>Outcomes/Analysis (outcomes of interest in bold)</b>	<b>Results (includes confidence intervals and p values where given) (results of interest in bold)</b>
Walsh, 1990 <sup>28</sup> (RCT)	90 male warehouse workers in grocery distribution centre San Antonio Texas	Gr. 1 - no intervention (n=30) Gr. 2 - 1-hour training (n=30) Gr. 3 - 1-hour training and back belt (n=30)	6 months	work injury incidence	no significant change (no injury data given)
				lost time	less lost time (2.5 days) in Gr. 3 (p=0.03)
				knowledge	improved knowledge Gr. 2 (p=0.001) & Gr.3 (p=0.003)
				abdominal strength	no significant difference
				productivity	no significant difference
				health care utilization	no data given
Reddell, 1992 <sup>26</sup> (RCT)	642 baggage handlers airline industry; southern USA	Gr. 1 -weightlifting belt (n= 147) Gr. 2 -Belt and training (n= 127) Gr.3 - training only (n= 122) Gr.4 - no intervention (n=248)	8 months	injury rate lost workday case restricted workday WCB costs satisfaction questionnaire	no significant difference in all outcomes
Alexander, 1995 <sup>29</sup> (RCT)	60 hospital workers	1. back belts (n=30) 2. control (n=30)	3 months	incidence of low back injury	1 injury difference-not significant (p=0.533)
				perceived change in back pain	no significant change (p=0.981)
Van Poppel, 1998 <sup>27</sup> (RCT)	312 airline cargo workers in the Netherlands	1. education and lumbar support (n=70) 2. education (n=82) 3. lumbar support (n=83) 4. control (n=77)	6 months		no statistically significant change in outcomes
				<b>Incidence of LBP</b> <b>Number of days/ month with LBP</b>	risk difference = 1%, 95% CI (-10 to 13) difference in means = 0.4 (p= 0.92)
				<b>Workers on sick leave for LBP</b> <b>Number of days of sick leave per month due to LBP</b>	risk difference = 4%, 95% CI (-3 to 11) difference in means = 0, (p=0.52)

<b>Study (Design)</b>	<b>Target Population</b>	<b>Intervention / Exposure</b>	<b>Follow Up</b>	<b>Outcomes/Analysis (outcomes of interest in bold)</b>	<b>Results (includes confidence intervals and p values where given) (results of interest in bold)</b>
Kraus, 2002 <sup>25</sup> (RCT)	12,772 home attendants from 9 agencies USA	1. back belt (n=3837) 2. advice only (n=4300) 3. control (n=4635)	28 months	Low back injury rate ratios per 100 full time equivalents with back belt group as referent.	Advice only, 1.22 (adjusted), 95% CI, (0.70-2.11) Control, 1.36 (unadjusted), 95% CI (1.02-1.82)
Anderson, 1993 <sup>30</sup> (CCT)	266 warehouse workers	1. Belted workers (1 site) 2. Control (2 sites)	12 months	pre and post incidence of low back injuries per 200,000 hrs worked.	statistically significant decrease (30%) in incidence of low back injuries at back belt use site (p=0.02)
Thompson, 1994 <sup>31</sup> (CCT)	60 hospital workers	1. lumbar support, back school and exercise (n=41) 2. back school and exercise (n= 19)	3 months	self-report incidence of low back pain -difference in employee attitudes	Statistically significant decrease in incidence of low back pain (p< 0.05) - improvement in attitude in lumbar support, back school and exercise group (p< 0.05)
Kraus, 1996 <sup>32</sup> (Historical cohort)	36,000 Retail-trade home improvement industry (The Home Depot) California	employee work hours classified as 1. Pre back support use policy period (12,812,000 hours) 2. Post back support policy period (87,078,000 hours)	5 years 1989-1994	acute low back injury claims-incidence density rates per million work hours. Pre and post policy period	pre- 30.6 per mwhrs, post- 20.2 per mwhrs
				incidence density rate ratio (IDRR) without and with back support	significant decrease in IDRR. IDRR 1.52 95% CI (1.36 - 1.69)
				prevented fraction	prevented fraction 34%
Mitchell, 1994 <sup>34</sup> (Retrospective survey)	1316 Air Force Base workers Mid west USA	- belt use - no belt use	6 years 1985-1991	injury rate with and without belts	OR 0.60, 95% CI (.36 -1.00) p=0.0508
				injury rate with previous history of low back injury	OR 5.56, 95% CI (3.35 - 9.26) p=0.0001
				injury rate with kg. lift per day	OR 1.01, 95% CI (1.01 - 1.02) p= 0.0005
				lost time or limited duty days	no significant difference (p=0.92 and p=0.58)
				cost effectiveness belt- no belt	belts not cost effective
Wassell, 2000 <sup>33</sup> (Prospective cohort)	13,873 workers Retail-supermarket-	-voluntary back belt use policy (71 stores) -mandatory back belt use policy (89 stores)	3 years 1996-1998		no statistically difference in outcomes
				back injury claims with and without belts	OR 1.22, 95% CI (0.87 - 1.70)

<b>Study (Design)</b>	<b>Target Population</b>	<b>Intervention / Exposure</b>	<b>Follow Up</b>	<b>Outcomes/Analysis (outcomes of interest in bold)</b>	<b>Results (includes confidence intervals and p values where given) (results of interest in bold)</b>
	merchandise stores 30 US states			6 month self-report episodes of low back pain	OR 0.97, 95% CI (0.83 - 1.13)

**Table 4. Summary of Recommendations: Use of Back Belts to Prevent Occupational Low Back Pain.**

MANOEUVER	EFFECTIVENESS	LEVEL OF EVIDENCE <ref>	RECOMMENDATIONS*
Use of back belts in the workplace to prevent the incidence of occupational low back pain, or time lost from work due to LBP	There is conflicting evidence that back belt use reduces <u>incidence</u> of or <u>lost work time</u> due to low back pain.	Walsh (Level I, fair): negative** for incidence, positive** for lost time van Poppel (Level I, fair): negative for both incidence and lost time Alexander (Level I, fair): negative for incidence Kraus (Level I, fair): positive for incidence Wassell (Level II-2, good): negative for incidence	The CTF concludes that the existing evidence is conflicting and does not allow making a recommendation for or against the use of back belts to prevent occupational low back pain or to reduce lost work time due to LBP. <b>(C Recommendation).</b>

\* See Appendix 1 for definitions of the levels of evidence, quality ratings, and grades of recommendations

\*\* positive = significant differences benefiting the back belt group; negative = no significant differences between groups

<b>Appendix 1: Methodology of the Canadian Task Force on Preventive Health Care</b>	
<p><i>Critical appraisal</i></p> <p>The Task Force reviewed 1) the initial analytic framework and key questions for the proposed review; 2) the subsequent draft(s) of the complete manuscript providing critical appraisal of the evidence prepared by the lead author(s), including identification and critical appraisal of key studies, and ratings of the quality of this evidence using the task force's established methodological hierarchy (sidebar); and 3) a summary of the evidence and proposed recommendations.</p> <p><i>Development</i></p> <p>Evidence for this topic was presented by the lead author(s) and deliberated upon during 4 task force meetings. Expert panelists addressed critical issues, clarified ambiguous concepts and analyzed the synthesis of the evidence. At the end of this process, the specific clinical recommendations proposed by the lead author were discussed, as were issues related to clarification of the recommendations for clinical application and any gaps in evidence. The results of this process are reflected in the description of the decision criteria presented with the specific recommendations. The group and lead author(s) arrived at final decisions on recommendations unanimously.</p> <p>Subsequent to the meetings, the lead author revised the manuscript accordingly. After final revision, the manuscript was sent by the Task Force to 2 experts in the field (identified by Task Force members at the meeting). Feedback from these experts was incorporated into a subsequent draft of the manuscript.</p> <p>Efforts to achieve adequate documentation, transparency, comprehensiveness, objectivity and adherence to the task force methodology were maintained throughout the review development, the consensus process and beyond to ensure uniformity and impartiality in the final report.</p>	<p><b>Levels of evidence</b></p> <p><i>A. Research design rating:</i></p> <p><b>I</b> Evidence from randomized controlled trial(s)</p> <p><b>II-1</b> Evidence from controlled trial(s) without randomization</p> <p><b>II-2</b> Evidence from cohort or case-control analytic studies, preferably from more than one centre or research group</p> <p><b>II-3</b> Evidence from comparisons between times or places with or without the intervention; dramatic results from uncontrolled studies could be included here</p> <p><b>III</b> Opinions of respected authorities, based on clinical experience; descriptive studies or reports of expert committees</p>
	<p><i>B. Quality (internal validity) rating (see Harris et al., 2001):</i></p> <p><b>Good</b> A study that meets all design- specific criteria* well.</p> <p><b>Fair</b> A study that does not meet (or it is not clear that it meets) at least one design-specific criterion* but has no known “fatal flaw”.</p> <p><b>Poor</b> A study that has at least one design-specific* “fatal flaw”, or an accumulation of lesser flaws to the extent that the results of the study are not deemed able to inform recommendations.</p>
	<p>*General design specific criteria are outlined in Harris et al., 2001. Inclusion/exclusion criteria are detailed in the Methods section.</p>
	<p><b>Recommendations Grades for Specific Clinical Preventive Actions</b></p> <p><b>A</b> The CTF concludes that there is <b>good</b> evidence to recommend the clinical preventive action.</p> <p><b>B</b> The CTF concludes that there is <b>fair</b> evidence to recommend the clinical preventive action.</p> <p><b>C</b> The CTF concludes that the existing evidence is <b>conflicting</b> and does not allow making a recommendation for or against use of the clinical preventive action, however other factors may influence decision-making.</p> <p><b>D</b> The CTF concludes that there is <b>fair</b> evidence to recommend against the clinical preventive action.</p> <p><b>E</b> The CTF concludes that there is <b>good</b> evidence to recommend against the clinical preventive action.</p> <p><b>I</b> The CTF concludes that there is <b>insufficient</b> evidence (in quantity and/or quality) to make a recommendation, however other factors may influence decision-making.</p>
	<p><i>The CTF recognizes that in many cases patient specific factors need to be considered and discussed, such as the value the patient places on the clinical preventive action; its possible positive and negative outcomes; and the context and/or personal circumstances of the patient (medical and other). In certain circumstances where the evidence is complex, conflicting or insufficient, a more detailed discussion may be required.</i></p>