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EVALUATION OF A DDC/CITATION-DISMISSAL PROGRAM IN NORTH CAROLINA

Final Report

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Introduction

The Defensive Driver Course/Citation Dismissal Program began in Mecklenburg County, North Carolina on July 1, 1989 as a joint effort between the court system and the Safety and Health Council of North Carolina, a non-profit safety educational association and statewide chapter of the National Safety Council. Based on information from the Safety and Health Council, the goal of this program is to reduce traffic crashes by providing the minor traffic violator with an educational experience to improve their attitudes toward safe driving while they learn better driving habits.

Under this program, drivers who are stopped by traffic officers and are cited for 1 of 46 different minor violations are eligible to complete the National Safety Council's four-hour defensive driving course. Upon successful completion, the charge will be dismissed and no court appearance is required, no fine is levied, and no driver's license points (or record of any conviction) is entered into the driver's record. The opportunity to participate in the educational course is voluntary on the part of the defendant in that they could either choose to go to the course or to proceed through the normal court system. To prohibit repeat offenders, the school option would not be available to the same driver but once every three years.

Approximately 24,000 motorists in Mecklenburg County and 12,000 motorists in five other participating N.C. judicial districts have elected to take this Defensive Driving Course in the first year -- July 1, 1989 to July 1, 1990.

The school program uses a curriculum developed by the National Safety Council known as the "Defensive Driving Course" (DDC). Courses given to all participants are standardized through use of a teaching manual developed by the National Safety Council, the instructors are trained and qualified by the NSC staff, and thus the program provided in all locations is quite similar.

While one of the goals of the program was a reduction in the minor traffic violation-related load on the judicial system in these counties, the major emphasis for implementing this program was the desire to provide additional traffic safety education to drivers who had been involved in some violation-related behavior. Because the program was only instituted in a relatively small area of the State, questions later arose in the NC Legislature concerning the desirability of expanding it statewide. This led to formation of a Legislative Task Force which was to develop information on and recommendations

concerning the program. After discussions concerning the benefits of the program, it was decided by the Task Force that a study of the program effectiveness in terms of changes in subsequent violations and accidents was needed. The UNC Highway Safety Research Center was requested by this Task Force and the Governor's Highway Safety Program to conduct such an evaluation. This report entails the details of that evaluation and the results.

The narrative below will provide a brief review of the literature concerning driver improvement programs (particularly DDC-type courses), an overview and details of the study methodology, the results of the analyses, and a discussion of the findings and possible future evaluative efforts.

Review of the Literature

Driver improvement programs of one type or another have been in existence for at least 25 years and have included a variety of activities ranging from warning letters or pre-suspension hearings through state licensing agencies to driver improvement clinics offered to drivers who have been cited for violations. As stated in Struckman-Johnson, et. al (1989):

> The general premise of these activities is that lack of knowledge about safe driving and/or inappropriate attitudes are responsible for motor vehicle crashes. Thus, the driver improvement activity is designed to increase knowledge and modify attitudes. It is assumed that these increases in knowledge and changes in attitudes cause some positive change in the behavior of the participants that is reflected in reduced crash experience, It is also assumed that the same behavior that causes crashes is evidenced in traffic violations, and that violations will also be reduced.

While numerous examples of evaluations of these programs have appeared in the literature, the results have not agreed with any certainty as to program effectiveness in reducing violations and crashes. In general, subsequent violations were lowered while crashes were not affected. In an effort to synthesize the findings of this body of literature and to explore possible reasons for the lack of agreement, Struckman-Johnson, et. al (1989) conducted a detailed critical review of the existing literature to 1) determine the generality of previous findings that driver improvement programs affect violations but not crashes, and 2) determine if previous studies provide support for possible explanations of differential effects on crashes and

violations. The possible explanations addressed are that a) since crashes are relatively rare events, the ability of an evaluation to detect statistically significant effects on crashes depends on having adequate sample sizes; b) crash involvement is less dependent on driver behavior (due to behavior of other drivers and environmental elements) than violations and therefore behavior modification is more closely tied to violations than crashes; and c) that driver improvement programs are ineffective in modifying driver behavior and effects on violations are a result of participants learning to manipulate the traffic safety system by repeatedly enrolling in the programs to have violations removed from their records.

The authors undertook a comprehensive review of driver improvement program evaluations by contacting the governor's representatives in all 50 states to obtain locally sponsored evaluations as well as conducting a traditional literature search. They identified a total of approximately 65 studies that evaluated the effectiveness of one or more programs. Each of the studies was then reviewed to determine its methodological adequacy. Primary emphasis in the rating of methodology was placed on the presence of a non-treated control or comparison group, with highest ratings given to evaluations characterized by random assignment to experimental and control groups (i.e., evaluations in which a pool of potential subjects was randomly divided into experimental and control groups prior to the implementation of the treatment.)

The importance of defining methodological adequacy lies in the ultimate strength of the findings. Studies with poor methodology result in measured findings, but the effects shown are often not the result of the treatment, but of other causes such as underlying trends, other factors occurring at the same time as the treatment, and regression-to-the-mean (a known phenomenon in which subjects chosen due to "poor" records in one time period will "improve" in the next time period even in the absence of treatment). Unfortunately, in the past, the driver improvement/education field has been characterized to a troubling extent by such studies.

Of interest in this regard is a study conducted by the State of New York to evaluate their driver improvement program (1988) which is often used to support the positive benefits of DDC courses. This study compared the crash and conviction rates (per 1000 participants) for 84,806 drivers before and after attending DDC classes. The authors reported an overall 15 percent

reduction in the crash rate and a 56 percent reduction in the conviction rate for the participants (all statistically significant results).

However, two aspects of this study lead to questions about the validity of the results -- the lack of a control or comparison group and the "volunteer" nature of a large part of the participant sample. First, the study was essentially a before/after study with no control/comparison group used to eliminate possible biases. Thus, any effect measured could well have resulted from "causes" other than the treatment (as appears to be the case). Second, the participants were either "public," who voluntarily took the classes to receive insurance premium and/or driver license point reductions, or "occupational," the vast majority of whom were required to attend as a job requirement. Whereas the total pool of participants showed the previously stated 15 and 56 percent reductions in crash and conviction rates, all of the "positive" effect was attributable to the "public" drivers who took the course. It is noted that this "public" group is at least partially composed of what could be considered "problem" drivers -- drivers who had more points on their driving records than the average driver. This is based on the fact that one of the mechanisms for "advertising" the course was through DMV warning letters to drivers who had accumulated six or more points on their record in the past 18 months, and on the fact that approximately 30% of the "public" participants stated that "point reduction" was one of the main reasons for attendance. Thus, it is a fair assumption that a significant portion of the "public" group would be classed as some level of "poorer-than-average" drivers, drivers who would be expected to improve even without treatment due to the above-noted regression phenomenon.

The subset of 7,874 "occupational" participants, who might be considered a more representative pool of "normal" drivers, showed a 12 percent reduction in violations, but a 28 percent increase in crashes. One of the subgroups in this "occupational" sample were police officers who attended an Emergency Vehicles Operations Course (EVOC), which may well differ from the standard driver-improvement course. This subgroup experienced greatly increased crash rates after the course (probably due to increases in exposure to hazardous driving situations), and greatly decreased violations rates with essentially no convictions reported in the after period (perhaps due to failure to cite police officers for traffic violations). If this questionable subgroup is removed

from the "occupational" group, the remaining participants experienced a 13 percent increase in subsequent violations and a 14 percent increase in subsequent crashes. In short, due to methodology problems, little weight can be given to the findings of this study.

For their study, Struckman-Johnson, et. al were able to identify 19 studies out of the 65 reviewed that provided methodologically sound evaluations of the activities. These 19 studies reported on a total of 59 different driver improvement activities.

Table 1, extracted from their report, summarizes the results of the 59 studies reviewed. As can be seen, less than half (44.1%) of the programs

Table 1. Significance of violation and crash effects from Struckman-Johnson, et. al.

		Significant Crash Effect		
		No	Yes	Total
Significant Violation Effect	No	26 78.8 56.5	7 21.2 53.8	33 100.0 55.9
	Yes	20 76.9 43.5	6 23.1 46.2	26 100.0 44.1
	Total	46 78.0	13 22.0	59

Note: Cell contents are total frequency, percent of row total, and percent of column total, respectively.

showed a significant effect on violations (with two being in the direction of more violations for treated drivers) and less than a fourth (22%) showed a significant effect on subsequent crashes. Overall, only 6 of the 59 programs (10.2%) showed significant effects on both violations and crashes. Further analyses lead to the overall conclusion that, based on these 59 studies, "the effect of driver improvement programs on violations provides minimal predictive information about the effects of these same programs on crashes."

Since the focus of this current report is on the effects of the National Safety Council's Defensive Driving Course on violations and crashes, Tables 2

Table 2. Crash and violation effects for driver improvement activities using non-DDC group educational sessions (*Significant at $p<0.05$)

Author(s)	Subjects	Type of Treatment	Treatment N	Control N	% Crash Effect	% Viol. Effect
Coppin (1973)	Problem drivers	Subject interaction meeting	1810	1530	+3	-2
	Problem drivers	Leader interaction meeting	1824	1530	+5	-4
	Problem drivers	Group education meeting	1741	1530	-4	-6
	Problem drivers	Driver improvement meeting	1856	1530	+3	-6
Kadell (1984)	Problem drivers	Group education meeting	4900	4900	-10*	-10*
	Problem drivers	Modified group education meeting	4900	4900	-7	-12*
Kadell, et al.(1980)	Problem drivers	Group education meeting	2733	2611	-7	-12
Lynn (1982)	Problem drivers	Individual hearing plus driver improvement clinic	1738	1650	-11	-21*
Marsh (1978)	Problem drivers	Group education meeting	2539	2539	-1	-9*
	Problem drivers	Training In Perceptual Strategy (TIPS)	2538	2539	-1	-10*
	Problem drivers	Attitude training plus TIPS	2543	2539	+6	-8*
	Problem drivers	Attitude plus TIPS plus threat	2535	2539	+4	-9*
Peck & Kadell (1983)	Problem drivers	Group education meeting	5500	5500	-17*	-14*
Ulmer (1978)	Violators at adjudication	General education session	2062	772	-4	+40*
Wooten, et al.(1981)	Problem drivers	Group education meeting	2619	663	+4	-1

and 3 provide a summary of the 19 studies that were identified by Struckman-Johnson, et. al as being methodologically adequate and that consisted of some type of group-education treatment. This was done to separate these treatments from other types such as administrative reviews, warning letters, and individual counselling.

As can be seen in Table 2, which refers to non-DDC group education programs, 14 of the 15 group education studies showed some reductions in violations after treatment, with 9 of the 15 showing statistically significant reductions. The significant violation improvements ranged from -8 to -21 percent. One study reported a 40 percent increase in violations. Nine of the 15 studies of non-DDC group-education treatments showed some reduction in crashes, but only two reported statistically significant reductions, with one being -10 and the other being -17 percent.

Since this current study is directed to group education programs in which the DDC curriculum has been used, Table 3 presents the results of pertinent studies considered adequate from a methodological standpoint by Struckman-Johnson, et al. Here, only one of the four studies indicated a statistically

Table 3. Reported crash and violation effects for driver improvement activities using DDC curricula (*Significant at $p<0.05$)

Author(s)	Subjects	Type of Treatment	Treatment N	Control N	% Crash Effect	% Viol. Effect
Planek, et al.(1974)	High school driver education students	NSC Defensive Driving Course	290	284	+7	-7
Salzberg & Klingberg (1978)	Problem drivers	NSC Defensive Driving Course	1662	888	-10	-13*
Schupack & Planek (1975)	High school seniors	NSC Defensive Driving Course	156	126	-11	-1
Ulmer(1978)	Violators at adjudication	NSC Defensive Driving Course	1021	373	+37	+5

significant effect, and that was a 13 percent reduction in violations. Three of the four studies indicated some reduction in violations, two of the four

indicated some (non-significant) decrease in crashes, and two of the four indicated some (non-significant) increase in crashes.

In summary, past research which has been methodologically sound has indicated, in general, that non-DDC group-education programs may have some effect on the subsequent violation history of the students, but there is little evidence of a significant effect on subsequent crashes. The studies of the DDC course have shown similar results.

Study Methodology

Overview. As noted above, the goal of the proposed DDC evaluation required of HSRC was to determine whether or not the course has had an effect on the subsequent crash and violation experience of those attending. It is noted that HSRC was not looking at changes in attitudes, changes in belt wearing behavior, changes in accident "fault", or changes in accident severity. While all of these criteria could be studied (if an appropriate study design was established), our charge was to look solely at the overall crash and violation experience.

As noted above, the best evaluation methodology that could be employed with any driver-improvement program would involve a before/after study with random assignment of a pool of subjects to treatment and control. In such a study, people eligible for the Mecklenberg DDC school would have been split into a group allowed to attend the school and another group who were required to go through the court process as they normally would have (i.e., as would have been the case in the absence of the school). This assignment to school treatment/control would have been done randomly -- perhaps by the officer flipping a coin to determine whether the person cited would have the opportunity to attend. As in the implementation of most new experimental treatments, this was not done. Instead the program was instituted as an option for all drivers within the participating judicial districts.

An alternative design which could be conducted with the available data would have been a simple comparison of the prior and subsequent (before/after) driving records of the group attending the DDC program, counting the violation which was dismissed as occuring in the before period. However, this is the weakest of all possible study designs in that it does not control for other "causes" which could bias the results. Particularly troubling in this current

setting would be the "regression to the mean" bias. Here, based simply on chance and the underlying "true" driving habits, persons who have had a violation in a prior period would be expected to "improve" in a subsequent period even in the absence of treatment.

Thus, what was needed was a comparison group of drivers who were similar to the group who attended the school, but who had to pass through the normal North Carolina court process for traffic violators since no school was available to them. Because Mecklenburg and the surrounding areas are fairly urban in nature, the study design used involved the selection of a second NC urban area as the comparison county -- Wake County, with Raleigh as it major urban area. (We chose not to use Mecklenburg residents who did not take the course as a comparison group, since, by definition, they would be a self-selected group of drivers. Such self-selection could be hypothesized to result in either a group of "better" drivers or a group of "poorer" drivers. They would be better if middle-age drivers with low insurance rates and no prior points chose not to attend. They would be worse if young, aggressive males who "don't have time for school" chose not to attend. Regardless of direction, there is little reason to believe these non-attendees would be similar to those who did attend. In addition, given the popularity of the program, there were doubts as to whether the sample size of non-attenders would prove adequate for analysis.)

The study involved a number of different comparisons of the prior and subsequent accident and violation histories of persons who attended the school in the Mecklenburg County area with the records of Wake County residents who had a conviction on their driving record during the same time period that persons in Mecklenburg County attended the school. This match of "conviction" to "school attendance" is logical since those persons who attended the school received a citation which supposedly would have resulted in a conviction on their driving record, but was dismissed since the school existed.

Thus, as detailed in the following paragraphs, the basic analyses involved comparing the prior and subsequent driving records of these two groups subcategorized by other variables which might lead to differences in driver histories -- age, race, sex and prior violation/accident history.

The data. The data used for evaluating the DDC was taken from the driving records of two groups of drivers: the study group consisting of drivers who

completed the DDC within the time interval from July 1, 1989 through April 15, 1990; and a comparison group of Wake County drivers who had received a similar violation during the same time interval. Computer files were obtained containing the license number of each subject completing the DDC, and the offense or violation which triggered the subjects' enrollment in the DDC. Subjects were dropped from the study file if the triggering violation was not one which would have been recorded on the driver history record, (e.g., seat belt violation or darkened windows). For the remaining subjects, a study file was developed which contained the following data items extracted from the driver history file:

Driver Age, Sex, Race
Prior Accidents
Prior Violations
Subsequent Accidents
Subsequent Violations

The variable - Prior Accidents - was a count of the accidents in which the subject was involved, accumulated over a two-year period ending 21 days before the subject's DDC completion date. (The 21-day window was chosen since the normal enforcement procedure in the two counties is for the arresting officer to assign a court date on the citation, and the normal time interval between citation and court date is three weeks. The study group then had to complete the DDC program prior to their court date.) Subsequent accidents were accidents accumulated over a six-month time period beginning the day after the DDC completion date. Prior and subsequent violations were defined similarly.

For the comparison group, all drivers were selected if they were residents of Wake County, and if they had a qualifying violation in the time interval July 1, 1989 - April 15, 1990. Thus, these were people who would have been eligible to attend the DDC, (and be in the study group) if the violation had occurred in Mecklenburg County. In cases where a Wake County driver had more than one such violation in the specified time interval, the earliest (chronological) such violation was taken as the qualifying or triggering event.

The same set of data items was obtained from the driver history file for the comparison group. In this case prior accidents and violations were accumulated over a two-year period ending the day before the arrest date corresponding to the qualifying violation. Subsequent accidents and violations were accumulated over a six-month interval beginning the day after the

conviction date of the triggering violation. As with the study group, the average length of time between these two intervals was approximately 21 days. The development of these study files is shown schematically in Figure 1. The resulting data file contained records for 19,467 study (DDC) subjects and 23,684 comparison subjects.

Data Analysis

Table 4 shows distributions of age, race, sex, prior violations, and prior accidents in the study and control or comparison groups. The table also gives the distributions of these characteristics for random samples of about 50,000 Wake and Mecklenburg County drivers. Differences in the distributions of age, race, sex, and possibly prior record between the study and control groups may be a reflection of the self-selection process of attending the DDC. However, differences in prior accidents and violations may also be due to differences in the two communities, (e.g., enforcement, traffic density). The fact that distributions of prior violations and prior accidents in the two random samples tend to differ in the same direction as the evaluation samples seems to further suggest some community differences.

Two different analysis strategies were used to take into account these differing sample characteristics. The first approach was designed to take into account differences in the subjects themselves, between the study and comparison samples. For these analyses, the subjects were partitioned into 32 subpopulations defined by the combinations of the four age categories (i.e., 16-20, 21-30, 31-40, 41+), sex (male or female), two race categories (white or black), and two levels of a prior driving record variable (0 or 1+). Within each subpopulation the proportion having subsequent accidents (or violations) in the study group was compared with the similar proportion in the control

Figure 1. Development of DDC Evaluation Data Files

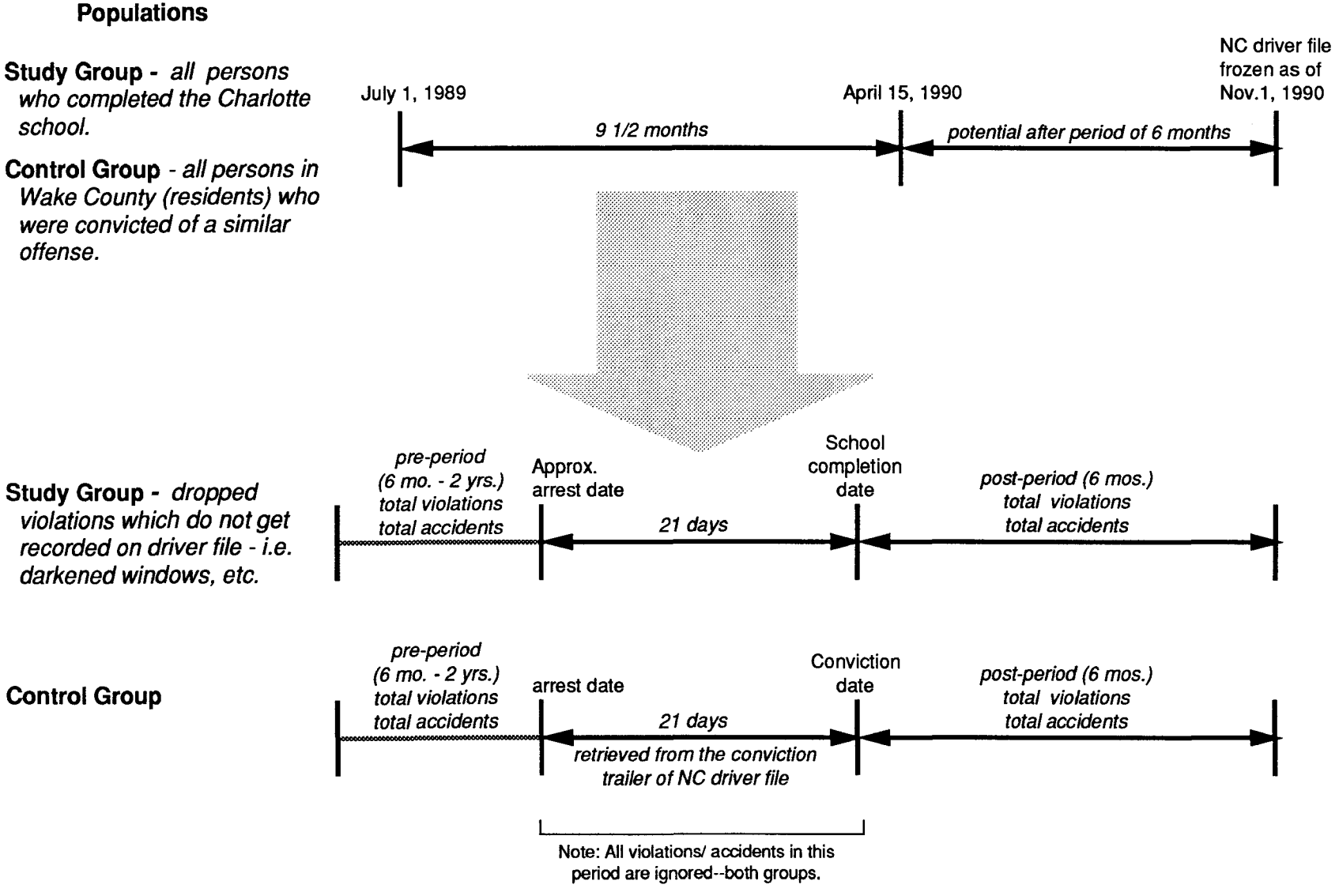

Table 4. Sample Characteristics

		DDC Evaluation Samples		Random Samples	
		Study	Control	Mecklenburg (54,112)	Wake Co. (44,017)
Age	16-20	2829 (14.53%)	3497 (14.77%)	(7.28%)	(7.48%)
	21-30	6556 (33.68%)	9729 (41.08%)	(27.69%)	(29.16%)
	31-40	4962 (25.49%)	5796 (24.47%)	(26.23%)	(27.29%)
	41+	5120 (26.30%)	4462 (19.68%)	(38.80%)	(36.07%)
Race	White	16338 (82.27%)	18070 (73.72%)	(72.76%)	(75.63%)
	Black	3129 (15.76%)	5614 (22.90%)	(23.98%)	(20.01%)
	Other	392 (1.97%)	828 (3.38%)	(3.26%)	(4.36%)
Sex	Male	12050 (60.68%)	16225 (66.19%)	(52.46%)	(53.19%)
	Female	7809 (39.32%)	8287 (33.81%)	(47.54%)	(46.81%)
Prior Violations	0	14248 (71.75%)	16438 (67.06%)	(85.28%)	(83.59%)
	1+	5611 (28.25%)	8074 (32.94%)	(14.72%)	(16.41%)
Prior Accidents	0	15533 (78.22%)	19641 (80.13%)	(89.33%)	(90.63%)
	1+	4326 (21.78%)	4871 (19.87%)	(10.67%)	(9.37%)

group. Figure 2 shows graphically the data from one such analysis. This analysis uses prior accidents as a controlling factor and compares the proportions having subsequent accidents. It can be seen from the figure that for most of the subpopulations -- 28 of the 32 -- a higher percent of the study group had subsequent accidents than did the comparison group. An analysis of these data was carried out by fitting a log-linear model to the proportions using SAS PROC CATMOD. A main effect model fit the data well; the residual X^2 was not significant, $p = .155$, while the effect due to group, (study vs control) was highly significant ($p = .0001$), and estimated the study group to have subsequent accident rates that were 26.7 percent higher, overall, than those of the control group.

Two additional CATMOD models were developed which used prior violations (0 vs 1 or more), and overall driving record (no accidents or violations vs 1 or more of either), respectively, as a controlling factor along with age, sex, race, and group. The estimated group effect was significant in both cases ($p = .0001$), and study group accident rates were estimated to be 29.0 percent and 28.1 percent, respectively, higher than the rates of the comparison group.

Average accidents per driver were also analyzed using a linear analysis of covariance-type model. After adjusting for differences in age, race, sex, number of prior accidents, and number of prior violations, the estimated group means were:

DDC study group: .0846 accidents/driver
Comparison group: .0677 accidents/driver

The group effect was again significant ($p = .0001$), and estimated accidents per driver were 25.0 percent higher for the study group.

When subsequent violations were analyzed, the roles of the study group and comparison group were essentially reversed. Specifically, in 28 of the 32 subpopulations defined by age, race, sex, and prior violations (0 vs 1 or more), the proportion of subjects having a violation in the subsequent period was lower for the study group than for the comparison group. Based on a model that was fit to these data, a statistically significant group effect was estimated ($p = .0001$), and the subsequent violation rate for the study group was estimated at 16.7 percent lower than that of the comparison group. Similar

Figure 2. Percent of Drivers with Accidents in Follow-up Period

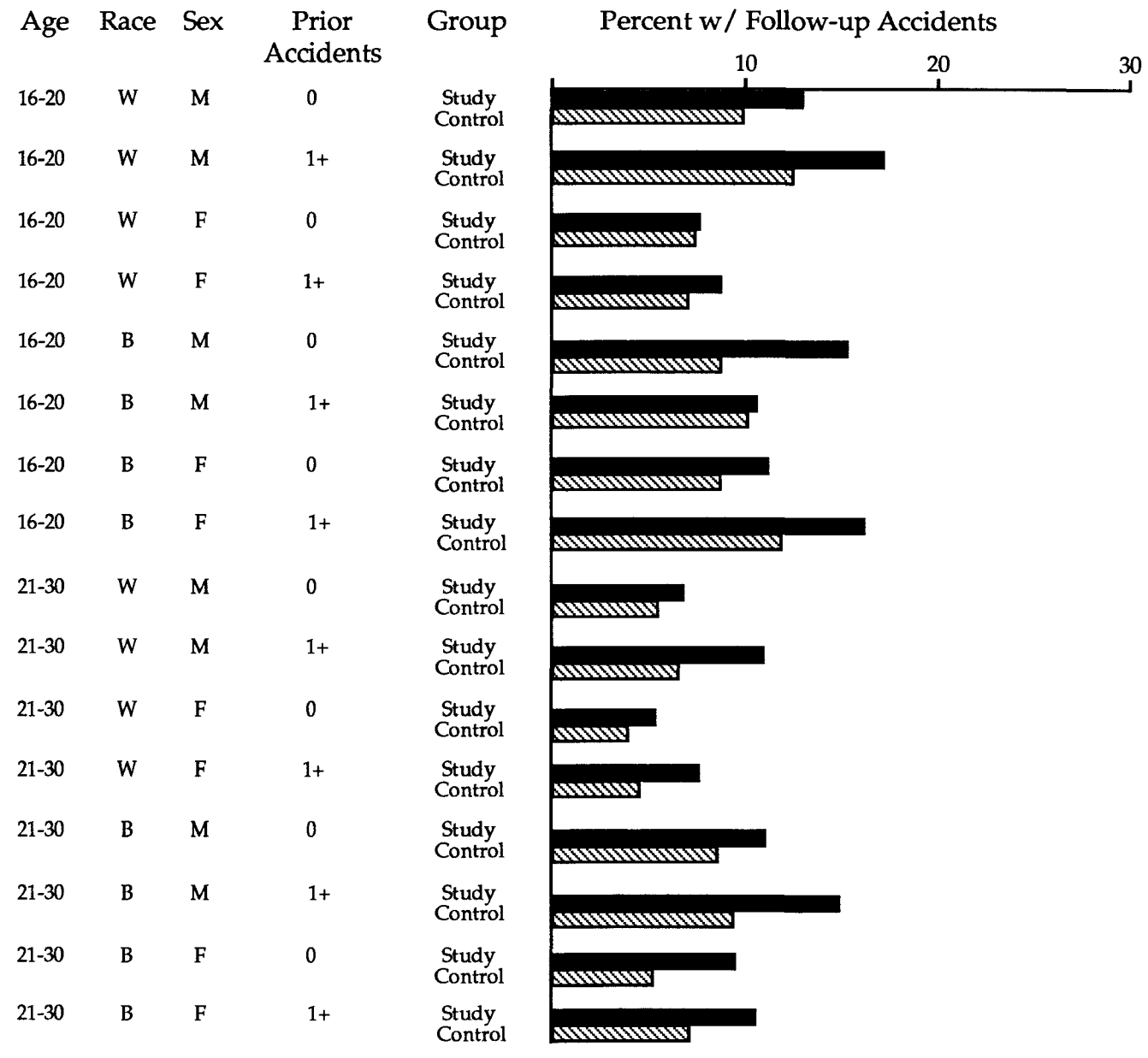

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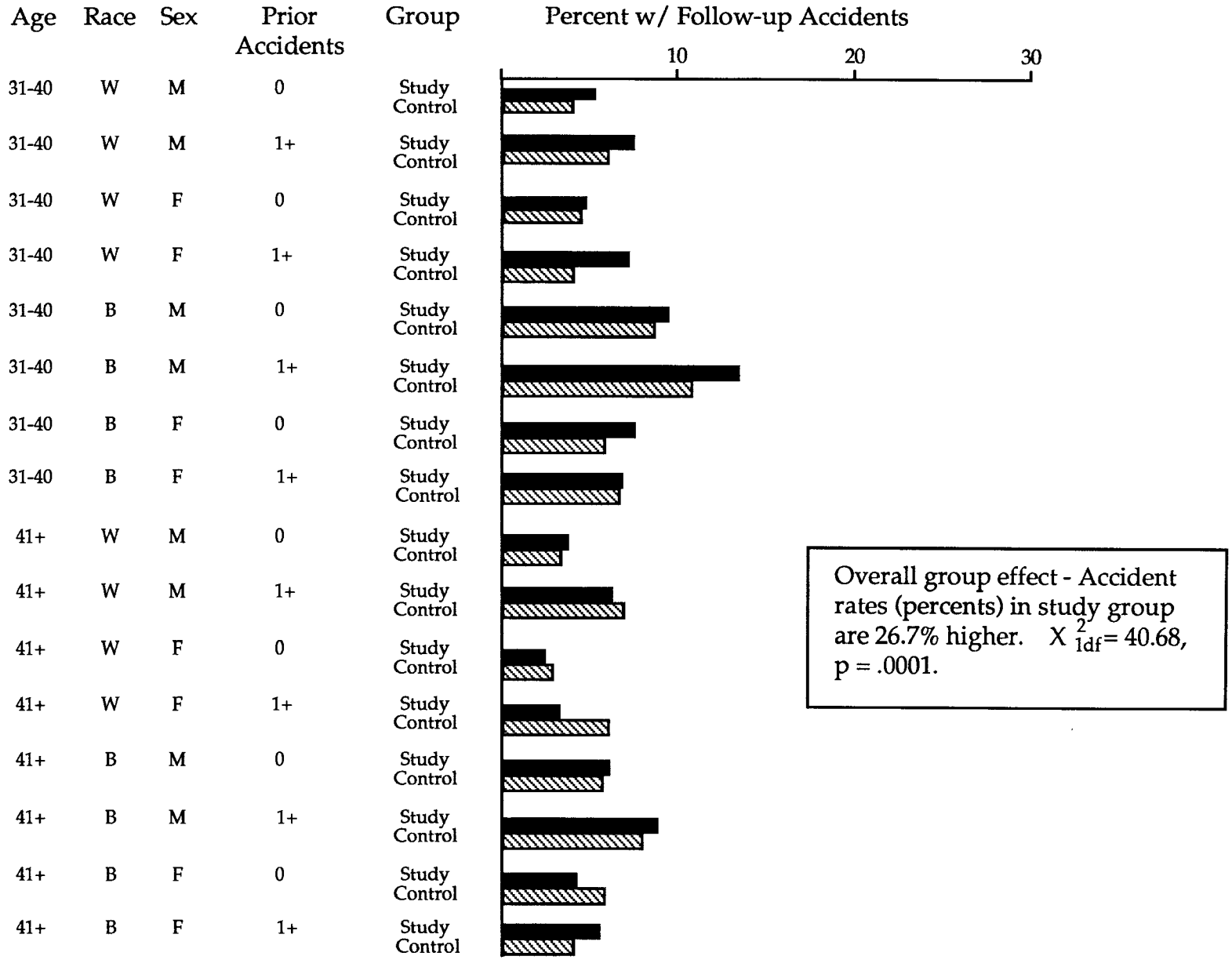
Figure 2. Percent of Drivers with Accidents in Follow-up Period (cont)



Overall group effect - Accident rates (percents) in study group are 26.7% higher. $X^2_{1df} = 40.68$, $p = .0001$.

results were obtained from models controlling for prior accidents or overall prior driving record. From a linear model for average subsequent violations per driver, the estimated group means, adjusted by age, race, sex, number of prior accidents, and number of prior convictions were:

Study group: .1563 violations/driver,
Comparison group: .1908 violations/driver.

Thus, average violations per driver were 18.1 percent lower for the study group than for the comparison group.

The consistency of the differences in accident and violation rates within the various subpopulations seemed to further suggest some underlying differences between the two communities which would tend to produce these differences. In an attempt to negate the effects of such differences, consider the following model.

Let r_m and r_w represent some form of accident rates for Mecklenburg and Wake Counties, respectively, and suppose that due to community differences, $r_m = \alpha\, r_w$ where α is a scale factor such that $\alpha > 1$. The above relationship should apply to populations from the two communities in general, and in particular, to the study and comparison samples in the time interval prior to the start of the DDC.

Using subscripts 1 and 2 to indicate prior and subsequent periods then for the prior rates $r_{1m} = \alpha_1 r_{1w} = \alpha r_{1w}$. In the absence of the DDC, then, it would similarly follow in the subsequent period that $r_{2m} = \alpha_2 r_{2w} = \alpha r_{2w}$, so $\alpha_1 = \alpha_2 = \alpha$. However, if the DDC resulted in a reduction in r_{2m}, so that $r_{2m} < \alpha r_{2w}$, we could write $r_{2m} = \alpha_2 r_{2w}$, where $\alpha_2 < \alpha = \alpha_1$, or $\alpha_2/\alpha_1 < 1$. Now consider ratios of accident rates in the subsequent period to accident rates in the prior period. Let

$$R_m = \frac{r_{2m}}{r_{1m}} = \frac{\alpha_2\, r_{2w}}{\alpha_1\, r_{1w}} = \frac{\alpha_2}{\alpha_1} R_w$$

If $(\alpha_2/\alpha_1) < 1$, then this ratio of accident rates should be smaller for Mecklenburg County than for Wake County. Note also, that (α_2/α_1) can be examined directly by the ratio of accident rate ratios R_M/R_W.

Values of the quantities R_m and R_w defined as ratios of proportions of drivers having subsequent accidents to proportions with prior accidents, within 16 subpopulations defined by combinations of age, race, and sex are shown graphically in Figure 3. The figure shows the accident ratios to be larger for the study group than for the comparison group through most of the younger age categories, approximately equal through the middle age categories, and larger for the control group for the oldest age categories.

As before, a formal analysis was carried out by fitting a CATMOD model to the data. The fitted model did, indeed, contain a significant age by group interaction ($p = .014$). The estimated group effects and their statistical significance are also given in Figure 3 within each of the four age categories. Thus, for drivers aged 30 and under the study group had significantly higher after-to-before accident ratios, drivers in the 31-40 age range did not differ significantly, and for those over 40 the study group was lower and marginally significant. Further examination of this latter group indicates that the lower study group rates are primarily found within the two female subgroups, with little difference between the male study and comparison subgroups. Using a weighted average as an overall measure, the study group ratio was about 6 percent higher than the control group ratio. Taken from the same analysis, Figure 4 shows values of the ratio of ratios (or α_2/α_1, using the previous notation) compared to the value 1.0, where a value of 1.0 would mean no difference between the DDC and comparison group, a value greater than 1.0 would mean that the DDC group had a higher accident ratio, and a value less than 1.0 would indicate that the DDC group had a lower ratio.

Figure 5 shows a similar set of ratios of after-to-before violations. These ratios (mostly < 1.0) were generated by a CATMOD model which contained only main effects, including a significant group effect ($p = .0001$), which showed the study group to have after-to-before violation ratios that were 13.8 percent lower than those for the comparison group.

In summary, these analyses of the DDC subjects subsequent accidents and violations consistently showed their violation rates to be reduced approximately 13 to 15 percent relative to their prior levels and relative to the behavior of a similar group of comparison drivers. We find no evidence, however, for similar reductions in subsequent accidents, with the possible exception of female drivers over 40 years of age. For younger drivers (<30),

Figure 3. Ratios of Percent with Follow-up Accidents to Percent with Prior Accidents

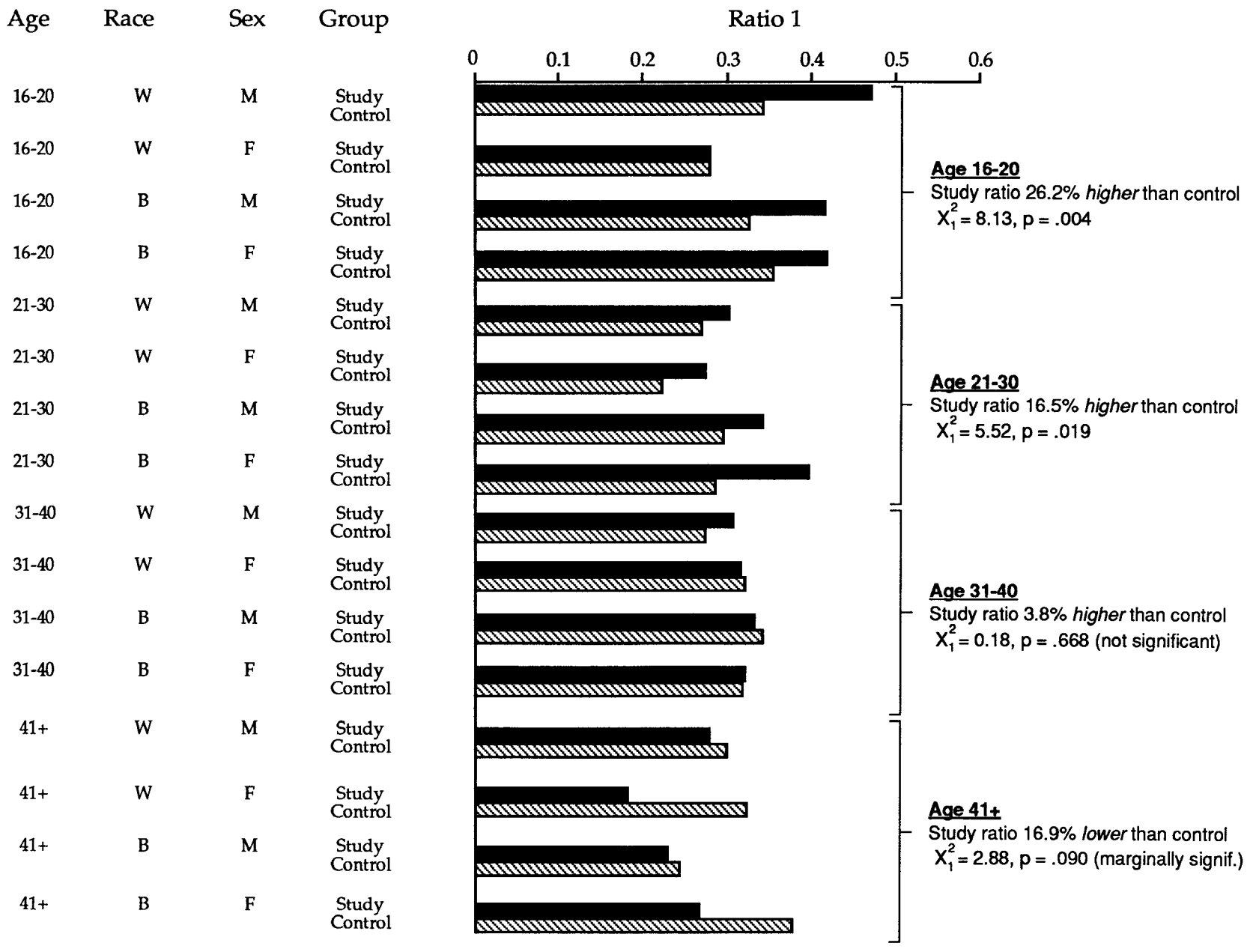


Figure 4. Ratio of Accident Ratios

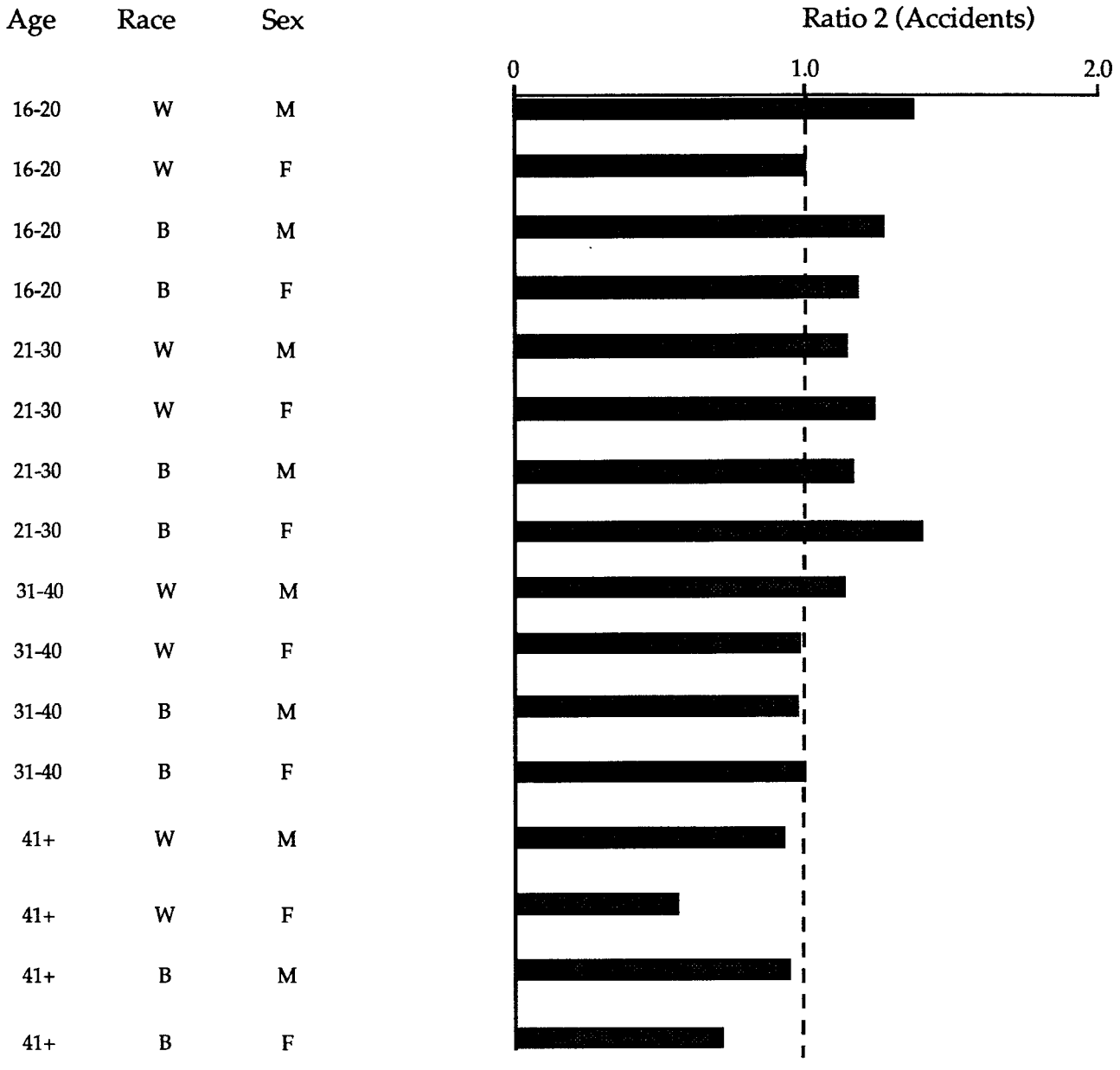
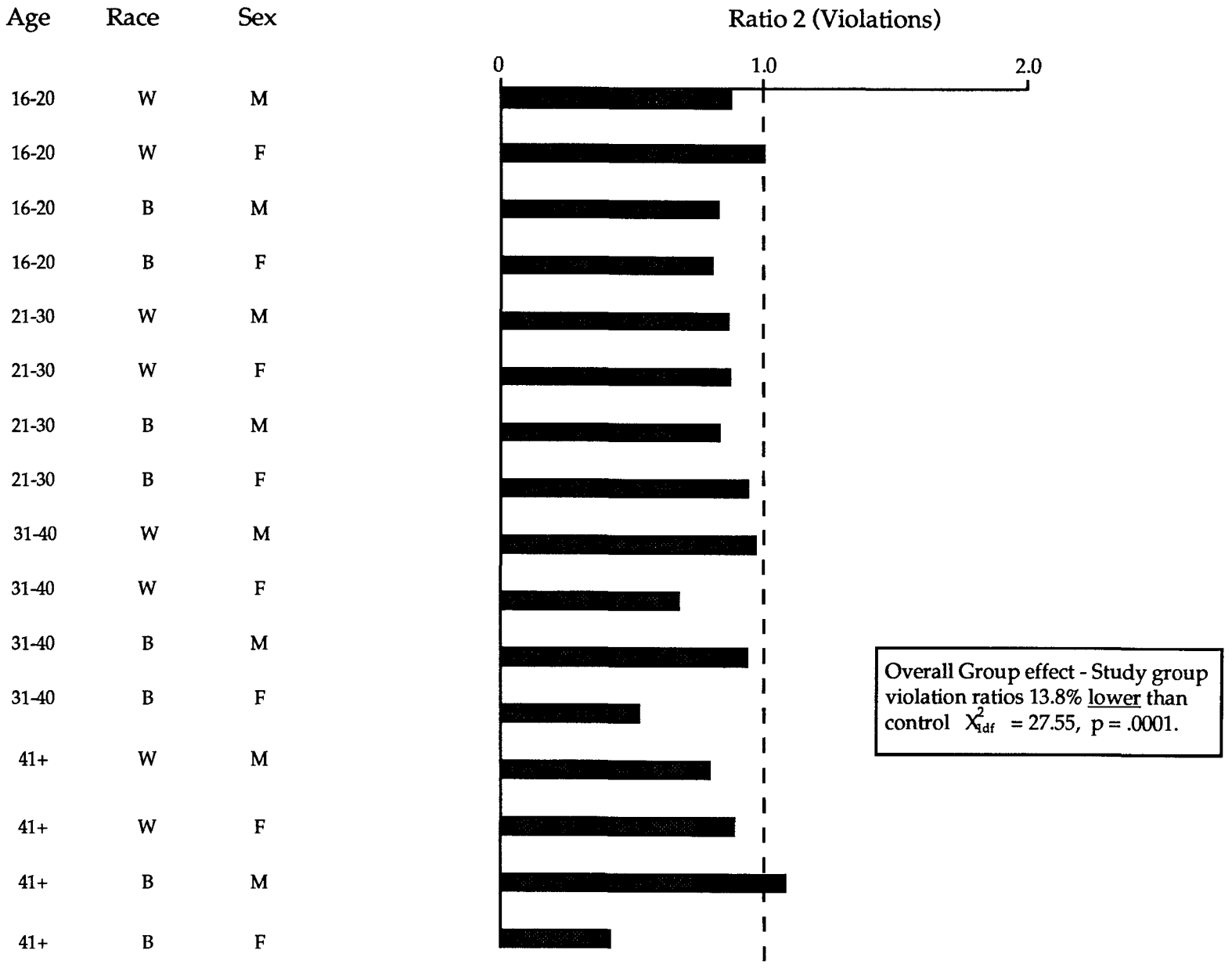


Figure 5. Ratio of Violation Ratios



the data suggest that the DDC is less effective in reducing accidents than the standard judicial process.

Discussion

Similar to prior studies reviewed earlier, there is little indication in this data set that the Defensive Driving Course/Citation-Dismissal Program had as great an effect on subsequent crashes as did the normal court procedure in the comparison county. However, the reader must note what this study is and is not.

First, the study is not an evaluation of all driver education programs. The results are certainly not transferrable to any programs involving beginning drivers or programs involving drivers if the education program is coupled with a sanction (such as court conviction results in points and/or other licensing restrictions). DDC is a four-hour event rather than a long-term educational process. Rather, this study is a comparison of a group who attended a DDC program and had a citation dismissed (who theoretically would have been convicted of a minor traffic violation) versus a group of similar people convicted of the traffic violation who did not have such a school available in a similar county. Thus, we are comparing two treatments -- DDC\Citation-dismissal versus results of the normal court system.

A cursory look at the data might lead the reader to conclude that this school "made drivers worse" by "increasing their accident histories." This is certainly not the conclusion of the authors. There is no logical reason to believe that what is taught under the DDC program would have any negative effect on subsequent driving behavior. The key here is what is being compared -- not just education to nothing, but an educational program to another treatment (i.e., the normal treatment associated with going through the court system, being convicted, paying a fine, and having DMV driver license points and/or other sanctions added to one's record). These data would indicate that the group education/citation-dismissal system was not as good at reducing the subsequent accident history as the normal court proceedings. The comparison made here was because of the way the system was set up -- as a substitute for the normal court process.

There is also some limited indication that certain subgroups did appear to experience more of a benefit from the DDC program than from the normal court

system. These were primarily those persons in the over-40 age group, with the overwhelming part of the effect being for older white and black females. Based on the data, the DDC program looked approximately equally effective as the court sanctioning program for 31-40 white females, black males, and black females. (The 31-40 year-old white males in the program group showed a higher subsequent accident ratio.)

Second, the evaluation procedure did not analyze every possible effect that such a group education program might be hypothesized to have. DDC might be hypothesized to have an effect on "at-fault" accidents, an effect on the severity of accidents (resulting both from people putting themselves in less severe situations and from increased use of occupant restraints), an effect on savings to the court systems, or other hypothesized outcomes. This study, however, focused simply and completely on crash experience and violation experience of the study group as compared to the comparison group and made no attempt to look at these other factors.

Finally, in any study in which a comparison group has be drawn from a separate location, there may be residual differences in the underlying populations that cannot be completely controlled for (e.g., differences in drivers, enforcement systems, exposure factors such as higher probability for accidents to a population living in a more urbanized area, weather, road systems, etc.). We have attempted to control for these variables by analyzing the data in two different ways, both of which use prior information as a control for what happens in the after period. However, it is not possible to "guarantee" that all of the possible biasing factors could be accounted for since the program was not initially set up in such a way as to provide for a truly excellent evaluation. Such precise control of all other biasing factors can only be gained through the imposition of a design in which people from the same pool of drivers in the same locations are randomly assigned either to the program or to the control group.

With these caveats in mind, based on the data available and on the results of past well-designed studies, we can not conclude that the Defensive Driving Curriculum\Citation-Dismissal Program had more of an effect with respect to crash experience than the normal court system in a similar county for similar drivers.

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