

Explaining changes in walking and bicycling behavior: challenges for transportation research

Kevin J Krizek

College of Architecture and Planning, University of Colorado, Campus Box 126 POB 173364, Denver, CO 80217-3364, USA; e-mail: krizek@colorado.edu

Susan L Handy

Department of Environmental Science and Policy, University of California at Davis, 2132 Wickson Hall, Davis, CA 95616, USA; e-mail: slhandy@ucdavis.edu

Ann Forsyth

City and Regional Planning, Cornell University, 106 West Sibley Hall, Ithaca, NY 14853, USA; e-mail: forsyth@cornell.edu

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Abstract. As issues of traffic congestion, obesity, and environmental conservation receive increased attention globally and in the US, focus turns to the role that walking and cycling can play in mitigating such problems. This enthusiasm has created a need for evidence on the degree to which policies to increase walking and cycling travel have worked. This paper outlines the important challenges researchers face in their attempts to produce credible evidence on walking and cycling interventions. It closes by discussing matters to consider in such research endeavors, including the importance of clear conceptualization, sound research design, measurement innovations, and strategic sampling.

Introduction

As issues of traffic congestion, obesity, and climate change garner increased attention both globally and in the US, focus turns to the role of walking and cycling in mitigating such concerns. One is hard pressed to find any community *not* looking to spur walking and cycling activity through planning activities. This enthusiasm has created a need for evidence on the degree to which different policies have succeeded in increasing walking and cycling travel and producing other benefits for the community. Did the sidewalk encourage more people to become physically active? Did showers and locker rooms at the worksite spur cycling to work? Did the improved intersection increase rates of walking? How much fuel was saved by constructing the bicycle trail? Answers to such questions could influence future policy decisions.

Evidence so far is sparse but may soon be growing. In the US the 2005 federal transportation authorization bill, SAFETEA-LU (safe, accountable, flexible, efficient transportation equity act: a legacy for users), provides more than \$500 million to communities to construct nonmotorized transportation facilities and promote use of these facilities. The Nonmotorized Transportation Pilot Program (NTPP) specifically included \$100 million for pilot programs in four communities to increase levels of walking and cycling. Such ‘interventions’ provide a living laboratory often called for but rarely exploited in the transportation planning field. Thus, the NTPP also required an evaluation of the efficacy of these programs under the logic that documenting benefits in one community provides a basis for judging the potential benefits of proposed policies in other communities.

However, evaluation research on programs like those funded by the NTPP is rare because it is fraught with practical challenges as well as political ones: expectations are high, interventions are modest, and effects may be unclear. Pedestrian and cycling

advocates have struggled for decades to gain significant attention for their proposals based on the belief that infrastructure improvements can make a difference. But such improvements typically represent marginal changes within extensive and complex transportation systems in which travelers have multiple options with respect to mode and route choice. In these contexts any behavioral changes that such improvements effect are likely to be relatively small, making them difficult to establish statistically. In addition, disentangling the effects of pedestrian and cycling improvements from the effect of other factors is tricky. For advocates, studies that do not produce proof of the effectiveness of such improvements can be hard to accept because they potentially jeopardize future investments.

The purpose of this paper is to outline the particular challenges and difficult trade-offs that researchers face in evaluating the effectiveness of interventions designed to increase walking and cycling through infrastructure improvements and other changes to the built environment; our purpose is not to review the existing evidence or pass judgment on the effectiveness of such interventions.⁽¹⁾ Our intent is more simply to provide guidance to researchers engaged in such endeavors, and to build awareness among potential consumers of their results, including the planners, advocates, and decision makers of these challenges. We first outline a conceptual framework for assessing the impacts of increased walking and cycling. We then explore and describe a number of methodological issues facing researchers in designing before-and-after studies of the effects of walking and cycling interventions, including research design, conceptualization, measurement, and sampling. The paper offers a number of pointers for improving such research.

Conceptual framework

Researchers in the public health and transportation fields—both of which are interested in walking and cycling—have highlighted the need to use sound theoretical models in attempts to understand (through research) and influence (through interventions) human behavior (TRB and Institute of Medicine, 2005). Health behavior researchers, including physical activity researchers, rely on a set of psychology-based theories referred to as social ecological models (McLeroy et al, 1988; Sallis and Owen, 1997). According to these models, behavior is influenced by different factors at multiple levels, including individual, interpersonal, and environmental levels (see figure 1); thus: (1) studies of walking and cycling behavior must account for factors at all levels, and (2) interventions to encourage heightened levels of walking and cycling, what we call ‘walking/cycling interventions’, need to operate at several different levels (Pikora et al, 2003).

Travel behavior researchers, in contrast, draw on utility maximizing theory from economics to explain the choices that individuals make about travel, including the decision to walk or cycle (Handy, 2005; Handy et al, 2002). In this field, researchers assume that individuals choose the option, from among some set of available options, that maximizes their utility. Utility, in turn, is assumed to depend in large part on the cost of travel, including both monetary and time costs. This approach thus emphasizes factors at the environmental level, particularly travel distance. In extending this theory to the study of walking and cycling, researchers have also recognized the importance of the quality of the environment as a determinant of behavior. Other recent applications of travel behavior theory emphasize the role of individual attitudes and preferences

⁽¹⁾ Transportation interventions have been reviewed by Ogilvie et al (2004). Recent reviews of the empirical evidence on factors influencing walking include Saelens and Handy (2008) and Badland and Schofield (2005).

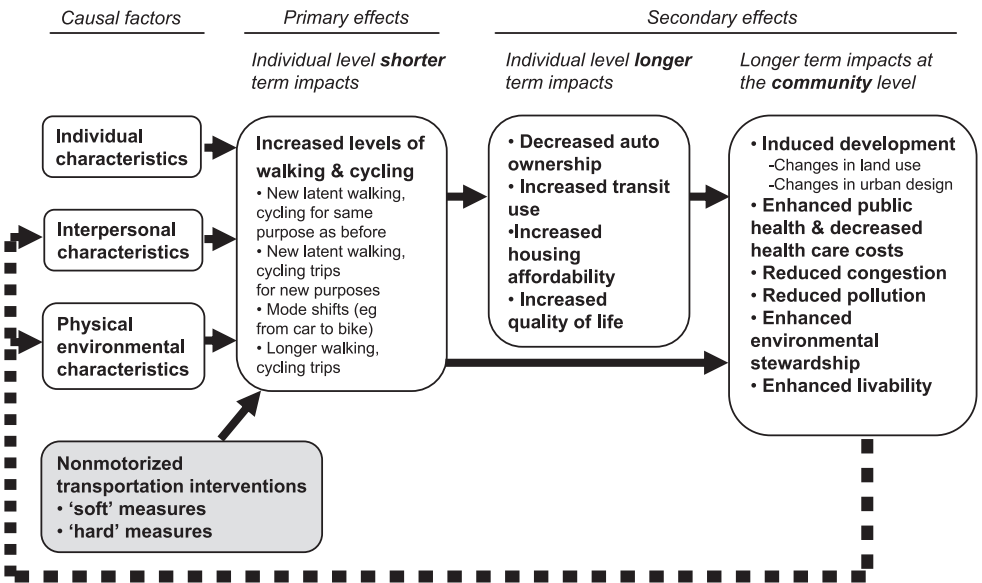


Figure 1. Conceptual framework.

(eg Gärling et al, 2003) and interpersonal relationships in the form of household interactions (eg Bhat and Pendyala, 2005) and social networks (eg Axhausen, 2003). The two theoretical frameworks are thus beginning to converge on a common framework that considers the interplay between individual, interpersonal, and environmental factors.

As shaped by these theoretical perspectives, walking and cycling interventions generally fall under the umbrella of either 'soft' measures (eg education, encouragement, or enforcement) or 'hard' measures (eg infrastructure investments, such as better street crossings or bicycle lanes). The former may bring about increased walking and cycling through psychological changes (the individual level), such as increased desire or motivation to walk, or through complex social interactions (the interpersonal level)—for example, by instigating the exchange of information with peer groups (eg 'someone else at work told me how easy it was to bike to work'), a form of social learning (Ajzen, 1991; Bandura, 1986; Baranowski et al, 2002). The interventions from the 'hard' category consist of changes in the built environment, including transportation infrastructure as well as land-use patterns. Behavioral changes theoretically result primarily from the increases in access, attractiveness, safety, comfort, and security that these infrastructure improvements offer. Additionally, they may stimulate changes in perceptions, attitudes, and other psychological factors similar to those anticipated by soft measures.

Regardless of the factors influenced by the intervention (eg whether individual, interpersonal, or environmental factors), primary impacts occurring in the short term could take the form of replacement of trips previously made by motorized modes, generation of new walking or cycling trips, or lengthening of the total time or distance of walking and cycling trips. Increased levels of use at the individual level, in turn, lead to secondary effects over the longer term at the individual level, such as changes in auto ownership, increases in transit ridership, or improved health. In addition, a number of secondary benefits may also accrue to the community as a whole over the longer term. For example, a new bicycle path, if extensively used, may prompt or stimulate development and increased private investment—impacts that could take years to come to fruition.

These secondary effects, at both the individual and community level, each contribute to other desired policy outcomes, such as improved air quality, reduced health-care costs, and increased livability (figure 1). The broad range of possible policy outcomes from walking and cycling activity expands the justification for programs to promote such activity. At the same time, however, such oft-cited benefits generate other expectations that are more challenging to support because the benefits are more tenuously connected to levels of walking and cycling. Issues often mentioned at the forefront of walking and cycling policy initiatives—traffic congestion, obesity, and environmental conservation—have many different causes, and levels of walking and cycling may have a minor effect on the overall extent of the problem. The challenge for researchers is to separate the effect of the intervention from the effects of these other forces.

Research design

The research design goal in this context is to be able to say with some certainty that an intervention of a certain type will have an impact of an estimated magnitude and/or character. To legitimately make such a statement one needs evidence that the intervention has a causal effect on behavior. Widely accepted criteria for establishing causality are association, nonspuriousness, and time order (Singleton and Straits, 2005). Meeting these criteria is not easy in behavioral research, and it usually occurs in stages as knowledge accumulates and research designs evolve. Below we outline several approaches to research design for testing the conceptual model described above and discuss their effectiveness in meeting the criteria for causality.

Initial research on a behavioral question generally uses cross-sectional designs. The purpose of these early studies is to develop a broad understanding of the factors associated with the behavior of interest. Associations between potential causal factors and behavior are examined for a sample of the population at one point in time. Statistically significant associations meet one of the criteria for establishing causality, and the evidence is stronger if the study statistically accounts for other potential causal factors, thereby supporting the assertion that the associations are nonspurious. Most research on walking and cycling behavior falls into this category (eg Cervero and Duncan, 2003; Dill and Carr, 2003; Krizek and Johnson, 2006; Nelson and Allen, 1997; Rietveld and Daniel, 2004). These studies point to infrastructure design, street patterns, destinations, traffic, and population densities as key factors associated with walking and cycling. They do not, however, prove that a change in any one of those factors will lead to a change in walking and cycling.

Cross-sectional studies provide the foundation for studies that use panel, experimental, or quasi-experimental designs to provide stronger evidence of causality. Researchers in the public health field commonly use evidence from cross-sectional studies to decide what factors to target in interventions designed to change behavior (Kahn et al, 2002; Rychetnik et al, 2002). The intervention then serves as the ‘treatment’ in the study. In a true experimental study a sample of participants is randomly assigned to treatment and control groups, to ensure that the association is not spurious, and behavior is measured both before and after the treatment, to establish time order for the cause and effect. A significantly greater change in behavior for the treatment group than for the control group is evidence of a causal effect. A true experiment can often be used to study the effect of a soft walking or cycling intervention, but most hard interventions do not allow for the random assignment of participants to treatment and control groups.

When a true experiment is not possible, studies ideally employ a quasi-experimental design and control for factors other than the intervention that might influence walking and cycling. A common approach is to measure behavior in communities before and

Table 1. Research designs, advantages, and disadvantages.

Label of research approach	Representation O = observe, T = treatment	Primary advantages	Primary disadvantages
Cohort designs (ie panel)	same individuals: $O_{t, \text{pre}} \rightarrow T \rightarrow O_{t, \text{post}}$ $O_{t, \text{pre}} \rightarrow O_{t, \text{post}}$	Can sample exact same people in different settings (or after a treatment)	Difficult to account for attrition. Changes may be due to causes other than the treatment.
Two-group pretest-posttest design using an untreated control group	$O_{t, \text{pre}} \rightarrow T \rightarrow O_{t, \text{post}}$ $O_{t, \text{pre}} \rightarrow O_{t, \text{post}}$	Can isolate causal factors hypothesized to influence outcome variables	Sometimes difficult to find similar control group.
One-group pretest-posttest design	$O_{t, \text{pre}} \rightarrow T \rightarrow O_{t, \text{post}}$	Cost effective	Difficult to rule out other explanatory factors.
One-group posttest-only design (ie cross-sectional)	$T \rightarrow O_{t, \text{post}}$	Cost effective	Lack of baseline data to compare against. Difficult to rule out other explanatory factors.

after an intervention occurs and at the same points in time in control communities matched on key characteristics to the community where the intervention is taking place (Barnes et al, 2005). Examples include studies measuring the impact of street lighting (Painter, 1996), bicycle lanes (McBeth, 1999), and infrastructure improvements around schools (Boarnet et al, 2003). Several variations on this pretest–posttest approach are possible, falling into a ‘hierarchy of robustness’, depending on the constraints of the situation (table 1). However, without proper control groups—matched for conceptually significant demographic, socioeconomic, and built environment features—it is impossible to isolate the effect of the treatment from the effects of broader phenomena, such as social changes or changes in the price of alternative modes.

Thus, policy makers and advocates, as well as researchers themselves, must be careful not to overstate the strength of the available evidence; cross-sectional results are often cited as evidence that an intervention will cause a certain outcome (Winship and Morgan, 1999). On the other hand, the absence of a body of intervention studies that provides strong evidence of the effects of an intervention does not mean that communities should not attempt walking and cycling interventions. After all, cross-sectional studies can provide solid evidence on potentially promising approaches. The body of intervention studies will only grow if communities are willing to try new approaches—and to work with researchers to rigorously evaluate them. In the meantime, interventions can be taken on with a realistic but not overly confident assessment, based on available evidence, of their potential.

Conceptualization

Whichever research design is ultimately employed, a number of matters need to be addressed with respect to how walking and cycling behaviors are defined and subsequently measured. A comprehensive analysis of walking and cycling activity needs to pay special attention to the following four conceptualization issues.

Walking versus cycling?

Conventional transportation analysis (and policy) often groups walking and cycling together as nonmotorized modes, thereby implying that such travel serves similar purposes and markets. Both activities are human powered and entail greater direct exposure to environmental conditions than transit or auto. While grouping them often suffices (when combined, they comprise almost 10% of all trips in the US), walking and cycling are functionally different in that they fulfill different daily purposes for individuals and pose different problems for facility planning and community design:

Walking. All trips start and end on foot, making walking essential to all travel. Requirements for sidewalks and other pedestrian infrastructure (eg crosswalks, public spaces) may be embedded within local zoning and subdivision codes. Pedestrian trips are usually short, often no more than a few city blocks. Finally, and most importantly, many factors influence the choice to walk for travel purposes, including the attractiveness of the route (eg interesting facades, a variety of architecture, the absence of long, blank walls), route choices for variety and safety, and the number of destinations within a walkable distance (eg work places or nearby stores).

Cycling. Bicycle trips generally traverse longer distances at higher speeds than pedestrian trips, requiring longer corridors (such as wide curb lanes and on-street or off-street bike paths), and are considered street-legal vehicles for most local roadways. The bulk of bicycle trips that are made are discretionary in nature, and, whereas nearly everyone can walk, bicycling applies to a considerably smaller market of travelers, for a variety of reasons. Cycling requires equipment which must be stored when not in use. Furthermore, not everyone owns or has access to a bicycle. During the summer months in most of the US, the cycling market includes just over a quarter of the American population, but there are far fewer year-round cyclists using this mode for travel other than recreation (BTS, 2003). Bicyclists who share the road also have unique safety concerns, dealing with the close proximity of autos speeding by, for example. Gender differences are greater for bicycling than for walking (Krizek et al, 2005).

Grouping the modes can help in analyzing the role of nonmotorized travel as a part of overall travel behavior. However, a more detailed understanding of the factors that spur pedestrian travel and bicycle use requires a separation of the two activities.

Which behavioral dimensions?

Both walking and cycling have many different behavioral dimensions that might be of relevance in understanding the primary and secondary effects of an intervention. A fundamental distinction is between traveling for utilitarian versus recreational purposes. Different factors have been shown to affect the two types of travel (Saelens and Handy, 2008), different measurement instruments may be required for each, and each may demand different policy initiatives. Other dimensions to consider include whether or not individuals walk or cycle, the frequency with which they walk or cycle, the distances they walk or cycle, the time they spend walking or cycling, the purpose of trips for which they walk or cycle, and the routes they choose for walking or cycling. Intermediate effects might also be of interest—for example, changes in attitudes as a result of the intervention that do not necessarily translate into changes in behavior, at least in the short term. Different dimensions have different implications for policy outcomes and different implications for data collection, as described below.

Which secondary effects?

In addition to the direct effect of the intervention on walking and cycling levels, one or more secondary effects occurring over the longer term may be of interest. Legislation from section 1807 of SAFETEA-LU, for example, calls for the development of statistical information about whether pilot projects funded under the legislation have

led to changes in motor vehicle usage, nonmotorized transportation usage, public transportation usage, congestion, energy consumption, frequency of bicycling and walking, connectivity to community activity centers, health, and environment. Measuring any one of these outcomes may be challenging; measuring all of them could provide a career's worth of work for a transportation researcher. Narrowing the list to those that can be practically measured and which offer the greatest promise of substantive benefits will help to ensure the quality and usefulness of the research.

Several leading practitioners and academics suggest that many of the benefits often touted for walking and cycling facilities—decreased congestion, decreased consumption of natural resources, lower production of harmful pollutants, and even overall increases in physical activity—will not ultimately come to fruition (eg Giuliano and Hanson, 2004; Lockwood, 2006). Research shows that people do very little walking overall (Oakes et al, 2007), meaning that secondary benefits are marginal. Rates of bicycling are currently so low that even a quadrupling of the number of people in the United States who bike to work would lessen environmental and other harms from motorized vehicles by a miniscule degree—prompting some to term it a fringe mode (Gordon and Richardson, 1998). Furthermore, making small interventions in the existing built environment (eg improving intersections, installing sidewalks) or other walking or cycling policies or programs (eg installing showers) will likely have only a modest effect on one's propensity to drive less [driving is relatively inelastic: even dramatic increases in gas prices have not had a significant impact on levels of driving (Hughes et al, 2008)]. This is not to say that the individual benefits are insignificant; rather, that their cumulative effect is limited.

A few select benefits that are more related to the relatively ambiguous goal of 'livability' appear to hold more hope for meeting expectations. A prominent transportation consultant, after reviewing much of the literature on the benefits of nonmotorized modes and interacting with policy officials about such matters argues, "from a policy perspective, the subject of non-motorized transportation presents a bit of a dilemma. Statistics are spotty and the literature appears to be heavily populated with advocacy. Thus, the overarching policy questions are whether non-motorized transportation, in fact, is a transportation services issue or a lifestyle issue, and is that distinction important?" (Lockwood, 2006, page 2). As Giuliano and Hanson (2004, page 398) suggest, "building communities with abundant walking and biking opportunities may be more about livability than solving transportation problems." Of course, measuring livability presents its own challenges (Krzek, 2006).

Which populations?

Different segments of the population also have different patterns of walking and cycling. Consider, for example, three distinct populations which are likely to be affected differently by walking and cycling investments: elementary school students, university students, and timid cyclists. Elementary students may need programmatic interventions that increase the perceived safety of their route to school, such as the walking school bus. University students may be sensitive to parking pricing on campus, and good sidewalk connections between more peripheral (cheaper) parking spaces and the campus may encourage walking. Timid cyclists may need off-street bicycle trails. As a result, bicycle planning practice differentiates between beginning cyclists, recreational cyclists, and serious cyclists, and advocates for planning facilities according to the type of cyclists to be served (AASHTO, 1999). Public health interventions designed to increase walking are also tailored to specific populations, such as women, children, or the elderly (eg King et al, 2006). Studies of the effectiveness of interventions must take into account the experience level and the attitudes, preferences, and

Table 2. Populations and different outcomes.

Example populations of interest	Example effects expected from improved walking or cycling facilities, by different populations					
	further distance of walking or bike travel	increased rates of walking or bike travel for varied trip purposes	using walking or bike travel to substitute for selected auto trips	increased walking or bike travel for recreation use	greater acceptance of walking or bike use for others	increased choice and quality of life
Devoted walkers and cyclists	●	●	○	○		●
Periodic walkers and cyclists		○	◐	◐		●
Potential walkers and cyclists			○	●	◐	●
Nonusers					●	○

Magnitude of effect for the specific population:
 ● = first-order effect: short-term, large effect from intervention
 ◐ = second-order effect: longer term or smaller effect from intervention
 ○ = third-order effect: long-term and small effect from intervention
 Empty cell = little effect for that population

perceptions of the population segment affected. As a further articulation of this point, table 2 presents the range and magnitude of effects that may be expected from different populations.

Careful delineation of treatment and control groups is important to ensure comparability or exchangeability of people—so that one is not comparing unlike people in addition to unlike situations (Oakes et al, 2007). But this is not always easy. Although some programs are targeted at specific population segments (eg safety programs in elementary schools), others may affect the entire community to greater or lesser degrees. Consider, for example, a major new recreational trail. First, different groups might show different sensitivities to the presence of a trail. In particular, the new facility may help to promote some people into a more active group, say from potential to periodic walker, but it may have little effect on groups that are already active—that is, devoted walkers (table 2). The trail may thus serve as a treatment for some groups more than for other groups. Second, the effect of the trail is likely to be less for residents who live farther from the trail. Experimental designs call for the identification of a control group not affected by the trail opening, to compare with the treatment group, but how far from the trail does the treatment end? One solution is to use distance from the trail as a control variable, rather than dividing the population into treatment and control groups. These issues influence the definition of the target population, with implications for sampling strategies, as described below.

Measurement

Primary effects

Measuring walking or cycling is another challenge for researchers. There are three general strategies to obtain information about walking and cycling behavior: (1) ask people to report for themselves the details of their behavior (self reported), either through a survey or through a travel diary; (2) observe people's activity, either manually or using sensing equipment; (3) employ instrumentation on bodies or bicycles

Table 3. Measurement strategies and how well the behavior is captured.

Measurement strategies	Phenomenon or behavior to measure				
	who does and does not walk or cycle	number of trips	distance	purpose or destination	intensity
Self-report	+	✓	-	+	-
Observation	-	+	-	-	✓
Instrumentation	+	✓	+	-	+ ^a

+ : is good at capturing this phenomenon/behavior; ✓ : fair; - : poor.

^aThe ability to measure intensity differs from accelerometers (good) to GPS units (poor).

to measure behavior (Troiano, 2005). Self-report measures tend to be cheaper but less accurate than observational or instrumental approaches, but each has distinct advantages and disadvantages and is more or less appropriate for different dimensions of walking and cycling, as shown in table 3. Until recently, few efforts have compared measures across strategies (Troped et al, 2001).

The first strategy, self-reporting, generally has two problems. The first problem stems from issues of definition, particularly for walking trips. What constitutes a walking trip is not always clear to survey respondents; researchers themselves do not always adopt a clear definition. For example, surveys may or may not capture the following walking behaviors: (1) walking from one store in the mall to another, (2) walking five blocks from home to the bus, (3) walking the dog, (4) walking to the store when the visit to the store is simply an excuse for walking. Survey instruments must be carefully designed to capture all relevant walking and cycling. Discrepancies in definition make it difficult for differing surveys with different instruments and coding to be compared with one another (Agrawal and Schimek, 2007).

Travel surveys tend to undercount walking and cycling because many people fail to think of walking or cycling as legitimate modes of travel; they may omit walking and cycling trips when asked to report their daily travel. For this reason, the 2001 National Household Travel Survey (NHTS) made a special effort to prompt respondents about walking and bicycle trips using a follow-up telephone questionnaire. Interviewers asked, "Did [you] use any other type of transportation during [your] stay in [city here], including bicycling and walking? So far, I have recorded [N] trip(s). Before we continue, did [you] take any other walks, bike rides, or drives on [trip date]? Please include any other trips where [you] started and ended in the same place." Walking trips increased significantly between the 1995 National Personal Travel Survey and the 2001 NHTS, and survey administrators believe this increase is attributable to the improved prompts, rather than a true increase. Also of concern is the period of time covered by the diary: a one-day diary may miss occasional use of walking and bicycling as a mode of transportation and as a form of exercise or recreation (Agrawal and Schimek, 2007). Another problem with the self-report approach is that walking and cycling are considered, by many, to be virtuous behaviors. Most people feel they should be doing more of it and therefore tend to overestimate the degree to which they engage in such activities. The magnitude of this 'halo effect', as it is often labeled, is uncertain.

Data collected using automated means are often considered superior to those obtained by self-reported means. Two types of instrumental approaches are used. One approach is to use counters, infrared sensors, or video cameras in specific locations to count pedestrians and cyclists. In this case, data are obtained only for those who are walking and cycling on specific facilities (Aultman-Hall and Hall, 1998;

Menard et al, forthcoming) or in specific contexts (Willis et al, 2004). This approach ignores those who are not walking or cycling, and it does not provide a mechanism for obtaining more than basic information (such as gender and approximate age, in the case of video cameras) about the people who are walking and cycling. It is therefore of limited help in efforts to understand why individuals walk or cycle, though it can be used to measure changes in behavior resulting from improvements to facilities or soft measures such as promotional programs.

A second approach is to attach a motion sensor directly to individuals. Accelerometers, devices that measure motion of the hip, are often considered the gold standard for measuring physical activity. However, this approach has its own limitations. For one thing, it can be costly: an accelerometer currently costs approximately \$300–\$400 per unit. In addition, such units measure weight-bearing activity (like walking) better than cycling. However, such instruments have the benefit of measuring intensity of activity. The less expensive pedometers, in the tens rather than the hundreds of dollars, have a different set of issues, only giving an overall count of movement with no information about type of activity or intensity; they provide reasonably good estimates of walking activity but they do not accurately measure bicycle use.

In addition, questions arise about how best to process the information—literally minute-by-minute readings of motion in the case of accelerometers (Troiano, 2005). For example, if there is little or no motion recorded, has the person taken off the accelerometer or are they merely being sedentary? The counts or values measured by the accelerometer have different meanings depending on the type of activity being measured, or a person's metabolic rate (Matthews, 2005). Research is currently under way to standardize methods of cleaning and interpreting the data as well as validating it against other data collection strategies.

Some new technologies hold promise but until recently have been too expensive, bulky, or require much maintenance. For example, global positioning systems (GPS) are getting smaller and cheaper and memory is increasing, but they can be relatively bulky and require frequent battery changes or recharging. They are also a more indirect measure of activity intensity, and typically do not operate inside buildings, which limits their use in examining overall physical activity (Duncan et al, 2007). However, if mounted to a bicycle, they have some advantages (Menard et al, forthcoming). Using multiple measures can help triangulate findings, but adds cost. The bottom line is that measuring walking and cycling is in no way a simple task.

Secondary effects

Secondary effects of an intervention depend not just on the increase in walking and biking that the intervention produces but also on the type of activity that the additional walking and bicycling replaces. With a fixed amount of time in a day, an individual who walks or bikes more must do less of something else; what that something else is determines whether the secondary effects are positive or negative. Physical activity outcomes are positive if additional walking and bicycling replaces inactivity; if it replaces other forms of physical activity, secondary effects may even be negative. Environmental outcomes are positive if additional walking and bicycling replaces driving; if it replaces other activities, the environmental benefits are likely to be limited. These possibilities mean that potentially replaceable activities must be measured as well.

Evidence on physical activity suggests that walking for transportation may simply replace walking for recreation. Many studies show that transportation walking is fostered by dense, mixed-use areas and recreational walking is higher in other locations (Forsyth et al, 2007; 2008; Lee and Moudon, 2006). However, a new round of research

examining total walking shows that residents of the lower density areas make up for lower walking for transportation with greater leisure walking. In fact, rather like a travel budget, these studies seem to show the existence of a physical activity budget, so that an increase in one form of physical activity leads to a decrease in another (Oakes et al, 2007; Rodriguez et al, 2006). Studies aiming to document the effects on total physical activity must include a measure of total physical activity. Options, as described above, include self-reports, pedometers, accelerometers, and emerging technologies such as GPS; in general, the more accurate options are the more costly and burdensome options.

Evidence on vehicle travel suggests that increased walking and biking does not necessarily reduce driving: not every walk or bicycle trip replaces a driving trip, and, even when it does, the distance are relatively short. One study found that 73% of walking trips were substitutes for driving trips, but estimated that this substitution saved only 2.1 miles of driving per person over a month (Handy and Clifton, 2001). Furthermore, evidence points to a latent demand for auto travel in congested urban areas: any relief in congestion coming from some individuals substituting walking and biking for driving will be immediately consumed by additional driving from other individuals (Cervero, 2002; Noland, 2001). Thus, studies aiming to document the effects on the environment must include a measure of vehicle miles traveled for both treatment and control groups. The standard approach is to use a travel diary survey of the type employed in the NHTS. As noted earlier, these surveys have historically not given accurate measures of bicycling and walking trips. Their ability to measure vehicle travel accurately has now been established with the help of on-board GPS units (Doherty et al, 1999; Wolf et al, 2001), though nonresponse bias is a continuing concern. These surveys are expensive and burdensome for the respondent, however.

Sampling

Sampling is challenging in studies of the impact of bicycle and pedestrian interventions for two reasons: the lack of a clear delineation of the 'treatment group' experiencing the intervention, as discussed above, and the relative infrequency of walking and cycling as modes of transportation, which makes achieving a sufficient sample size difficult.

Sample size

Nationally, these modes represent a small portion of total travel when viewed in the context of conventional multimodal national transportation statistics (Pucher and Dijkstra, 2001; Pucher et al, 1999). The relative rarity of walking and cycling means that it is hard to assemble a sufficiently large sample of people who cycle and/or walk, or of walking and cycling trips. Walking comprises 8.7% of all trips, or about 3.8 walk trips per week, according to the NHTS, with a mean distance of 0.62 miles and a median distance of 0.25 miles owing to the skewed distribution in walk trip length (Agrawal and Schimek, 2007). The recent Twin Cities Walking Study in the Twin Cities area (including St Paul), which employed a seven-day travel and leisure walking and biking diary and which only sampled individuals in the warmer months, found that, over the course of the seven days, 519 people out of 715 in the sample walked in trips or recreational loops that were not just getting to a form of motorized transportation, and they walked a median distance of 0.74 miles per day or about seven and a half blocks—a small share of overall travel (Oakes et al, 2007). Of course, as noted earlier, walking is probably undercounted in these surveys because most people walk as part of trips by other modes (eg walking to the car).

The challenges are even greater for bicycling. A 2003 survey from the Bureau of Transportation Statistics found that almost three quarters of the American population

never rode a bicycle or had not done so during a thirty-day period over the summer of 2002 (BTS, 2003). The NHTS reports that the percentage of adults who cycled on their survey day ranges across cities from about 0.25% to about 2.35%. Previous work examined a variety of data sources to arrive at the percentage of Americans who bicycle over a given period of time (Barnes and Krizek, 2005). Results suggest 1% of adults bicycle on a given day, 5.3% bicycle on a given week, 16% bicycle on a given month, 29% bicycle in the summer, and 40% bicycle in a year. Even in central cities deemed cycling friendly (eg Minneapolis and St Paul), the regional travel survey for almost 2000 households provided usable detailed travel behavior for only eight-six cyclists (Krizek and Johnson, 2006). In the Twin Cities Walking Study, a mere seventy-three individuals (10.2%) traveled by bike at all in a seven-day period. For those seventy-three people the median distance bicycle per week was 9.6 miles. Nationally, cycling comes in at a mere 0.8% of all trips (Federal Highway Administration, 2001). This means a very large sample of the population is needed to achieve a sufficiently large sample of cyclists and bicycle trips for analysis purposes.

The relative rarity of walking or cycling means that measuring statistically significant changes among the general population can be costly. As an illustration, assume travel diary information was collected for 1000 (n) residents in a community from one year to the next; at an estimated \$100 per travel diary interview (conservative estimate), this amounts to \$100,000 of direct survey costs. Assume that the 1000 individuals in each community complete the US average of four trips per day, yielding information on about 4000 trips. Assume the communities have cycling rates above the national average—say at 1% of all trips ($p = 0.01$)—this would result in a mere 40 of the 4000 trips for cycling (and most of these forty trips would likely be from the same people). Now suppose that data from the postintervention survey show the mode split of cycling doubles to 2%, or eighty cycling trips in the community. A statistical analysis at the 95% significance level would be able to confirm an increase in cycling (that is, such a change is outside the bounds of the confidence interval). If, however, the change in use was smaller than an increase to 2% (ie anything less than doubling), a statistical analysis would not be able to confidently detect such a change, assuming this sample size. Put another way, the chance of detecting a *doubling* of the rate of cycling among the general population from 1% to 2% of all trips ($p = 0.02$) is about 92%; however, an increase to a more likely outcome of 1.2% ($p = 0.012$) will confidently be detected only about 44% of the time.

In general, the weaker the relationships to be detected, the more control variables one will use, the smaller the number of cases in the smallest class of any variable, or the greater the variance of one's variables, the larger the sample size has to be. One solution is to employ a quota sampling approach, in which individuals exhibiting specific behaviors are recruited until a certain sample size for this group is obtained. This approach is often used when the target group represents a small share of the total population, as is the case for cyclists and, to a lesser extent, pedestrians. The result is a disproportionate stratified sample: cyclists represent a greater share of the sample than they do of the population. In analyzing the results, the sample for each group is weighted according to its share of the total population in order to correct for disproportionate sampling. This is an effective and well-established way of ensuring a sufficiently large sample size for infrequent behaviors.

Conclusions

Despite their increasingly recognized potential as a solution to several pressing problems, walking and cycling remain the most understudied—and least understood—modes of travel. Complicating the study of walking and cycling as modes

of transportation is their frequent use for exercise and recreation rather than for travel. The lack of research in this area contributes to, and is hampered by, the lack of a consistent effort to collect and distribute data on these behaviors and the environments in which they occur. The deficiency of secondary data sources focusing on walking and cycling travel is well documented. A growing appreciation of this deficit has led to a number of efforts to improve the quality and quantity of data on walking and cycling. Additional data will contribute to a better understanding of the factors that influence the choice to walk or cycle.

But what policy makers ultimately need to know is whether a particular policy, project, or program will lead to an increase in walking and bicycling. Such evidence comes from intervention studies—studies of the impact of the policy, project, or program on the behavior of those to whom the intervention applies. In documenting the impact of bicycle and pedestrian interventions, researchers need to consider the following:

(1) *Research design.* The best approach for establishing causality is to measure behavior before and after the intervention for those individuals targeted by the intervention, and for a control group of individuals not exposed to the intervention.

(2) *Conceptualizing.* Clear definition of the primary impacts of interest, combined with the selection of a limited set of promising secondary effects, is key. Identification of the role of other factors potentially influencing walking and cycling is also essential.

(3) *Measurement.* Different measurement techniques offer different strengths and weaknesses. Measurement approaches for primary effects should include not only those using facilities but also those who are not. Measures of potential substitutable behaviors are also needed to ensure accurate assessment of secondary effects, such as congestion reduction.

(4) *Sampling.* Because walking and cycling are rare relative to other forms of travel, large sample sizes are needed to ensure the ability to arrive at conclusions with desired levels of statistical significance. Quota sampling is an effective way to achieve the same result with smaller sample sizes.

Adhering to all of these recommendations takes skill, time, resources, and patience, and many not be possible in every study. Researchers have a responsibility to employ sound methodologies and represent their results accurately. But consumers of research also have a responsibility to understand the limitations of the available evidence and not misuse that evidence in making the case for bicycle and pedestrian interventions. We hope that we have helped both researchers and research consumers understand better the challenges inherent in efforts to document the effects of bicycle and pedestrian interventions.

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