# Measuring Community Bicycle Helmet Use among Children

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## SYNOPSIS

Bicycling is a popular recreational activity and a principal mode of transportation for children in the United States, yet about 300 children die and 430,000 are injured annually. Wearing a bicycle helmet is an important countermeasure, since it reduces the risk of serious brain injury by up to 85%. The Centers for Disease Control and Prevention (CDC) have funded state health departments to conduct bicycle helmet programs, and their effectiveness has been evaluated by monitoring community bicycle helmet use. Although it would appear that measuring bicycle helmet use is easy, it is actually neither simple nor straightforward. The authors describe what they have learned about assessing helmet use and what methods have been most useful. They also detail several key practical decisions that define the current CDC position regarding helmet use assessment. Although important enough in their own right, the lessons learned in the CDC's bicycle helmet evaluation may serve as a model for evaluating other injury prevention and public health programs.

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Bicycling is a popular recreational activity and a sound alternative mode of transportation that can reduce traffic congestion and air pollution and enhance physical fitness. An estimated 44.3 million US residents younger than 21 years ride bicycles, and, on the average, they spend about five hours per week doing so.[1] Injuries associated with bicycling are an important public health issue for children in the United States. In 1998, 269 children and teens died of bicycle-related injuries, and an estimated 430,000 were treated in emergency departments for bicycle-related injuries.[2,3]

Wearing a bicycle helmet reduces the risk of brain injury in a crash by 74% to 85%.[4] Approximately one fatal head injury could be prevented every day, and one nonfatal head injury every four seconds, if every rider wore a helmet.[5] Market forces have made helmets widely available for purchase for under $10, yet only about 50% of US children between 5 and 14 years old own a helmet, and only 25% report always wearing it while bicycling.[6]

Because children and teens have higher bicycle-related mortality and injury rates than any other age group, many bicycle helmet promotion programs target this age group.[2,3] Intervention programs include, singly or in combination, elements of public education and information, school education, helmet give-away or discount programs, and state laws, local ordinances, or school policies that mandate helmet use. Because funds for these programs are relatively scarce, it is important to determine program effectiveness.

In this article, we focus on the issue of determining bicycle helmet program effectiveness through evaluation, rather than program delivery. We explore some of the decisions that public health workers must make as they plan evaluations of helmet promotion programs. We describe the state-of-the-art methodology of helmet use measurement, the current recommended approach used by the CDC, and problems that remain. This approach may be useful in monitoring injury prevention programs that deal with other causes of injury.

## WHAT SHOULD BE MEASURED?

We prefer to measure helmet use rather than head injury rates, since (*a*) few communities have surveillance systems that adequately capture nonfatal head injuries for bicycle or any other cause of injury; (*b*) the annual number of injury events for most communities is too small to yield stable estimates; (*c*) a long interval often occurs between the occurrence and reporting of cases, if they are reported at all; and (*d*) injuries may not be coded (or are coded improperly) according to the external cause-of-injury codes (E codes), making case-finding and classification cumbersome. Because in most communities bicycle-related brain injuries are a relatively rare event, measuring local bicycle-related head injury rates requires aggregating many years of data to achieve adequate numbers for analysis.

Because bicycle helmet use relates directly to the likelihood of brain injury, and because promoting helmet use is the central activity of most community-based bicycle-safety programs, we believe that helmet use is the most appropriate proxy indicator of preventable bicycle-related head injuries. Unlike measurements of head injury, community measurements of helmet use can generate sufficient statistical power within a short time by increasing the number of rides observed. An estimate of the predicted number of prevented brain injuries can be calculated from the change in helmet use, once the etiologic fraction is known.[7]

Similarly, we prefer to measure helmet use, not helmet ownership. Ownership is a necessary, but not sufficient, condition for use. For example, in two rural Texas towns where every elementary school child had been given a helmet, fewer than 40% were observed to use it.[8] Accurately estimating ownership is difficult. Data based on sales records may be misleading because one or more sources of purchase (e.g., retail, mail order, yard sales) may be overlooked. Stores may be reluctant to release sales information. Because a helmet may be used for many years, current sales figures do not indicate prevalence of ownership. Helmets may be shared among siblings. A child may own a helmet that no longer fits. Therefore, local sales or personal ownership of helmets should be considered supportive, not primary, evidence of program effectiveness.

## HOW SHOULD HELMET USE BE DEFINED AND CATEGORIZED?

The term *use* includes components of frequency and correctness. Incorrect use, e.g., a loose chin strap or tilted helmet position, is relatively easy to detect during observational surveys, but difficult to determine by telephone or mail surveys. In contrast, determining frequency of use is ascertained more readily by phone or mail survey, since it is impractical to intercept a rider during an observational study to ask. Because long-term recall may be faulty, responses to inquiries about use “in the last month” or even “the last time you rode a bike” may be more accurate than use “during the past year.” Therefore, we recommend asking about helmet use during a recent, brief period, and assume that it represents general use.

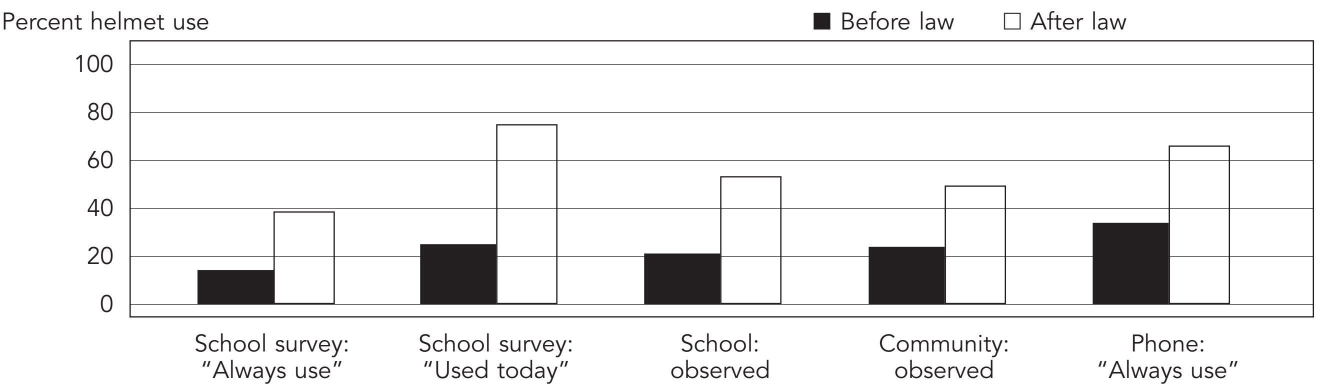
Responses concerning use are typically assigned to standard categories; we recommend the following use categories: "Always," "More than half," "About half," "Less than half," and "Never." No standard classification system exists for collapsing levels of helmet use responses during analysis. Therefore, different surveys have produced different results. For example, a 1994 telephone survey of adults reporting helmet use by their children indicated that 25% of children 5–14 years of age "always" wore their helmets, yet a similar 1998 telephone survey indicated that about 69% of children "generally" wore their helmets, defined as wearing a helmet "more than half the time," "nearly always," or "always."[1,6] We recommend the conservative approach, defining use as "always use" and defining non-use as all other categories. We accept the premise that self-reported use overestimates observed use, as described below.

## WHAT SURVEY DESIGNS HAVE BEEN USEFUL?

Three general approaches to surveys have been used: proxy reports by adults via mail or telephone, self-reports (e.g., show of hands, paper-and-pencil questionnaires), and direct observations; Table 1 details their advantages and disadvantages. Overall, we believe that observational surveys are the best method to measure the outcome of interest—namely, helmet use. Observational surveys are more accurate, since recall or social desirability bias are not present. However, several key problems exist, especially (*a*) their cross-sectional design; (*b*) their inability to identify or classify demographic and other personal characteristics of riders; (*c*) the difficulty in selecting an unbiased sample of sites to observe riders; and (*d*) the resources needed and logistical problems associated with conducting observations. An observed rider may use a helmet only intermittently, so a single observation may not accurately reflect habitual use. Demographic data may not be collected for some riders, or may be misclassified. For example, it may be difficult to correctly estimate the cyclist's age, given the variance in children's height, weight, and sexual maturation. Ideally, each rider would be positively identified by the observer, so that demographic data could be appended to the record later. If this were the case, "double-counting" could be avoided (Table 2) and before-after surveys would have the more robust design of a cohort, rather than a cross-sectional design. However, accurate identification is not always possible. For example, among 350 elementary school children riding in a small town, only 184 (53%) could be positively identified by name by local teachers or parents conducting the observations.[8] In more populous cities, the proportion of children correctly identified from a distance is likely to be even lower.

How does the type of survey affect the results? To help answer this, a multifaceted evaluation was conducted to determine the effectiveness of the 1994 Oregon bicycle helmet use law.[9] Components included (*a*) observing riders on arterial roads with high traffic volume; (*b*) observing students riding to or from one of 33 middle schools randomly selected across the state; (*c*) conducting a classroom show-of-hands in upper elementary and middle-schools on the day of the observational survey throughout the state to verify self-reported helmet use; and (*d*) conducting a random-digit-dial telephone survey of adults reporting their children's helmet use the last time they rode a bicycle. Prelaw helmet use ranged from 15% by classroom survey of "always use," to 37% by telephone survey (Figure 1). After the law, use ranged from 39% (classroom survey of "always use") to 76% (classroom

**Figure 1. Bicycle helmet use among Oregon children before and after a state law**




"School survey: Always use" and "School survey: Used today" are based on show-of-hands classroom surveys. "Phone: Always use" data are from Oregon Behavioral Risk Factor Surveillance System survey. From Reference 9.

**Table 1. Practical features of survey options of bicycle helmet use**

| *Feature* | *Telephone* | *Mail* | *Classroom: Show-of-Hands* | *Classroom: Paper-and-Pencil* | *Observations*[a] |
|---|---|---|---|---|---|
| *Sampling* | | | | | |
| Validity of sampling frame | Yes | Unknown | Yes | Yes | Unknown |
| Obtaining random sample | Easy | Easy | Easy | Easy | Difficult |
| Complete cross-section obtained | Possible | Possible | Yes | Yes | Unknown |
| *Data collection and survey* | | | | | |
| Existing instrument available? | Yes | Yes | Yes | Yes | Yes |
| Ability to query other variables | Good | Good | Fair | Fair | Poor |
| Time required for completion | Intermediate | Intermediate | Brief | Brief | Extended |
| Response rate | Intermediate | Low | High | High | High |
| Influence of weather | None | None | None | None | Substantial |
| Repeat surveys feasible | Doubtful | Doubtful | Yes | Yes | Yes |
| Informed consent needed? | Yes | Yes | Probably | Probably | No |
| Quality control of data collection | Good | Good | Fair | Fair | Fair |
| *Efficiency and costs* | | | | | |
| Efficiency: proportion of those queried who ride a bicycle | Low[b] | Low[b] | High | High | Depends on sites selected |
| Costs and tasks | Expensive: instrument development, interviewer salaries, telephone charges, analysis | Expensive: instrument development, postage, analysis | Moderate: project coordinator, analysis | Moderate: project coordinator, analysis | Moderate: project coordinator, observer salaries, analysis |
| Obtaining an inclusive, randomized, geographically appropriate telephone list | Expensive | N/A | N/A | N/A | N/A |
| Special training needed? | Yes | No | Yes | No | Yes |
| *Respondent factors* | | | | | |
| Response rate | Intermediate | Low[18] | High | High | High |
| Validity of proxy responses | Possible problem | Possible problem | N/A | N/A | N/A |
| Informed consent needed? | Yes | Yes | Often | Often | No |
| Ease of understanding | Good | Good | Unpredictable | Unpredictable | N/A |
| *Potential for bias* | | | | | |
| Observer bias | Intermediate | Low | Intermediate | Low | Intermediate |
| Self-report bias | High | High | High | High | None |
| Independent variable misclassification (e.g., age, gender) | Low | Low | None | None | High |
| Social desirability bias | Possibly high | Possibly high | High | High | None |
| Telephone ownership bias | High, esp. in lower-income groups[18] | N/A | N/A | N/A | N/A |
| Test-retest bias | Retests seldom done | Retests seldom done | High | High | None |
| Cluster effect | Possible | Possible | Possible | Possible | Possible |
| *Analysis* | | | | | |
| Level of difficulty | Difficult, if complex survey design is used | Difficult, if complex survey design is used | Usually simple | Usually simple | Usually simple |

[a]Observations held near schools may witness only a few students, or a particular stratum of students. Overall, children are more likely to wear a helmet to school than while riding in their community. A representative case-mix of sites (e.g., parks, streets) is difficult to obtain. Double-counting may occur at school and at neighborhood sites. Helmet use may be episodic. Age, gender, or race/ethnicity may be misclassified.

[b]Only 35% of families have children living at home, and some do not ride a bicycle.[19]

**Table 2. Special practical issues**

| | | |
|---|---|---|
| *1. Using solo versus paired observers* | | |
| | Solo observers: | More cost-efficient |
| | Paired observers: | May feel safer<br>Less boredom<br>May record large group of bicyclists more accurately |
| *2. Other issues related to observers* | | |
| | Training needed to learn the location and boundaries of observation site, correct use of instrument<br>Practice needed to accurately estimate rider age<br>Compensating observers is likely to reduce absenteeism on the day of the survey | |
| *3. Developing the instrument*[11,14,15,20] | | |
| | Key variables: helmet use, time of observation, estimated rider age (recorded as integer or age group)<br>Line listing preferred to hatch marks to enhance richness of analysis | |
| *4. Possible double-counting of the same rider* | | |
| | **Advantages:** | May functionally weight the results for duration of exposure.<br>Results in a conservative estimate by biasing results towards the null (no change in helmet use). |
| | **Disadvantages:** | Cycling enthusiasts tend to ride longer per outing, increasing their chance of being counted twice. They may also wear helmets more regularly than less-frequent bicyclists.<br>Violates assumptions of independence that underlie inferential statistics. |
| | **Possible solutions:** | Count rides, not riders.<br>Do not record second or subsequent observations during same 30-minute period.<br>Fixed observers should record bicyclists riding past them in only one, not both, directions.<br>Restrict the amount of time that anyone observes a particular site. |

self-reported use on the day of the survey). Even though these approaches yielded different absolute estimates of use, the methods yielded similar degrees of change from prelaw to postlaw use. Accordingly, we believe that some degree of overestimating helmet use by self-reported surveys is acceptable, provided that the same method is used to measure helmet use during subsequent surveys in the same population.

## WHERE SHOULD OBSERVATIONS BE CONDUCTED?

The most important aspect of survey design is site selection. The mix of sites needs to represent all riders in the target population, and individual sites need to be selected to yield high efficiency. The possibility of selection bias always exists. Increasing the sample size or conducting an elaborate statistical analysis cannot rectify problems that arise from selection bias.

Why is site selection so important? The population of riders in a community is neither evenly nor randomly distributed with respect to location, and not all possible riding locations in a community are known. Accordingly, the likelihood of observing a child at a particular site is not known. In epidemiologic terms, a probabilistic sample, in which the probability of observing each child can be determined, does not exist. Instead, for observational surveys, the sampling frame is assumed to render a sample of children who have an equal likelihood of being observed and who fairly represent all child riders in the target community.

However, this may not be the case. A study in Austin, Texas, that led to a city-wide intervention indicated the consequences of using different mixes of site selection for sampling. The city was divided into quadrants, and one high school was randomly selected from each quadrant. A mobile surveillance method was used to observe students from the elementary and middle schools associated with that high school, which included high- and low-income families (see later in this article, under How Should Observations Be Conducted?). Before intervention, helmet use was 9.8%; afterwards, it was 10.8%.[10] A second sample drawn from the same 16 elementary schools also included several parks, neighborhoods, and bicycle paths. In that sample, observed helmet use increased from 18% to 75% following the intervention.[11] The difference in base line values, as well as the difference in outcomes following the intervention, emphasizes the dual importance of initial site selection and restricting follow-up observations to those sites. We interpret these findings to indicate that helmet use of on-road riders was relatively low, whereas use in off-road sites was substantially higher.

Given that the true sampling frame is unknown, how should sites be selected? In practice, these decisions are based on convenience (proximity), accessibility, efficiency, and personal safety. When observer convenience and safety are paramount, the sampling frame is likely to include schools, neighborhoods near schools, or neighborhoods or parks near observers' homes. When time or cost are most important, efficiency—the attempt to maximize the number of riders per hour of observation—is critical. An efficient community sampling frame can be developed by asking community informants—such as mothers, employees with children, members of the local parks and recreation office, local police, school transportation coordinators, and state transportation office workers—to identify sites where children commonly ride, including specific school areas, neighborhoods, parks, and bicycle trails. In one study, this community informant approach resulted in two to four times more child riders observed per unit time and cost 2.9 to 7.0 times less than an approach based on bicycle club member opinions and use of maps to divide the city.[12]

Each type of observation site has advantages and disadvantages. Schools provide a well-defined sampling frame, since nearly all children go to school and all schools in a community are known. However, if low-income children are bussed to school, school-based observations may overrepresent more affluent children who live closer to school, ride bicycles more or less often, or have higher helmet use rates. In school districts that have adopted a helmet use school policy, school observations may indicate higher use than neighborhood observations.

Other sources of selection bias exist. Neighborhood samples may exclude low-income housing areas because of concerns about the personal safety of observers. This may overestimate helmet use, because low-income children wear helmets less often.[13] Results may be misleading if riders from a nonintervention community are counted unknowingly during a neighborhood observation. The ideal intervention sample should come entirely from the target population of those exposed to the intervention and not include anyone who was not exposed. To do so, either the entire population needs to receive the intervention (presently possible only for small communities), or the intervention community needs to be geographically isolated from other communities.

The use of one or more control groups is the classical epidemiologic method of dealing with threats to validity. In theory, a control group should be composed of individuals similar in age, gender, household income, and other demographic features as the intervention group, yet who were not exposed to the intervention. In our experience, the use of control groups has been problematic. More resources are needed to mount a survey in both the control and intervention communities. Finding a truly unexposed control population can be difficult, because broadcast media and even intervention programs may reach them. Some investigators seek to overcome this difficulty by surveying several control communities, but this increases the resources required. Two particularly difficult methodologic issues arise with the use of control groups. First, how does one determine that the intervention and control populations are comparable? Ideally, both demographics and base line helmet use should be similar in the intervention and control communities. However, even when community demographics are comparable, base line helmet use may differ. Substantial time and resources may be wasted trying to find a control community that has comparable demographics and base line helmet use. Community demographic data are generally available from local sources, but helmet use data generally require original surveys. Second, what adjustments should be made for differences in base line helmet use? Adjusting the base line and subsequent helmet use proportions by subtracting the difference in base line helmet use between the intervention and control communities from the community with the higher base line value may not rectify the problem. Base line helmet use itself is a mathematical determinant of the degree of change, such that a 10% change in use requires a greater proportion of people to change their behavior in a community where base line use is 50% (increase to 55%), compared with one where base line use is 20% (increase to only 22%). Attempts to standardize use rates by multiplying the degree of change of the lower group by the quotient of the higher and lower base line rates may not solve this problem either, because it may inflate the true change in the lesser group. Each age- and gender-specific change in the analysis would have to be treated similarly, which could compound these difficulties.

We have not found a satisfactory way to resolve these problems of control groups. Instead, we recommend that a modified time-series design be used to assess changes in community helmet use. It is more practical and directly informative, and it avoids the potential problems described. We believe that the use of control communities should be reserved for research purposes; for community public health practice, resources potentially spent on studying control groups should be used instead to provide interventions. Evaluation resources should focus on the target community, attempting to ascertain the degree of change accurately and ensuring that the outcome is a direct consequence of the intervention. This is best accomplished by conducting serial observations in the intervention community over several years, rather than relying on a single set of before–after observations in the intervention and control communities. When feasible, we recommend conducting observations during several separate time periods before an intervention begins; this helps to determine the best estimate of the base line value and to ascertain its stability.

## HOW MANY CHILDREN NEED TO BE OBSERVED?

Sample size calculations are normally used to determine the minimum number of observations needed to determine statistical significance. If we assume that initial helmet use is 25% and that a 15% change in helmet use (e.g., an increase to 40% helmet use) is required to reflect a meaningful difference with an alpha = 0.05 and power = 0.8, then 165 observations must be made using a one-tailed approach, or 203 observations, using a more conservative, two-tailed approach (PASS 6.0, NCSS, 329 North 1000 East, Kaysville, UT 84037).[6] These sample sizes are ordinarily sufficient to allow crude and stratified analysis. Enough children should be observed so that the stratified analysis is stable, meaning that a few riders wearing or not wearing helmets in that strata do not change the conclusions. In practice, this requires that each cell of the stratified analysis exceed 25 riders.

However, such sample size calculations are based on probabilistic sampling theory, in which the likelihood of selecting a rider is known, and they may not apply to sampling frames based on efficiency and convenience, as commonly used in this setting. Thus, statistics can be used as a guide, but not a mandate, of sample size. All other factors being equal, more observations taken during each time period, and more cycles of observations, are better than fewer ones. Results that are internally consistent are more likely to reflect stable estimates.

## HOW SHOULD OBSERVATIONS BE CONDUCTED?

Three general types of observation methods can be used: stationary observers, continuously mobile observers who record bicyclist behavior from inside a moving vehicle, and stationary observers who migrate periodically to a new site. Stationary observers, such as those watching through a window as children approach a school bicycle rack, wait for a child to ride into view. Stationary observers posted on streets generally see fewer children per hour than mobile observers. To offset this relative inefficiency, sometimes stationary observers are posted simultaneously at multiple sites—schools, parks, or bicycle trails—where rider density is high. However, higher rider density may be observed in a particular area because more bicycle enthusiasts ride there, and enthusiasts are known to be more likely than the general public to use helmets. In contradistinction, enthusiasts may be underrepresented in mobile observation surveys because trails, park paths and other off-road areas cannot be observed readily from the street. Migratory observation methods attempt to combine the best features of mobile and stationary methods, by posting observers at fixed sites for short periods, then having them rotate to the next predetermined fixed site. Even so, the ideal admixture of stationary and mobile sites needed to fairly represent a community is unknown. Furthermore, we do not know whether data should be weighted to adjust for either the time an observer spends at each site or the number of observations made per unit time, or simply pooled without weighting.

Differential observer bias may occur between methods. Mobile and stationary observers have recorded different results in the same target population, both in Minnesota and in New York (written communications, Mark Kinde, Minnesota Department of Health; Susan Hardman, New York State Department of Health). Although reasons for such discrepancies are not apparent, they underscore the importance of using the same methods each time a community is observed.

Several model protocols exist. Stationary adult observers in Seattle, Washington, and Portland, Oregon, unobtrusively recorded helmet use for 20 minutes while watching a school, playground, bicycle path, park, or street intersection.[14] Alternatively, using an efficient mobile technique, paid recorders drove through a section of a city along a spiral route centered around a randomly selected school in a city quadrant.[10] To record children riding for both transportation and recreation, observations were made on different days of the week and at various times of day. Observers used portable tape recorders to record observations quickly, then transcribe them later.

A successful migratory method was developed in New York State using school bus routing maps known to cover the entire school district.[15] A random sample of such bus routes was chosen, and observers were posted at bus boarding points to record helmet use by bicyclists riding at a time of day when children were out of school and not riding the bus. After 10 minutes, the recorders moved to the next bus stop along the route to observe there. Because all bus routes serve an approximately equal number of passengers, the sum of all observations should fairly represent the population density of school-age children and, presumably, the population density of bicycle riders, assuming that most children from those neighborhoods attend a public school.

In another migratory model, local bicycle club members designed routes from popular streets, parks, bike trails, and community swimming pools, mixed with residential streets determined only from maps.[16] Bicyclist volunteers stopped at predetermined points along each route for 10 minutes to conduct observations before moving to the next point to conduct more observations. This method effectively canvassed the entire length of the route for much of the time the team was working and at the same time afforded bicyclist-observers some recreation.

## PROTECTION OF HUMAN SUBJECTS IN RESEARCH

Because helmet use observations are most often made in the context of a publicly displayed behavior and without any personal contact by the investigator, recording such behavior does not generally require bicyclists' informed consent, nor does it involve issues of protecting the rights of subjects in a research project. However, informed consent is likely to be required if riders are asked questions face-to-face or by mail or telephone survey. The need for human subjects protection review also depends on whether data are used solely for program evaluation (less likely to require such review) or for generalizable research. Also, circumstances in which a control community is enrolled (usually for the purpose of research) but not provided with an intervention may have special human subjects protection considerations that should be brought before an institutional review board.

## WHEN SHOULD HELMET USE BE MEASURED?

Serial observations should be conducted at the same sites, at about the same time of day, under the same environmental (weather) conditions, and preferably by the same observers each time, because time and season may influence helmet use. Typically, helmet use is lower in the summer when helmets may be uncomfortably hot to wear. Helmet use may vary by day of the week, with lower use occurring during weekends when any existing school policy mandating helmet use is not in effect. Also, helmet use may vary by time of day, weather, or lighting conditions. Use may decline during a hot afternoon or at night when disproportionately more US teens ride, since they seldom use helmets. Also, use is likely to be highest just after a new program begins. It is not known for how long, or even if, the rate of helmet use in a population becomes stable after initial program delivery. We therefore suggest that helmet use be monitored for at least six to 12 months after a program ends, and that observations cover periods of high reinforcement (e.g., during the school year) and low reinforcement (e.g., summer), both on weekdays and weekends. Because relatively few people ride at night, any putative advantages of improving the sampling frame by observing at night is likely to be offset by lower efficiency.

In practice, a well-designed evaluation might include one or more observation periods before the intervention begins, followed by a set of observations several weeks after the intervention and every few months thereafter. If resources are limited, we suggest forgoing the immediate postprogram observation, because it probably overestimates long-term change in helmet use. Although assessment of short-term benefits may be conducted during the first six months of a program, long-term benefits may require several years to become apparent.

## CONCLUSION

Efforts to monitor progress toward achieving helmet use goals need to continue, and should be as accurate and reliable as possible. The techniques described here are intended to standardize the measurements needed, but validity and reliability issues remain. Future methodologic work can help improve our measurements and might help us learn how best to evaluate interventions for other types of injuries. Lessons learned through this example can be applied to evaluation of other public health issues, such as walk-to-school programs, use of personal flotation devices, and others in which the key outcome is a publicly displayed behavior. It may be particularly beneficial in those circumstances in which the incidence or prevalence of the health outcome is low (making surveillance relatively expensive), but in which an observable proximate measure of effectiveness can be identified and counted. A desirable prerequisite for this approach is that the relationship between the countermeasure and the outcome of interest (i.e., the etiologic fraction) is known, so that it is possible to estimate how many injuries can be prevented for each incremental increase in countermeasure use. In the final analysis, monitoring community bicycle helmet use remains a combination of art and science.[17]

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