Health benefits of increased walking for sedentary, generally healthy older adults: using longitudinal data to approximate an intervention trial

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Health Benefits of Increased Walking for Sedentary, Generally Healthy Older Adults: Using Longitudinal Data to Approximate an Intervention Trial

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Background. Older adults are often advised to walk more, but randomized trials have not conclusively established the benefits of walking in this age group. Typical analyses based on observational data may have biased results. Here, we propose a "limited-bias," more interpretable estimate of the health benefits to sedentary healthy older adults of walking more, using longitudinal data from the Cardiovascular Health Study.

Methods. The number of city blocks walked per week, collected annually, was classified as sedentary (<7 blocks per week), somewhat active, or active (≥28). Analysis was restricted to persons sedentary and healthy in the first 2 years. In Year 3, some became more active (the treatment groups). Self-rated health at Year 5 (follow-up) was regressed on walking at Year 3, with additional covariates from Year 2, when all were sedentary.

Results. At follow-up, 83.5% of those active at baseline had excellent, very good, or good self-rated health, as compared with 63.9% of the sedentary, an apparent benefit of 19.6 percentage points. After covariate adjustment, the limited-bias estimate of the benefit was 11.2 percentage points (95% confidence interval 3.7–18.6). Ten different outcome measures showed a benefit, ranging from 5 to 11 percentage points. Estimates from other study designs were smaller, less interpretable, and potentially more biased.

Conclusions. In longitudinal studies where walking and health are ascertained at every wave, limited-bias estimates can provide better estimates of the benefits of walking. A surprisingly small increase in walking was associated with meaningful health benefits.

Key Words: Exercise—Walking—Selection bias—Self-rated health—ADL.

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Physical activity is recommended for persons of all ages (1). Because walking is the most commonly reported leisure-time physical activity in the United States and is relatively common among the elderly adults (2,3), older adults are often advised to walk more. But the few long-term randomized trials of the health benefits of walking for older adults have had equivocal results (4), and the recommendations to walk more have been based primarily on results from observational studies. Such studies have found a strong correlation between the amount walked and survival and incident cardiovascular disease (CVD) in older men (5,6) and an inverse association between walking intensity and cardiovascular events in older women (7).

Among adult diabetics, walking at least 2 hours per week was correlated with lower all-cause mortality (39%) and cardiovascular mortality (34%) over 8 years of follow-up (8). However, when isolated from overall physical activity, walking showed no significant association with either mortality or CVD in the Shanghai Women’s Health Study (9). As discussed below, the health benefits of walking estimated from observational data may be biased. We propose to use longitudinal data to make the analysis of observational data more like the analysis of a randomized trial.

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Suppose the goal is to estimate the benefit of increased walking for healthy sedentary persons. Ideally, a randomized controlled trial (RCT) would be conducted. The RCT might randomize persons who had been healthy and sedentary (according to the trial’s eligibility criteria) for some amount of time to one of three walking groups: sedentary, somewhat active, or active. The health of the three groups might be compared 2 years later. Differences among the groups would be unequivocal estimates of the benefit of the walking program for healthy sedentary persons. Health at randomization would be unassociated with treatment group because of randomization. The analysis might control for treatment group because of randomization. The analysis might control for measures of health status at baseline anyway to adjust for...
any small imbalances among the groups and perhaps increase power. If the group of interest included older adults, the health outcome measure should account for death (10). Note that the RCT design has four important features: (a) it addresses the question of interest (the effect of walking on the health of sedentary persons), (b) the time when the walking program started is known, (c) the health at the start was equivalent in the three groups because of randomization, and (d) any covariates used in the model were measured before anyone started to exercise and so cannot have been in the causal pathway between exercise and health.

Most often, however, the benefits of walking have been estimated from observational cohort studies that measured physical activity only once, at baseline, and followed participants prospectively for health events (here called the “typical design”) (5–9,11–14). A typical analysis might classify participants’ walking level at the study’s baseline and examine their health 2 years later, controlling by regression for baseline differences in the health of the groups. Note that this analysis has none of the features of the RCT just mentioned: (a) it does not effectively address the question of interest because analysis is not restricted to healthy sedentary older adults, (b) the time when persons started to exercise is unknown and may have been long before baseline, (c) persons’ health when they started to exercise is unknown and cannot be adjusted for, and (d) all the covariates for the treatment group were measured after exercise was already initiated and so may be in the causal pathway between exercise and health. (Controlling for health variables that have already been improved by exercise is likely to bias the effects toward the null.)

A different analysis is possible in cohort studies exemplified by the Cardiovascular Health Study (CHS; described below), which measured physical activity every year and health status every 6 months. This limited-bias design, diagrammed in Figure 1, mimics an RCT by restricting analysis to persons who were healthy and sedentary in the first 2 years of data collection. By Year 3, referred to here as the analytic baseline, some persons will have increased their walking level, which may have affected their health in Year 5. Health at Year 5, as defined by the study, would be regressed on walking level in Year 3. Health variables measured at Year 2 (when all were sedentary) and perhaps at Year 2.5 are used as covariates.

This design has several features in common with the RCT: (a) it does address the question of interest because it is restricted to previously sedentary and healthy persons, (b) the time when exercise commenced is known to be sometime between Years 2 and 3, (c) health status when exercise started is known to within 6 months and so can be controlled for approximately, and (d) all covariates but one (health at Year 2.5) were measured at Year 2, before any exercise started, and therefore are not in the causal pathway. The Year 2.5 health measure was included as a “compromise covariate.” For persons whose health changed between Years 2 and 2.5, who then started to exercise, and who received benefits of exercise after Year 2.5, this analysis is appropriate because it controls for health at Year 2.5. The design may overcontrol for variables in the causal pathway for any persons who both started to exercise and then changed their health between Years 2 and 2.5. The design may undercontrol for health at the start of exercise for persons who both changed their health and then started to exercise between Years 2.5 and 3.0.

Because this design is more similar to the RCT than is the typical design and because the possibilities of over- and
undercorrection are limited to the probably small number of persons who had all their exercise and health changes within a 6-month period, we refer to it as the “limited-bias” design. In this study, we implemented the limited-bias design to estimate the health benefit of moving from sedentary to active for healthy older adults. Ten different definitions of health were used, and all health measures were coded to account for death. Results were compared with those of the “typical” analysis.

Methods

Data

Data came from the CHS, a population-based longitudinal study of risk factors for heart disease and stroke in 5,888 adults aged 65 years and older at baseline (15). Participants were recruited from a random sample of Medicare eligibles in four U.S. communities, and extensive data were collected during annual clinic visits and telephone calls. The original cohort of 5,201 participants, recruited in about 1990, had up to 10 annual clinic examinations. A second cohort of 687 African Americans, enrolled in about 1993, had up to seven annual examinations. Limited data were collected by telephone 6 months after each clinic visit. Follow-up was virtually complete for surviving participants (16).

The reported number of “city blocks or the equivalent” walked outside the home in the previous week was collected in 1990 and annually from 1992 to 1999. Walking is a major component of physical activity as measured in the Minnesota Leisure Time Activities (MLTA) questionnaire (17). In 1990, the number of blocks walked was moderately correlated (r = .45) with the MLTA (excluding chores, on the log scale), suggesting that the reported number of blocks walked has both face and construct validity as a measure of physical activity. The blocks data were coded into approximate tertiles as sedentary (less than 7 blocks per week), somewhat active (7–27 blocks), and active (28 or more blocks per week). This definition of sedentary is consistent with that used in other studies of older adults (14). Because the number of city blocks per mile is not standard (often from 10 to 20 blocks per mile), we must assume that persons’ definition of a city block was the same, on average, in the three activity groups.

The primary outcome, measured every 6 months, was self-rated health: is your health excellent, very good, good, fair, or poor? This well-recognized predictor of health, function, and survival (18) was dichotomized to indicate whether the person was healthy (excellent, very good, or good) or not healthy (fair, poor, or dead). This outcome measure thus accounts for people who died and the analysis can be truly prospective.

To increase the generalizability of the findings, we considered nine additional health outcomes and definitions of being “healthy.” All variables were dichotomized to healthy/not healthy. Definitions of healthy included having a Modified Mini-Mental State Examination score ≥90 (19); no difficulties with activities of daily living (ADL)—walking, transferring, eating, dressing, bathing, or toiletting; no difficulties with instrumental activities of daily living (IADL)—heavy or light housework, shopping, meal preparation, money management, or telephoning; a Center for Epidemiologic Studies Short Depression score <10 (20,21); measured gait >0.8 m/s; never had angina, coronary heart disease, congestive heart failure, claudication, myocardial infarction, stroke, transient ischemic attack, angioplasty, or coronary artery bypass surgery (CVD); no days in bed in the previous 2 weeks; not hospitalized in the previous year; and being alive. Data missing between a person’s first and last observed measures (about 5%) were imputed from a person-specific regression of the variable on the log of time from the last known measure.

The 10 health-related variables were used in several ways. The sample was restricted to persons who were sedentary and healthy at Years 1 and 2. (Healthy is a binary variable that was defined in several different ways, as described later.) The Year 5 value of healthy was the outcome variable, and the Year 2 values of all 10 health variables were used as covariates in the regressions. We also created a combined variable denoting having neither ADL nor IADL difficulties.

To implement the design in Figure 1, we sorted through each person’s data to locate (at most) one period of five consecutive years in which the person was healthy and sedentary in the first 2 years, the number of blocks walked was known at Year 3 (referred to henceforth as the analytic “baseline”), and the health state (healthy, sick, or dead) was known at Year 5 (referred to henceforth as “follow-up”). The dependent variable is a binary variable indicating whether the person was healthy at follow-up (Year 5). The primary analysis, based on self-rated health, was described above. For the other outcome variables, healthy was defined specific to that variable. For example, in the ADL analysis, “healthy” means having no ADL difficulties (coded 1) and “not healthy” represents either having ADL difficulties or being dead (coded 0). The sample was restricted to persons who were sedentary and had no ADL difficulties in Years 1 and 2.

Analysis

The limited-bias analysis was restricted to persons who were both sedentary and healthy in the 2 years before the analytic baseline (Year 3). The independent variable of interest was the walking level at baseline (sedentary, somewhat active, and active). The primary dependent variable (Y) indicated whether the person was healthy (not sick or dead) at follow-up. Y was regressed on dummy variables for walking level at baseline (Year 3) and on covariates measured at Years 2 and 2.5. We used ordinary least squares regression so that coefficients could be interpreted as the
(adjusted) difference in the percentage healthy relative to those who remained sedentary. This is also known as the risk difference. Using least squares when the dependent variable is binary is appropriate for large samples when the proportion healthy is not close to 0 or 1 (22). We used the continuous (not the binary) version of the variables at Year 2 as regression covariates, to control as well as possible for preexisting differences among the groups.

For comparison, we performed additional analyses that did control for health-related variables measured at the analytic baseline, that did not require persons to be sedentary or healthy in the previous 2 years, or that did not include death in the outcome. We expected that analyses that adjusted for baseline health or that analyzed only survivors would underestimate the effect of walking on health. Because randomized trials often have different outcome measures and selection criteria than self-rated health, we conducted 10 additional limited-bias analyses, substituting each of the other health measures in turn for self-rated health. For example, in one analysis we restricted analysis to persons with no ADL difficulties in Years 1 and 2, and the outcome at Year 5 was whether the person was free of ADL difficulties (healthy, coded as 1) versus having ADL difficulties or having died (coded as 0).

## FINDINGS

### Descriptive Longitudinal Analysis

For the primary analysis, of self-rated health, we identified a subset of 1,409 CHS participants who were sedentary and healthy in Years 1 and 2. Table 1 shows descriptive statistics for all variables in Years 1 through 5 as a function of the walking category at Year 3 (the analytic baseline). (The “somewhat active” group was omitted from the table, to save space, but is described online (23).) We next point out a few features of the data to help familiarize the reader with this complex table.

Columns are labeled by the year of the data and by the treatment group (exercise level at Year 3). At Years 1 and 2, all were sedentary, by design (line 2). By Year 3 (the analytic baseline), 829 persons remain sedentary, 422 were somewhat active (not shown), and 158 persons had become active. Line 3 shows the median number of blocks walked in each period. In the active category, this dropped from 48 at baseline to 10 at follow-up but was still higher than in the other groups. In Years 1 and 2, 100% were healthy, by design (line 4). At baseline, those percentages had dropped to 82.3% and 92.4% for the two walking groups. That is, at baseline the active group was already healthier than the sedentary. There is a similar relationship at follow-up, where the unadjusted effect of becoming active was 83.5 − 63.9 = 19.6 percentage points. Line 5 in the table shows the percentage who were healthy at Year 2.5, 6 months before baseline, which was also higher for the active. The percentage alive at follow-up (line 6) was also higher for the active. The sedentary group was slightly older and included fewer blacks and men than the active (lines 7–9).

Lines 10–17 show the additional health-related variables. At all years, for all health measures, persons active in Year 3 were healthier than those sedentary, and the two groups were significantly different on every variable but gender and race (results not shown). The early differences among

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### Table 1. Walking, Health and Demographic Factors Over 5 years for the Limited-Bias Analysis (N = 1409 – 422 somewhat active not shown)

<table>
<thead>
<tr>
<th>Study Year</th>
<th>1</th>
<th>2</th>
<th>3 (baseline)</th>
<th>4</th>
<th>5 (follow-up)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 Number of cases</td>
<td>829</td>
<td>158</td>
<td>829</td>
<td>158</td>
<td>829</td>
</tr>
<tr>
<td>2 Walking level this year*</td>
<td>Sed</td>
<td>Sed</td>
<td>Sed</td>
<td>Sed</td>
<td>Sed</td>
</tr>
<tr>
<td>3 Blocks (median)†</td>
<td>2.0</td>
<td>2.0</td>
<td>1.0</td>
<td>3.0</td>
<td>1.0</td>
</tr>
<tr>
<td>4 Exc/VG/Good (%)‡</td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
<td>82.3</td>
</tr>
<tr>
<td>5 Exc/VG/Good Year 2.5 (%)</td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
<td>84.5</td>
</tr>
<tr>
<td>6 Alive (%)</td>
<td>77.3</td>
<td>74.9</td>
<td>78.3</td>
<td>75.9</td>
<td>79.3</td>
</tr>
<tr>
<td>7 Age (mean)</td>
<td>80.0</td>
<td>86.7</td>
<td>80.0</td>
<td>86.7</td>
<td>80.0</td>
</tr>
<tr>
<td>8 Male (%)</td>
<td>77.9</td>
<td>94.3</td>
<td>75.3</td>
<td>92.4</td>
<td>71.3</td>
</tr>
<tr>
<td>9 White (%)</td>
<td>80.0</td>
<td>93.7</td>
<td>80.0</td>
<td>93.7</td>
<td>71.3</td>
</tr>
<tr>
<td>10 No ADL difficulties (%)</td>
<td>65.4</td>
<td>79.7</td>
<td>59.7</td>
<td>80.4</td>
<td>59.1</td>
</tr>
<tr>
<td>11 No IADL difficulties (%)</td>
<td>95.8</td>
<td>98.1</td>
<td>93.6</td>
<td>98.7</td>
<td>93.0</td>
</tr>
<tr>
<td>12 CESD &lt; 16 (%)</td>
<td>82.5</td>
<td>93.0</td>
<td>81.2</td>
<td>89.9</td>
<td>79.4</td>
</tr>
<tr>
<td>13 3MS &gt; 80 (%)</td>
<td>95.7</td>
<td>96.2</td>
<td>95.4</td>
<td>95.6</td>
<td>94.6</td>
</tr>
<tr>
<td>14 No bed days (%)</td>
<td>52.7</td>
<td>66.5</td>
<td>54.8</td>
<td>74.7</td>
<td>54.4</td>
</tr>
<tr>
<td>15 Gait &gt; 0.8 m/s (%)</td>
<td>89.6</td>
<td>90.5</td>
<td>89.1</td>
<td>89.2</td>
<td>86.4</td>
</tr>
<tr>
<td>16 No hospital days (%)</td>
<td>73.8</td>
<td>84.8</td>
<td>71.3</td>
<td>82.9</td>
<td>68.5</td>
</tr>
<tr>
<td>17 No CVD (%)</td>
<td>80.0</td>
<td>86.7</td>
<td>80.0</td>
<td>86.7</td>
<td>80.0</td>
</tr>
</tbody>
</table>

**Notes:** ADL = activities of daily living; CESD = Center for Epidemiologic Studies Short Depression; CVD = cardiovascular disease; IADL = instrumental activities of daily living; 3MS = Modified Mini-Mental State Examination.

* Sedentary (Sed) is defined as walking less than seven blocks per week at Year 3; active (Act) is walking at least 28 blocks per week. The “somewhat active” category is not shown.
† Median blocks are calculated for living persons only. All other health-related variables code dead as zero.
‡ Healthy is excellent, very good, or good self-reported health (Exc/VG/Good); sick is fair or poor health, or (at follow-up) dead.
groups suggest the importance of controlling for health-related covariates at the start of exercise in the regression analysis.

**Regression Results for Self-rated Health**

To estimate the benefit of starting to walk more, healthy (dummy variable representing excellent, very good, or good self-rated health at follow-up) was regressed on dummy variables for somewhat active and active, controlling at different steps for the covariates in Table 1. Sedentary was the reference category. The regression coefficients for active (\( \times 100 \)) are shown in column 1 of Table 2. Step 1 shows that, with only the baseline walking level in the regression, the percent who were healthy at follow-up was 19.6 percentage points higher in the active than in the sedentary group, as was already calculated from Table 1 (83.5%–63.9%). The difference was statistically significant at the .01 level based on a two-tailed test.

Step 2 added age, sex, and race to the regression, which decreased the coefficient (the benefit of walking) slightly to 16.0 percentage points. Step 3 added the Year 2 values of the nine health-related variables (lines 4 and 10–17 in Table 1), which reduced the coefficient to 11.0. Step 4 added self-rated health at Year 2.5. Although this compromise covariate was highly significant (not shown), the regression coefficient for active did not change much at this step. After controlling for these variables, the active were 10.7 percentage points more likely than the sedentary to be healthy at follow-up (95% confidence interval [CI] 3.3–18.2 percentage points). This is the limited-bias estimate of the walking effect. For comparison, Step 5 added the baseline (Year 3) values of the health-related variables, which we have argued should not be included because they are likely in the causal pathway between walking and health. As expected, the coefficient became smaller and less statistically significant.

**Regression Results From Different Study Designs**

Table 2 also provides comparisons with alternative analytic approaches. Column 1 is the limited-bias analysis. For column 2, we removed the health constraints in the 2 years prior to baseline, requiring only that persons have been sedentary in both years. The available sample size nearly doubled. The regression coefficients at steps 1 and 2 were nearly twice as big as those in column 1. The limited-bias estimate (Step 4) was smaller than in column 1 probably because the sample included previously sick as well as previously healthy persons.

Column 3 shows results for an analysis that also removed the restrictions on prior walking. This again doubled the sample size and gave a somewhat larger effect at Step 4, which was highly significant due to the larger sample size and different population. However, the covariates included at Step 3 were probably affected by prior walking for persons who were not previously sedentary and are thus likely in the causal pathway. The interpretation is unclear. It cannot be assumed that the coefficient in line 4 is an estimate of the benefit for a sedentary person of starting to walk more.

The fourth column shows the results of the most typical analysis of physical activity in the literature, in which only the baseline health variables are available for adjustment. Steps 3 and 4 could not be performed because data prior to baseline were assumed unavailable. The effect at Step 5 (6.9 percentage points) was lower than the limited-bias estimate in column 1 (10.7) in part because the populations are quite different and in part because of control for variables that were in the causal pathway between walking and health. As noted above, this analysis does not address the question of interest.

**Regression Results for 11 Outcome Variables**

To expand these results beyond self-rated health, Table 3 shows the limited-bias estimates of the benefit of becoming...
Table 3. Limited-Bias Estimates of the Effect of Becoming Active on 11 Health Outcomes for Healthy, Sedentary Older Adults

<table>
<thead>
<tr>
<th>Definition of Healthy</th>
<th>Adjusted % Healthy&lt;sup&gt;1&lt;/sup&gt;</th>
<th>Effect Size&lt;sup&gt;2&lt;/sup&gt;</th>
<th>95% Confidence Interval</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Sedentary</td>
<td>Active</td>
<td>Difference</td>
</tr>
<tr>
<td>Exc/VG/Good</td>
<td>1,409</td>
<td>63.9</td>
<td>74.7</td>
</tr>
<tr>
<td>No hospitalizations</td>
<td>1,952</td>
<td>71.1</td>
<td>81.6</td>
</tr>
<tr>
<td>No bed days</td>
<td>2,235</td>
<td>74.3</td>
<td>84.2</td>
</tr>
<tr>
<td>Alive</td>
<td>2,539</td>
<td>78.1</td>
<td>87.4</td>
</tr>
<tr>
<td>3MS &gt; 80</td>
<td>1,928</td>
<td>72.6</td>
<td>80.9</td>
</tr>
<tr>
<td>No ADL difficulties</td>
<td>1,570</td>
<td>65.5</td>
<td>73.1</td>
</tr>
<tr>
<td>CESD &lt; 16</td>
<td>2,192</td>
<td>75.0</td>
<td>82.2</td>
</tr>
<tr>
<td>No IADL difficulties</td>
<td>1,069</td>
<td>63.5</td>
<td>70.6</td>
</tr>
<tr>
<td>No CVD</td>
<td>1,580</td>
<td>73.0</td>
<td>79.8</td>
</tr>
<tr>
<td>Gait &gt; 0.8 m/s</td>
<td>610</td>
<td>68.1</td>
<td>73.1</td>
</tr>
<tr>
<td>No ADL or IADL difficulies</td>
<td>933</td>
<td>63.3</td>
<td>67.7</td>
</tr>
</tbody>
</table>

Notes: ADL = activities of daily living; CESD = Center for Epidemiologic Studies Short Depression; CVD = cardiovascular disease; IADL = instrumental activities of daily living; 3MS = Modified Mini-Mental State Examination.

<sup>1</sup>Number who were sedentary and healthy in Years 1 and 2, where healthy is defined by the variable in column 1.

<sup>2</sup>Adjusted means—percent healthy; active was adjusted to have the same distribution of the Step 4 covariates as sedentary.

There was concern that self-rated health at year 2.5 might lie in the causal pathway. The coefficient at Step 4 of Table 2 (which includes self-rated health at Year 2.5) was smaller than the coefficient at Step 3, which did not include that variable. For all 11 regressions in Table 3, the Step 4 coefficient was smaller than the Step 3 coefficient by 0.2–1.0 percentage points (not shown), and the significance levels were substantively the same at Steps 3 and 4. Inclusion of the compromise covariate, measured at Year 2.5, thus made little difference.

Summary and Discussion

Typical studies of the benefits of exercise use observational data to compare the follow-up health of groups who were exercising at baseline to those who were not while controlling for health-related variables measured at baseline. This approach may be biased against finding benefits, and the population to which such results apply is also unclear. We proposed a limited-bias analytic design and applied it to longitudinal data for older adults, using 10 different measures of health.

Limited-Bias Design

The limited-bias design selected persons who were sedentary and healthy in Years 1 and 2, grouped them by the amount they walked at Year 3, and compared the Year 5 health among the groups. The analysis controlled for covariates measured at Year 2 and for one covariate from Year 2.5 but not for covariates measured at Year 3. This design should be reasonably free from selection bias and did not adjust away any of the benefits of walking by controlling for variables that could already have been modified by the exercise reported at Year 3. Controlling for self-rated health measured at Year 2.5, the compromise covariate, made little difference in the estimated effect, suggesting that Step 3 (column 1 of Table 2) may also be a reasonable limited-bias estimate. The outcome variables coded death as not healthy, permitting all persons to be included in the analysis and any possible benefits of walking in decreasing mortality to be counted. The results are interpretable as the estimated benefit to healthy sedentary older adults of increasing their
walking level to 28 blocks or more per week. The limited-bias design answers the question of interest.

The use of 11 different selection criteria/outcome variables in Table 3 illustrates use of the limited-bias analysis for different outcomes, different subsets of participants, different sample sizes, and different ways of controlling for prior health (either as an eligibility criterion or as a covariate in the regression). The consistency of results in Table 3 suggests that the results are not sensitive to the exact form of the subject selection or the regression models. This design should be considered where appropriate data are available.

Other Designs and Analyses

We compared the limited-bias design with other analytic approaches that might have been taken. Restricting prior walking, but not prior health (column 2 of Table 2), may also be considered a limited-bias design, where the coefficient at Step 4 is interpreted as the estimated effect of increased walking on sedentary persons with “average” health. Restricting analysis to survivors at Year 5 had the expected consequence of underestimating the benefit of becoming active (23), and the analysis also had no prospective interpretation. Studies should attempt to include the possible benefits of walking on survival to avoid such problems.

The typical analysis of observational data regresses health at follow-up on baseline walking, controlling for health variables measured at baseline (column 4 of Table 2). The large walking benefit after control for demographics (25.5 percentage points at Step 2) shrank substantially after adjustment for baseline health variables (Step 5), some of which may already have been improved by walking. Furthermore, the population to which the results are generalizable is difficult to define. Unfortunately, in many cohort studies, data on walking and other physical activity are collected only in the first year of the study, and no prior information is available. It is desirable to collect walking information at every survey wave instead of just the first year.

Benefits of Walking More

Table 3 showed a positive benefit of becoming active for a healthy and sedentary person, for all 11 outcome measures. Because not all these measures would be used in an actual intervention trial, we will mention only self-rated health, ADL, IADL, and gait speed. The strong positive results for self-rated health have already been mentioned in detail. Results were also strong for ADL, where after adjustment 73% of the active but only 66% of the sedentary were free of ADL difficulties 2 years after baseline. The IADL analysis showed a marginally significant benefit (p < .10, two-tailed test). The analyses focusing on gait speed and on the combined exclusion/outcome variable “neither ADL nor IADL difficulties” also had a positive effect size, but this was not significantly different from zero perhaps because of the smaller sample size for those two comparisons.

Given the modest amount of walking required to be classified as active (only four blocks a day), the sizable benefits (5–11 percentage points) are encouraging. Other observational studies examined the association of walking with future health, cardiovascular events, and mortality (5–13). Although all used the typical analytic design, all but one study found significant benefits associated with a modest amount of walking, which is consistent with our findings.

Limitations

The perceived size of a city block may have varied among groups, the number of blocks walked is not a perfect surrogate for physical activity, and activity in a single week may not be typical of the entire year. Misclassification when assigning persons to groups usually results in conservative comparisons among the groups. Thus, the benefit of walking more may be larger than seen here. We did not have sufficient data to study sedentary sick persons or active persons who stopped walking. Although we controlled for a range of demographic and health-related variables, there may be unmeasured confounders that would have altered these results. Regression analyses shown in Table 3 used the same model for all health variables, for simplicity, but analyses optimized for a particular outcome might have had different results.

Conclusions

Typical estimates of the health benefits of walking, based on observational data, are subject to bias and difficult to interpret. When longitudinal data are available, a limited-bias estimate can be obtained by matching subjects on their trajectory of prior walking and health and controlling only for health covariates measured before any change in walking occurred. All limited-bias analyses showed substantial health and survival benefits from increasing walking to as few as 28 blocks per week (median 48 blocks). These findings support the recommendation that sedentary healthy older adults could benefit from even modest increases in the amount they walk.

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