Evaluating the Safety Effects of Bicycle Lanes in New York City

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Objectives. We evaluated the effects of on-street bicycle lanes installed prior to 2007 on different categories of crashes (total crashes, bicyclist crashes, pedestrian crashes, multiple-vehicle crashes, and injurious or fatal crashes) occurring on roadway segments and at intersections in New York City.

Methods. We used generalized estimating equation methodology to compare changes in police-reported crashes in a treatment group and a comparison group before and after installation of bicycle lanes. Our study approach allowed us to control confounding factors, such as built environment characteristics, that cannot typically be controlled when a comparison group is used.

Results. Installation of bicycle lanes did not lead to an increase in crashes, despite the probable increase in the number of bicyclists. The most likely explanations for the lack of increase in crashes are reduced vehicular speeds and fewer conflicts between vehicles and bicyclists after installation of these lanes.

Conclusions. Our results indicate that characteristics of the built environment have a direct impact on crashes and that they should thus be controlled in studies evaluating traffic countermeasures such as bicycle lanes. To prevent crashes at intersections, we recommend installation of ''bike boxes'' and markings that indicate the path of bicycle lanes across intersections. (*Am J Public Health.* 2012;102:1120–1127. doi:10.2105/AJPH.2011.300319)

Bicycling is a healthy, environmentally friendly alternative to automobile use.[1–3] Yet, in the United States bicycling is primarily considered a recreational pursuit rather than a means of utilitarian travel. Among the nearly 140 million commuting trips made every day, slightly less than 0.5% are made by bicycle.[4] Of trips for all purposes in the United States, only 1% are made by bicycle.[5] Approximately 25% of all trips made are less than 1 mile, and 75% of these short trips are made by automobile.[6] If some of these short trips were made by active modes such as walking or cycling, more people would reach the recommended 150 minutes of moderate-intensity physical activity per week (at present, fewer than 5% of adults engage in this amount of physical activity[7,8]). Integrating physical activity into daily routines such as bicycling to work[9] would also lead to sustained increases in habitual physical activity.[7]

The health and environmental benefits of cycling are clear and significant. However, bicyclists are vulnerable in that they share the same roadway with motorized vehicles. At intersections, they must maneuver their way through conflicting vehicular movements if they need to make a turn. Indeed, safety is a major concern that discourages people from bicycling.[10,11] When a crash occurs, bicyclists are much more likely than motor vehicle users to sustain an injury, and the injury is likely to be more severe. Therefore, there is a need to gain a full understanding of the factors associated with cycling safety, particularly because many American cities are installing extensive bicycle lane networks to encourage the use of cycling for commutes.[12]

Studies of the safety effects of bicycle lanes in the United States date back to the 1970s. Some of the early studies, based on self-reported data from surveys of bicyclists or police reports, compared bicycle crash rates on different types of roadways such as roads with or without marked bicycle lanes and off-road trails. These studies reported lower bicycle crash rates on roads with bicycle lanes than on roads without such lanes.[13–17] However, causality cannot be inferred from these neighborhood-level studies because of confounding factors. Results from studies conducted at the roadway segment or intersection level have been mixed.[18–20]

A major limitation of the existing studies is their lack of a rigorous quasi-experimental design that included a treatment group and a comparison group and that compared crashes in these groups before and after the installation of bicycle lanes.[21] A report published by the Transportation Research Board and the Institute of Medicine described the existing literature on the built environment and physical activity as follows:

> [M]ost of the studies conducted to date have been cross sectional. Longitudinal study designs using time-series data are also needed to investigate causal relationships between the built environment and physical activity.[22(p7)]

The report went on to state that

> [w]hen changes are made to the built environment—whether retrofitting existing environments or constructing new developments or communities—researchers should view such natural experiments as 'demonstration' projects and analyze their impacts on physical activity.[22(p229)]

The same limitations apply to later studies evaluating the impact of the installation of bicycle lanes on safety. In a before–after study of bicycle lanes on arterial roads in Madison, Wisconsin, Smith found an increase in bicyclist crashes on the 2 roads with bicycle lanes; however, the increase was insignificant relative to the increase in city-wide bicyclist crashes observed.[18] Increases in crashes were also found in a before–after study of bicycle lanes in Oxford, England.[19] To our knowledge, only 1 before–after study involved the use of both a treatment group and a comparison group to evaluate the safety impact of bicycle lanes.[20] This study, which focused on bicycle lanes installed in Copenhagen, Denmark, between 1988 and 2002, revealed increases in most types of crashes and injuries on roadway segments and at intersections with bicycle lanes; however, none of these increases were significant at the 5% level.

Using a quasi-experimental design that included a treatment group and a comparison group, we conducted a before–after analysis of 43 miles of bicycle lanes installed in the 5 boroughs of New York City from 1996 through 2006. The city's 5 boroughs vary greatly in built environment characteristics,[23] and this large variation helps strengthen the validity of our model results. We used generalized estimating equation (GEE) methodology[24] to account for correlations within repeated observations and to control factors (e.g., built environment factors) that could not be controlled through the use of a comparison group.

METHODS

We used a 2-stage design. In the first stage, we identified a comparison group comprising locations without bicycle lanes but with segment- or intersection-level characteristics comparable to those of the treatment group. The treatment group consisted of roadway segments in New York City where on-street bicycle lanes (not protected by a parking lane) had been installed from 1996 through 2006 (a total length of about 43 miles on 61 streets). Data on the dates during which bicycle lanes were installed and the locations of the bicycle lanes were available for each segment.

The dependent variable was police-reported crashes occurring on a roadway segment (a continuous section of roadway uninterrupted by a cross road or an intersection) or at an intersection. We distinguished among 5 categories of crashes: total crashes, multiple-vehicle crashes (crashes involving multiple vehicles but no bicyclists or pedestrians), bicyclist crashes (e.g., vehicle–bicycle collisions), pedestrian crashes (vehicle–pedestrian collisions), and injurious or fatal crashes (crashes that caused at least one injury or fatality).

For each category of the crashes described above, we calculated 2 categories of crashes for each segment or intersection: crashes within the 5-year period before the installation of bicycle lanes and crashes within the 2-year period after installation. Because a crash is a relatively rare event, use of the 5-year period before installation allowed us to capture more stable trends. Conversely, our use of a shorter period after installation allowed us to include more treatment group sites, because crash data were available only up to 2008; thus, use of a 5-year post-installation period would not have allowed us to evaluate bicycle lanes installed in 2003 or thereafter. We used an offset variable to control for differences in the length of the before and after periods.

In the second stage, we used GEE methodology to apply Poisson and negative binomial regression models to the data set consisting of observations in the treatment group and the comparison group before and after the installation of bicycle lanes. We evaluated the safety-related effects of bicycle lanes in the treatment group via the coefficients estimated from the models.

Controls

We examined crashes on roadway segments and crashes at intersections separately because of their distinct natures. Intersections are high-risk locations for bicycle–vehicle collisions as a result of conflicts between bicyclists and motor vehicle users.[25] For this reason, we divided our comparison group into 2 subgroups: a segment-level subgroup and an intersection-level subgroup.

Our selection of the segment-level subgroup was based on 3 segment-level factors that have been found to have a significant impact on crashes: 1-way versus 2-way roads,[26] divided versus undivided roadways (if they are 2-way roadways),[13,27] and number of travel lanes.[28,29] Table 1 shows a comparison between the treatment group and the comparison group with regard to these characteristics. We further controlled the geographic distribution of the comparison group locations to resemble the distribution in the treatment group.

Because bicycle lanes were installed over a period of more than 10 years (from 1996 through 2006), the treatment group comprised bicycle lane segments installed in different years. In other words, the before period and the after period for different bicycle lane segments were different, although they were of the same length. As an example, the 5-year before period and the 2-year after period for bicycle lanes installed in 2000 were 1995 to 1999 and 2001 to 2002, respectively.

As a result of these differences, the treatment group was divided into multiple subsets defined by the year of installation. For each subset, we selected a set of untreated locations by applying frequency-matching techniques to resemble the joint distribution of segment-level variables and the geographic distribution of the treatment group. After we identified each subset of the treatment group with a corresponding set of locations without bicycle lanes, we combined these untreated locations into the segment-level comparison subgroup.

Many of the bicycle lane segments in the treatment group were part of long corridors, whereas those in the comparison group were more likely to be scattered around the city. Therefore, we manually selected roadway segments that were parallel to those in the treatment group and added them to the comparison group. These procedures resulted in a segment-level subgroup of 1926 segments, corresponding to 579 bicycle lane segments.

From the segment-level comparison subgroup, we identified a second, intersection-level subgroup. We used control type (signalized or not)[30] and the number of roadway segments (arms) at the intersection,[27,28] both of which have been found to have a significant effect on crashes at intersections, to select the intersection-level subgroup. A comparison of these attributes is shown in Table 1. We applied the same set of procedures just described to generate this comparison subgroup, which comprised 1653 intersections, corresponding to 578 intersections in the treatment group.

Model

We combined the identified segment- and intersection-level subgroups with the corresponding treatment group to generate 2 data sets, one for segment-level crashes and the other for intersection-level crashes. We then used the GEE method to apply Poisson or negative binomial models to these data sets. We used the Wald test to determine whether there was overdispersion in the crash data. If the crash data were overdispersed, we used the negative binomial model; otherwise, we applied the Poisson model. For the dependent variable (number of crashes during a period), each segment or intersection had 2 measures: crashes during the 5-year period before the installation of bicycle lanes and crashes during the 2-year period after the installation. The GEE method was applied to control correlations within repeated measures for crashes at a location in the before and after periods.

TABLE 1—Matched Characteristics of Locations in the Treatment Group and the Comparison Group: New York City, 1996–2006

	Treatment Group, No. (%)	Comparison Group, No. (%)
	Segment characteristics	
Segments by borough		
Manhattan	80 (13.8)	269 (14.0)
Bronx	91 (15.7)	285 (14.8)
Brooklyn	273 (47.2)	940 (48.8)
Queens	135 (23.3)	432 (22.4)
Staten Island	0 (0.0)	0 (0.0)
Type of roadway		
1-way	288 (49.7)	979 (50.8)
2-way	291 (50.3)	947 (49.2)
Divided roadway		
No	502 (86.7)	1702 (88.4)
Yes	77 (13.3)	224 (11.6)
No. of travel lanes		
1	317 (54.7)	1067 (55.4)
2	212 (36.6)	689 (35.8)
3	14 (2.4)	47 (2.4)
4	30 (5.2)	101 (5.2)
≥5	6 (1.0)	22 (1.1)
Total	579 (100)	1926 (100)
	Intersection characteristics	
Intersections by borough		
Manhattan	97 (16.8)	236 (14.3)
Bronx	95 (16.4)	221 (13.4)
Brooklyn	278 (48.1)	852 (51.5)
Queens	108 (18.7)	344 (20.8)
Staten Island	0 (0.0)	0 (0.0)
Control type		
Signalized	349 (60.4)	965 (58.4)
All-way stop	15 (2.6)	50 (3.0)
Stop on minor road	107 (18.5)	332 (20.1)
No control	107 (18.5)	306 (18.5)
No. of arms		
3	148 (25.6)	399 (24.1)
4	415 (71.8)	1218 (73.7)
≥5	15 (2.6)	36 (2.2)
Total	578 (100)	1653 (100)

Note. The treatment group consisted of roadway segments in New York City where bicycle lanes had been installed from 1996 through 2006. The comparison group comprised locations without bicycle lanes but with segment- or intersection-level characteristics comparable to those of the treatment group. The sum of the percentages for the different number of travel lanes in the comparison group and the treatment group is 99.9 because of rounding.

We included 2 dummy variables in the model: one denoting the crash changes from the before period to the after period in the treatment group and the other denoting the crash changes in the comparison group. (Full model specification is available in Appendices A and B, available as a supplement to the online version of this article at http://www.ajph.org.) The 2 coefficients associated with the 2 dummy variables for the treatment group (a) and the comparison group (b) were of primary interest. The contrast between the 2 coefficients ($a - b$) represented the difference in crashes from the before period to the after period for the treatment group versus the comparison group.

It has been hypothesized in previous research that higher levels of exposure (for example, more vehicle traffic, pedestrians, and bicyclists) and more conflicts (more conflicting movements between different road users) are associated with a higher number of crashes.[31] Therefore, we entered a set of neighborhood-level and site-level (segment or intersection level) variables in the model to control exposure and conflicts. At the neighborhood level, we used daytime population density, retail density, and bicycle trip density to account for vehicular and bicyclist traffic exposures. We calculated daytime population density as the number of residents who live in a census tract plus the number of people who work in the census tract but live elsewhere, divided by the total census tract area. We calculated retail density as retail land use area divided by total census tract area. Finally, we calculated bicycle trip density as number of bicycle commuters divided by total census tract road length.

Site-level covariates included the presence of bus stops or parking on road segments, whether the segment was on a truck route, control type (signalized or not), and the number of arms at the intersection. These variables were included to account for conflicts between bicyclists and motorized vehicles.

In addition, we included an offset variable in the model—the number of years during which crash counts were collected—to account for differences between the 5-year before period and the 2-year after period. The coefficient of the offset variable was restricted to 1, under the assumption that crash counts would be proportional to the length of the before and after periods. To account for the difference in crashes between different groups during the before period, we also added 2 dummy variables to the model to alleviate the potential regression-to-mean effect, according to which locations with more crashes during the before period would be more likely to exhibit a reduction in crashes than those with fewer crashes in the before period.

RESULTS

Table 2 shows the number of crashes in the 5-year before period and 2-year after period

TABLE 2—Roadway Crashes Before and After Installation of Bicycle Lanes, by Group: New York City, 1996–2006

	Before Period (5 Years)		After Period (2 Years)		
Crash Type	Total	Average[a]	Total	Average[a]	Change,[b] %
Crashes on segments					
Total					
Treatment group	827	0.2857	209	0.1805	–36.8
Comparison group	2164	0.2247	537	0.1394	–38.0
Vehicle crashes					
Treatment group	559	0.1931	137	0.1183	–38.7
Comparison group	1511	0.1569	367	0.0953	–39.3
Pedestrian crashes					
Treatment group	175	0.0604	43	0.0371	–38.6
Comparison group	446	0.0463	118	0.0306	–33.9
Bicycle crashes					
Treatment group	47	0.0162	19	0.0164	1.2
Comparison group	112	0.0116	25	0.0065	–44.0
Injurious or fatal crashes					
Treatment group	612	0.2114	153	0.1321	–37.5
Comparison group	1504	0.1562	363	0.0942	–39.7
Crashes at intersections					
Total					
Treatment group	4577	1.5837	1494	1.2924	–18.4
Comparison group	13 450	1.6273	4124	1.2474	–23.3
Vehicle crashes					
Treatment group	3358	1.1619	969	0.8382	–27.9
Comparison group	10 199	1.234	2925	0.8848	–28.3
Pedestrian crashes					
Treatment group	767	0.2654	333	0.2881	8.6
Comparison group	2213	0.2678	843	0.2550	–4.8
Bicycle crashes					
Treatment group	317	0.1097	155	0.1341	22.2
Comparison group	680	0.0823	244	0.0738	–10.3
Injurious or fatal crashes					
Treatment group	3748	1.2969	1196	1.0346	–20.2
Comparison group	10 861	1.3174	3215	0.9725	–26.2

Note. The treatment group consisted of roadway segments in New York City where bicycle lanes had been installed from 1996–2006. The comparison group comprised locations without bicycle lanes but with segment- or intersection-level characteristics comparable to those of the treatment group.
[a]Average number of crashes per location per year.
[b]Change in average number of crashes.

for the treatment and comparison groups. At the segment level, total crashes, multiple-vehicle crashes, pedestrian crashes, and injurious and fatal crashes all decreased in both groups. There was a slight increase (1.2%) in bicyclist crashes in the treatment group and a decrease in the comparison group. At the intersection level, total crashes, multiple-vehicle crashes, and injurious crashes decreased in both groups. However, pedestrian and bicyclist crashes increased in the treatment group and decreased in the comparison group. The increases observed were likely due to higher exposure levels as bicyclists took advantage of the new bicycle lanes. We did not control for before–after differences in bicyclist and pedestrian volumes because these data were not available.

The abbreviated model results for segment- and intersection-level crashes are shown in Table 3. We used negative binomial models for total crashes, multiple-vehicle crashes, pedestrian crashes, and injurious and fatal crashes because we detected overdispersion in these crash types. For bicycle crashes, no overdispersion was detected, so we used Poisson models. (Full model results are available in Appendices A and B, available as a supplement to the online version of this article at http://www.ajph.org.)

Table 3 shows the effects of bicycle lanes on segment-level crashes. The difference between *a* and *b* was negative for total crashes, multiple-vehicle crashes, pedestrian crashes, and injurious and fatal crashes; at the segment level, crashes decreased more in the treatment group than in the comparison group. For bicyclist crashes, the difference between *a* and *b* was positive, suggesting an increase in bicyclist crashes in the treatment group after the installation of bicycle lanes. However, the increase was not significant at the 5% level.

Table 3 also shows the effects of bicycle lanes on intersection-level crashes. The difference between *a* and *b* was positive for all 5 crash types, suggesting increases in crashes of these types in the treatment group after the installation of bicycle lanes. Again, none of these increases were significant at the 5% level.

Few existing studies evaluating the impact of bicycle lanes on safety have included built environment attributes. The estimated models (Tables A4 and A5, available as supplements to the online version of this article at http://www.ajph.org) show that most of the neighborhood-level variables were significant at the 5% level. We calculated elasticities (the percentage change in crashes in response to a 1% increase in a given attribute) to provide a better understanding of the roles that these built environment characteristics play in crashes (Table 4). An elasticity of 1 indicates that in response to a 1% increase in a particular attribute, the percentage change in crashes is exactly 1%.

Daytime population density had the largest effect on segment- and intersection-level crashes (in particular, pedestrian crashes). Every 1% increase in daytime population density was associated with a 0.738% increase in

TABLE 3—Estimates of Effects for 5 Types of Crashes on Road Segments and at Intersections: New York City, 1996–2006

	Total Crashes	Vehicle Crashes	Pedestrian Crashes	Bicycle Crashes	Injurious or Fatal Crashes
Crashes on segments					
Dispersion parameter[a]	1.385* (0.079)	1.601* (0.104)	1.543* (0.197)		1.398* (0.095)
Treatment group, T1[b] (*a*)	−0.464* (0.083)	−0.484* (0.107)	−0.500* (0.205)	0.011 (0.263)	−0.475* (0.096)
Comparison group, T1[c] (*b*)	−0.407* (0.064)	−0.426* (0.081)	−0.343* (0.119)	−0.401* (0.235)	−0.420* (0.071)
a – *b*, estimate (SE; 95% CI)	−0.057 (0.106; −0.265, 0.150)	−0.058 (0.134; −0.321, 0.205)	−0.157 (0.236; −0.619, 0.369)	0.412 (0.352; −0.279, 1.102)	−0.056 (0.120; −0.290, 0.178)
% change in crashes, estimate (SE; 95% CI)	−5.6 (10.1; −25.4, 14.2)	−5.6 (12.8; −30.7, 19.5)	−14.5 (21.0; −55.7, 26.8)	50.9 (58.3; −63.4, 165.2)	−5.4 (11.4; −27.8, 17.0)
Crashes at intersections					
Dispersion parameter[a]	0.986* (0.030)	1.080* (0.035)	0.975* (0.057)		1.010* (0.033)
Treatment group, T1[b] (*a*)	−0.209* (0.040)	−0.339* (0.047)	0.090 (0.072)	0.201 (0.107)	−0.218* (0.044)
Comparison group, T1[c] (*b*)	−0.264* (0.030)	−0.346* (0.034)	0.027 (0.050)	−0.047 (0.083)	−0.286* (0.032)
a – *b*, estimate (SE; 95% CI)	0.055 (0.050; −0.042, 0.153)	0.007 (0.058; −0.105, 0.120)	0.063 (0.088; −0.108, 0.235)	0.248 (0.135; −0.016, 0.514)	0.068 (0.055; −0.039, 0.175)
% change in crashes, estimate (SE; 95% CI)	5.7 (5.3; −4.7, 16.1)	0.7 (5.9; −10.7, 12.2)	6.5 (9.4; −12.0, 20.0)	28.1 (17.5; −6.3, 62.4)	7.0 (5.9; −4.5, 18.5)

Note. The treatment group consisted of roadway segments in New York City where bicycle lanes had been installed from 1996 through 2006. The comparison group comprised locations without bicycle lanes but with segment- or intersection-level characteristics comparable to those of the treatment group.
[a]Dispersion parameter is not applicable for bicycle crashes because bicycle crashes data were not overdispersed, and thus they were modeled by Poisson regression instead of negative binomial.
[b]Dummy variable: 1 if the data point comes from the treatment group in the after period and 0 otherwise; *a* is the coefficient of this dummy variable
[c]Dummy variable: 1 if the data point comes from the comparison group in the after period and 0 otherwise; *b* is the coefficient of this dummy variable
*$P < .05$.

segment-level pedestrian crashes and a 0.474% increase in intersection-level pedestrian crashes. These elasticities should be interpreted with caution, given that we were not able to control before–after differences in bicyclist and pedestrian volumes; density may increase bicyclist and pedestrian volumes much faster than it increases the number of crashes.

Retail density and bicyclist trip density appeared to exert the smallest effect. Elasticities for both were less than 0.1%, and the effect of retail density on intersection-level crashes was negligible. The effect of the density of bus stops was larger for intersection-level crashes (with most hovering around 0.2%) than for segment-level crashes (which were mostly much smaller than 0.2%). Subway ridership did not appear to have a large effect; it was not significant for intersection-level crashes, and for segment-level crashes the calculated elasticities were mostly around 0.02%. One neighborhood sociodemographic characteristic, percentage of residents living below the poverty level, was significant for intersection-level crashes; the number of crashes appeared to increase as the percentage of the population living below the poverty level increased.

DISCUSSION

Our results indicate that the installation of bicycle lanes does not lead to an increase in crashes despite the likely increase in the number of bicyclists after the addition of such lanes. In fact, all crash types on segments where there are treatments (except with bicyclists) decreased. We did not control for differences in bicyclist volumes before and after the installation of bicycle lanes because these data were not generally available. Existing literature shows a positive association between the presence of bicycle lanes and bicycle volumes.[11,32–35] Based on New York City's Commuter Cycling Indicator (drawn from daytime bicycle volumes entering the city's central business district), bicycle volume increased during the study period, with a 51% increase from 1996 to 2006 and a 48% increase from 2006 to 2008 (the last years of the "after" period). This increase occurred at the same time as an expansion of the bicycle lane network, supporting the conclusion that bicycle volumes increased on these new bicycle lanes during this period.

TABLE 4—Elasticities of Neighborhood-Level Covariates for Crashes on Road Segments and at Intersections: New York City, 1996–2006

Neighborhood-Level Covariate	Covariate Value	Total Crashes	Vehicle Crashes	Pedestrian Crashes	Bicycle Crashes	Injurious or Fatal Crashes
Crashes on segments						
Daytime population density[a]		0.284	0.236	0.738	0.331	0.316
Average	52.517					
25th percentile	22.498					
50th percentile	38.437					
75th percentile	57.924					
Retail density						
Average	5.762	0.069	0.063			0.040
25th percentile	1.185	0.014	0.013			0.008
50th percentile	3.213	0.039	0.035			0.022
75th percentile	6.202	0.074	0.068			0.043
Bus stop density						
Average	83.892	0.159	0.143	0.101	0.025	0.168
25th percentile	47.143	0.090	0.080	0.057	0.014	0.094
50th percentile	80.386	0.153	0.137	0.096	0.024	0.161
75th percentile	115.028	0.219	0.196	0.138	0.035	0.230
Average subway ridership	2.698			0.027	0.019	
Average road bicycle trip density	3.261			0.026	0.062	
Crashes at intersections						
Daytime population density[a]		0.232	0.134	0.474	0.210	0.201
Average	61.478					
25th percentile	28.843					
50th percentile	44.831					
75th percentile	68.167					
Percentage below poverty level						
Average	0.267		0.116	0.144	0.124	0.175
25th percentile	0.122		0.053	0.066	0.057	0.080
50th percentile	0.244		0.106	0.131	0.114	0.160
75th percentile	0.382		0.166	0.205	0.177	0.250
Retail density						
Average	7.684				0.008	
25th percentile	1.921				0.002	
50th percentile	4.281				0.004	
75th percentile	8.368				0.008	
Bus stop density						
Average	97.062	0.165	0.116	0.223	0.233	0.175
25th percentile	60.533	0.103	0.073	0.139	0.145	0.109
50th percentile	91.771	0.156	0.110	0.211	0.220	0.165
75th percentile	127.898	0.217	0.153	0.294	0.307	0.230
Average road bicycle trip density	4.395			0.040	0.088	

[a]Independent of the value of this variable based on the model specification. Empty cells for some covariates indicate that the covariate is not in the model for the specific crash type.

In other words, if we could have properly controlled for differences in bicyclist volumes, we might have observed a significant reduction in crashes in the treatment group. This also points to the need to collect before-and-after bicycle volume data not only for the treatment group but also for the comparison group.

There are a number of possible reasons why we did not observe significant increases in crashes after the installation of bicycle lanes even though it is likely that bicyclist volumes increased significantly. Two primary possibilities are reduced vehicle speeds because of an increased awareness of bicyclists or lane narrowing and reduced conflicts because of the separation of vehicles and bicyclists.

Crashes at intersections appeared to increase, although not significantly; in interpreting this increase, it is important to note that the bicycle lanes included in this study were not designed as intersection safety treatments and generally did not involve design changes within intersections. For example, bicycle lanes discontinue at intersections and there are no lane markings at intersections that can guide bicyclists. We recommend 2 courses of action to increase the safety of bicycling at intersections. One is a "bike box," an area reserved for bicyclists to wait at a red light ahead of vehicular traffic. The bike box is defined by a second stop line painted on the road approximately 10 to 15 feet in front of the stop line for cars.[36] When bicyclists encounter a red light at an intersection that has a bike box, they can wait between the two stop lines, in front of the cars in their lane of traffic. This increases the visibility of bicyclists stopping at red lights and allows them to clear the intersection before vehicular traffic does, thus reducing conflicts.[37] Our other recommendation is that markings indicating the path of the bicycle lane across the intersection or other intersection treatments be added at intersections to reduce conflicts.[38–40]

Our results indicate that characteristics of the built environment should be included in safety studies. Built environment attributes have been largely excluded in existing studies assessing the effects of bicycle lanes.[18–20] The significance of these variables in our models indicates that the mere use of a comparison group is often not sufficient to ensure between-group similarity.

Our 2-stage approach offers a number of advantages over Jensen's study,[20] seemingly the only previous study assessing crashes in both a treatment group and a comparison group before and after installation of bicycle lanes. First, when a potential confounding factor is continuous, it is typically converted into a categorical variable for frequency matching when a comparison group is selected, and such conversion is often arbitrary. Second, although all potential confounding factors can be applied in the selection of the comparison group, this process usually results in a sample that is too small to allow useful evaluations. Finally, if confounding factors are used in selecting a comparison group, the effects of these factors can no longer be quantified. The second-stage regression models we applied controlled for factors that could not be controlled when selecting a comparison group, quantified their effects on crashes, and accounted for repeated measures.

In summary, our study, involving a rigorous quasi-experimental design, shows that installation of bicycle lanes does not lead to an increase in crashes, even with the likelihood of a greatly increased number of bicyclists using these lanes. To improve bicyclists' safety at intersections, we recommend the installation of bike boxes and markings that indicate the path of bicycle lanes across intersections, two features of more recently designed bicycle lanes in New York City and other cities. Our results also demonstrate the importance of controlling characteristics of the built environment in safety evaluation studies. ■

About the Authors

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This article was accepted June 2, 2011.

Contributors

L. Chen collected data, designed the study, conducted the analysis, and wrote the article. C. Chen designed the study, interpreted the results, and wrote the article. R. Srinivasan assisted with data analysis and reviewed the article. C.E. McKnight assisted with literature review and reviewed the article. R. Ewing participated in the literature review and reviewed the article. M. Roe assisted with data collection and reviewed the study results.

Acknowledgments

The research reported in this article was funded by the New York City Department of Transportation.

We thank Seth Berman, Anne Marie Doherty, and Gerard Soffian for their support throughout the project.

Note. The opinions expressed in the article do not reflect those of the New York City Department of Transportation. The authors are solely responsible for any errors that remain.

Human Participant Protection

No protocol approval was needed because this study involved aggregate crash data examined at the segment and intersection levels.

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