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THE RESPONSE TO FINES AND PROBABILITY OF DETECTION IN A SERIES OF EXPERIMENTS

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ABSTRACT

We use traffic data from a series of experiments in the United States and Israel to examine how illegal behavior is deterred by various penalty schemes and whether deterrence varies with age, income, driving record and criminal record. We find that red light running decreases sharply in response to an increase in the fine or an increase in the probability of being caught. The elasticity of violations with respect to the fine is larger for younger drivers and drivers with older cars. Drivers convicted of violent offenses or property offenses run more red lights on average but have the same elasticity as drivers without a criminal record. Within Israel, members of ethnic minority groups have the smallest elasticity with respect to a fine increase.

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I. Introduction

The economic model of crime activity suggests that potential offenders respond to the expected value of punishment (Becker [1968], Stigler [1970], Polinsky and Shavell [1984]). Critics of this view have argued that many criminals are irrational, uninformed or have such high discount rates that increases in expected punishment do little to create deterrence. Other researchers (e.g. Menniger [1968]) have argued that deterrence will fail because criminals are pre-destined to commit anti-social acts due to genes or early environment.[2]

We examine a series of traffic experiments and find that increases in fines or probability of apprehension increase deterrence for all groups of drivers, including those convicted of violent crimes and property crimes. The experiments are attempts by police agencies to reduce the incidence of people driving through red lights. The data allow us to examine how responsive people are to shifts in the magnitude of a fine and shifts in the probability of getting caught. Drivers exhibit a large response to both policy levers. For example, the introduction of red light cameras in two U.S. cities reduced the number of violations by about 50%. The elasticity of violations with respect to the size of the fine is roughly -0.20.

We find that drivers previously indicted for property or violent crimes break traffic laws more often. However, these drivers have as large an elasticity with respect

[2] See Hernnstein and Wilson [1985] for a general discussion of theories of criminal behavior.

to the fine increase as the general population. Younger drivers have a larger elasticity while wealthier drivers have a smaller elasticity[3].

Our finding that people are responsive to both the probability of apprehension and the magnitude of the penalty is consistent with much of the modern deterrence literature. For example, Levitt (1998) finds that the elasticity of crime with respect to the arrest rate is approximately -0.20. Kessler and Levitt (1999) use sentence enhancements to show that increases in prison sentences have a large deterrent effect.[4] Our results contrast somewhat with Grogger (1991) and Witte (1980) who find that criminals responded very little to the magnitude of the penalty (prison sentence).

The data employed here are from a series of experiments conducted in Virginia, California, and Israel regarding how people respond to shifts in fines and probabilities of being caught for running red lights. The use of data on traffic offenses rather than data on felonies and larcenies is somewhat unusual in the economics literature, but the data offer several advantages.[5] First, these data are from a fairly unique case in which there is an exogenous shift in the penalty or the probability. In two cases we have treatment groups with shifts and control groups without shifts. Second, in contrast to most crime data, there is no reporting problem. Cameras are used at each intersection to achieve full monitoring of the number of cars and the number of violations. Hence, the number of reported violations is the number of actual violations. Third, since there are no prison sentences handed out, there are almost no concerns of untangling deterrence

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[3] This finding is predicted by Polinsky and Shavell [1984], [1991] , and Garoupa [1998].
[4] Erhlich [1975] examines the deterrent effect of capital punishment.
[5] Becker [1968] considers the case of traffic violations as one of his examples. Other cases of the use of traffic violations, or even red light running, as examples of crime are Polinsky and Shavell [1991],

effects from incapacitation.[6] Fourth, we can compare the behavior of criminals and non-criminals in their response to a fine increase.

The social losses from red light running accidents are quite large and are on the same order of magnitude as many felonies. Red light running is a serious problem in virtually any country with a large number of cars and drivers. In the U.S. in 1998, roughly 2,000 deaths resulted from drivers running red lights.[7] This compares with about 17,000 murders in 1998 (FBI Uniform Crime Reports). Annually there are approximately 260,000 crashes in the U.S. caused by red light running.[8] The implied costs of car repair alone are on the order of $520 million per year.[9]

The structure of the paper is as follows. Section 2 is a brief description of the data, while section 3 presents the empirical framework used for the micro data and section 4 presents results. Conclusions are drawn in section 5.

Kaplow and Shavell [1994], and Friedman [1999]. Most economists follow the practice of Erlich [1975] or Levitt [1998] in examining murders, rapes, robberies, assaults, larceny, or auto theft.

[6] Few people lose their driver's licenses as a result of getting caught even multiple times.

[7] From analysis of NHTSA administration data. The Insurance Institute for Highway Safety estimated that the number of deaths from red light running was 745 in 1998. The figure of 2,000 deaths is based upon the number of fatal accidents in which someone was charged with running a red light.

[8] From US Dept. of Transportation and Insurance Institute for Highway Safety.

[9] This is a back of the envelope calculation based on a median car repair bill of $2000. Clearly in addition to the huge social costs, there could be social benefits to red light running if overall there is time saved for drivers. We make no effort to estimate these benefits.

II. Description of the Data and Experiments

To combat the problem of red light running, police agencies have taken a variety of steps. Among the most effective steps has been the installation of "red light cameras" at intersections. These are small cameras which fit inside a protective housing installed on a light pole, tree, or building. The camera is linked electronically to the traffic signal and wires buried in the road. When a car enters the intersection after the light has turned red, the camera takes a picture of the car's license plate and in some cases a picture of the driver. (This depends on the requirements under local laws.) Typically drivers have a grace period so that tickets are only issued if the car enters some fraction of a second after the light turns red.

The cameras can be completely hidden or they can be well advertised with signs. They are fixed in direction and one camera can only cover one direction of traffic, though it can cover multiple lanes in a single direction.

Evidence from around the world shows that public knowledge of the use of camera enforcement in a given area creates large reductions in the number of violations. We have data from experiments in Fairfax, Virginia, Oxnard, California, San Francisco, California, and Israel.[10]

In Fairfax, VA, the introduction of cameras was coupled with a controlled experiment to examine the magnitude of the drop in violations. Prior to any public

[10] The data on violations in each intersection for Fairfax and Oxnard come from traffic safety publications by Retting et al.[1996] and [1999]. Using additional data from the Oxnard Police and Fairfax City Planner's office, we have added information on the increase in probability of receiving a ticket and calculated the implied elasticity of response. The San Francisco and Israeli data were provided by the respective police departments.

announcement of the program, monitoring began at three types of intersections: 1.) camera intersections in Fairfax, 2.) non-camera intersections within Fairfax, and 3.) control intersections in nearby cities that do not use camera enforcement. The non-camera and control intersections were monitored using video cameras.

After several hundred hours of monitoring the level of violations, the camera enforcement program in Fairfax was announced with a publicity campaign including newspaper ads and signs posted at the city limits (but not specific intersections). This reflects increased probability of detection. The fine imposed on red light runners was been kept constant at $50 during the whole period under consideration. We take the dependent variable to be the number of violations per hour. We measure the drop in the rate of violations as a difference in difference; we take the difference before and after at the Fairfax camera intersections minus the difference before and after at the control intersections.

A natural question is whether or not the timing of when the cameras are installed is truly exogenous. First of all differencing out the control intersections should remove any overall trend in violations within Virginia. Secondly, the timing of the program was controlled more at the State level than at the local level. The State legislature had to pass a law legalizing the use of camera enforcement. Once they did that, Fairfax initiated its program soon after.

A second experiment similar to the Fairfax experiment was run in Oxnard, California at about the same time (circa July 1997). This experiment also involved camera and non-camera sites within Oxnard and control sites located in nearby cities (e.g. Santa Barbara.) The methodology and the resulting drops in violations are similar to those found in Fairfax.

The Oxnard experiment has an additional component: In January 1998, the State of California more than doubled the fine for running a red light. The fine was raised from $104 to $271. As is shown in Figure 1, this caused an immediate and large drop in the number of violations per day. The number of violations then stabilized at this new low level where it remained. We assume this shift in the fine to be exogenous and we use it to obtain an estimate of the elasticity of the rate of violations with respect to the fine. We believe that this estimate is useful because it includes complete monitoring and a quasi-exogenous shift in the fine.

The same fine shift occurred in San Francisco (due to the shift in California law) and we have monthly data for eight intersections. This enables us to run a regression with intersection fixed effects to get a second estimate of the response to a shift in the fine. The time series of violations per car for the largest intersection is presented in Figure 2. (The fine increase occurs during month 13, i.e. the month in which the big decline in violations/car starts.)

The largest data set we have is for Israel. In their effort to reduce traffic deaths, the Israelis have implemented a nation wide camera program over the past twenty years. As an additional measure to reduce violations, Israel raised the fine for running a red light from 400 shekels ($122) to 1000 shekels ($305) in December of 1996. We have 45 months of data, 1:1995-9:1998, across all 73 intersections in the country which have housings for the cameras. About half of the intersections, between 34 and 40, had an active camera in any given month. We use the Israeli data to run regressions with intersection fixed effects to estimate the drop in violations with respect to the fine increase.

In addition to the aggregate data for Israel, we also have two sets of micro data for Israeli drivers. First, we have a panel of 21,677 drivers who had a license in 1992. This is a random sample of 1% of Israeli drivers. (The police created the sample for us.) For each driver we have age, sex, marital status, year migrated to Israel, religion, and complete criminal and driving records. The criminal history includes number of arrests, indictments, and convictions for each of twelve different crimes. We aggregate these into property crimes, violent crimes, and white collar crimes. From the driving record we use the number and timing of red light violations, the number of speeding tickets, the number of failure to yield (yield sign) violations, and the number of convictions for driving under the influence of alcohol. In cases where the driver has committed a red light violation, we know the age of the driver's car.

Our second set of micro data is information on the full set of all red light tickets issued during 1992-1999 (the "ticket based sample"). This is a set of 221,870 tickets. For each observation we have same personal background, driving record, and criminal history as in the 1% sample of drivers. We also have unique driver ID numbers so that it is clear when the same driver is getting multiple tickets. Naturally there is some overlap between the sample of drivers and the population of tickets. The advantage of the former is that we can track the behavior of all drivers including those who do not receive red light tickets. The advantage of the latter is that we have a much larger data set given the entire population of tickets and we have age of car for every entry.

III. Empirical Framework

In the micro data, the number of red light tickets is modeled as a Poisson process. In other words, we assume that each driver has some fixed probability of a ticket during a short time period and that these short periods are independent trials. A driver's expected number or tickets during either the before or after period is given by:

(1) Expected number of tickets =exposure * exp($a + b_0$ *after).

"After" is a dummy for whether we are using an observation from before or after the fine increase. "Exposure" is the length of each time period. In this case there are 18 quarters for the before period and 14 quarters for the after period. The expected number of tickets in a single quarter in the before period is e^a. Expected tickets per quarter in the after period = e^{a+b_0}. We also run specifications that include right hand side controls for characteristics like male, married, or property criminal. And we interact the characteristics with "after" to allow for a differential response to the fine increase by each group. These equations have the following form:

(2) Expected number of tickets=

= exposure* exp[(a+ b_0 *after+ b_1 *male+ b_2 *(male*after))].

Finally, since we observe every driver before and after the fine increase, we can also estimate Poisson with individual fixed effects. This specification allows each driver to have her own base probability of a ticket. The main effects of male, property criminal and other individual characteristics are absorbed into the fixed effects, but we still can identify the coefficient on the interaction between each characteristic and "after." We also run the above regressions using OLS and OLS with fixed effects, rather than Poisson. (OLS results are in Appendix I).

IV. Results

The Response to a Shift in the Fine: Israeli Micro Data

We begin with results for the Israeli sample of drivers. We have a random sample of drivers and we have their driving and criminal records for 1992-1999. A large fine increase for red light violations was announced near the end June of 1996. We define a "before increase" period as being January 1992-June 1996. The "after" period is July 1996-December 1999. [11]

In Table 1, we see that the mean number of tickets per driver during the before period was .092.[12] 5.3 percent of the sample had been indicted for a property crime by 1992. 4.7 percent had been indicted for a violent crime and 3.4% for a white collar crime. Eighty-nine percent of the sample is Jewish while 76 percent are male and 81 percent are married. Fourteen and one half percent are aged 17-30 in 1992 and 26 percent are age 31-40. Four percent migrated to Israel within the past 20 years.

In Table 2, for various groups we show the mean number of tickets before and after the fine increase. The groups shown include the whole sample, the property

[11] We tried using other months, e.g. January 1997, as the first month of the "after" period. This did not make a significant change in the estimated elasticities.

[12] The raw decrease in number of tickets per driver is from .092 in the before period to .05 in the after period. This decrease does not control for the differing lengths of the two periods. Table 2 truncates the earlier period to 14 quarters and there we see that tickets per driver decreases from .073 to .050.

criminals, women, men, unmarried people, Jews, and non-Jews. For this table only, we truncate the before period (from below) so that it is the same length as the after period (i.e. 14 quarters). The table shows that drivers who have criminal indictments, or who are young, unmarried, or recently immigrated run more red lights than others. We also calculate an implied elasticity with respect to the fine increase using the change in tickets per driver and the fact that the fine increase was 150%.[13]

Table 2 shows that the elasticity of number of tickets with respect to the fine increase is -.21 with a standard error of .02. Property criminals have a slightly larger point estimate of the elasticity than non-criminals. However, this difference is not significant. Non-jews have an elasticity of -.10 which is significantly smaller than the -.23 elasticity for the Jews. People ages 17-30 have an elasticity of -.36 which is much larger than the -.16 for people older than 30. This latter difference is highly significant.

Figures 3 through 5 show the effects of the fine increase for the whole sample and for several subgroups. Figure 3 shows a time series of total number of tickets obtained by quarter. There is a downward break in the number of tickets after June 1996 (quarter 18), i.e. just after the fine increase was announced. Before and after the fine increase, there are several very large rises and falls that follow a seasonal pattern with tickets being lowest in the fourth calendar quarter and highest in the summer.[14] But ignoring the seasonal issue, the graph shows a downward shift in the series before and after June 1996 (quarter 18).

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[13] To obtain the standard errors for these elasticities, for each group we run a Poisson regression of number of tickets on a dummy for "after." The z-statistic on the incidence ratio for "after" is the z-statistic for the elasticity.

Figure 4 splits the sample by drivers ages 17-30 versus 31+ and shows tickets per driver per year over time. (The young drivers get so many more tickets per driver that we use a log scale to show the two lines on the same graph.) The figure shows that young drivers experience a big decrease in tickets starting in 1996. Older drivers also show some decrease in tickets per driver after the fine increase, though the effect is much smaller. Figure 5 shows similar time series for the Jews and the non-Jews in the sample. From 1992-1995, both groups have similar levels of tickets per driver. But, after the fine increase the Jews appear to have a larger decrease than do the non-Jews.

Table 3 uses the same sample of drivers and shows Poisson regressions of number of tickets on driver characteristics and driver characteristics interacted with the dummy for "after" the increase. In column (1), the coefficient on "after" is -.381 and is highly significant. In other words, after the fine increase, drivers experience roughly a 38% reduction in the number of tickets per quarter.[15] [The justification for this approximation is as follows: Suppose expected number of tickets = exposure*exp(-5.2 - .381*after). Take the natural log of both sides. When after=1, the fitted value for ln(tickets) is decreased by -.381.][16]

In column (2) we add driver characteristics. Drivers aged 17-30 in 1992 receive 73% more tickets relative to the base category of drivers age 51+. This difference is highly significant with a z-statistic of 13.4. Persons with speeding tickets, stop sign

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[14] In particular, there is a big drop in the number of tickets between quarters 15 and 16, prior to the fine increase. This drop is similar in magnitude to earlier seasonal drops and does not necessarily imply a break in the process.

[15] We control for the lengths of the before and after periods by setting exposure equal to 18 quarters and 14 quarters respectively. Thus our coefficients are interpreted as the effect on ln(number tickets in a given quarter).

[16] The true percentage decrease in number of tickets is 1-e^-.38 = -31.6%. We suggest the approximation to provide an easy interpretation of the Poisson coefficients.

tickets, criminal records, and those who have migrated to Israel within the past 20 years all receive substantially more tickets. (The immigrant effect could also be partially an age effect.) Property criminals receive 22 percent more tickets than non-criminals. The main effect of the dummy for Jewish is small and insignificant.

Column (3) adds interactions between driver characteristics and the dummy for the fine increase. (In other words, we allow the different types of drivers to have a differential response to the fine increase.) Only two of the interaction terms are statistically significant. Young drivers (age 17-30 in 1992) have a significantly larger response to the fine increase relative to drivers who are 51+. The coefficient on the interaction of after*young is -.635 which eliminates 2/3 the base difference between young and old drivers. Part of this large negative interaction term may stem from the fact that the young drivers age modestly between the before and after periods. (In the current draft, we do not report results controlling for this aging effect, but the aging is not enough to be driving the result.)

The other significant interaction term is for after*Jewish. Jews have a bigger response to the fine increase than do non-Jews. The total decrease in tickets for Jews is -.30 or about a 30% drop controlling for age and all other explanatory variables. The decrease in tickets for the non-Jews is only 3%. One interpretation of this result is that the non-Jews in Israel were less aware of the increase in the fine. Or the non-Jews may have less intention to pay traffic tickets they receive, so they have a lower price elasticity.

The most interesting result is that the interactions between "after" and the various crime dummies are insignificant. This indicates that drivers with criminal records have the same response to the fine increase as the non-criminals. This holds true for property criminals, violent criminals and white collar criminals. This result is consistent with the base elasticities in Table 2 which are similar for criminals and non-criminals. So, the criminals appear to have the same "rational" response to the fine increase as anyone else. One might expect that the criminals have less wealth and would therefore be more deterred by a fine increase. But this wealth effect could be offset by if the criminals are somewhat less likely to pay the fine.

Column (4) of Table 3 adds individual fixed effects. This allows us to estimate the coefficient on the interaction between after and individual characteristics while holding each person's probability of a ticket constant.[17] In the fixed effects specification, the interactions of after*Jew and after*young remain significant. The interactions between after and the crime dummies remain insignificant.

Tables 4 and 5 switch from the random sample of Israeli drivers to the entire universe of red light tickets between 1992 and 1999. Table 4 shows the mean of each driver characteristic (across the tickets) in the before and after periods. The means in Table 4 confirm the results from the driver sample. In the before period, 7% of the tickets are received by drivers indicted for a property crime. In the after period, 6.3% of tickets are given to property criminals. This modest decrease in the share of tickets shows that if anything, property criminals are slightly more sensitive to the fine increase relative to non-criminals.

[17] Poisson with fixed effects causes observations with no tickets in either period to drop out of the regression, so the sample size is reduced to 5,244 drivers.

The share of tickets issued to young drivers drops from 31% to 21% which confirms the larger elasticity for young drivers found earlier.[18] Jews see a modest decrease in their share of tickets following the fine increase (from 88% to 84%). This implies a fairly large percentage increase in the share of tickets for non-Jews from 12% to 16%.

The average age of the car used in the violation falls slightly from 6.1 years to 5.9 years from the before to the after period. We interpret this to mean that drivers with newer cars are somewhat less responsive to the fine increase. Though, another possibility is that average age of car driven in Israel is falling over time.

We transform the ticket based data to make the unit of observation an individual driver (instead of an individual ticket). The resulting data are the set of all drivers who received at least one red light ticket during the before period. For this group we model the number of tickets received during the before and after periods. The advantage of this approach (relative to our random sample of all drivers) is that we have a much larger sample of people, all of whom received at least one ticket. This allows us to obtain more precise estimates of the effects of the fine increase.

Table 5 shows a series of Poisson regressions using the above data. (These regressions are analogous to those in Table 3 but use the larger, differently selected sample.) In column (1), we see that the number of tickets drops by 34% in the after period. This coefficient is similar to the decrease of 38% found in column (1) of Table 3. Column (3) adds personal characteristics interacted with the dummy for "after." The coefficients on after* the three crime dummies are all negative and significant. The

[18] The ticket based sample allows drivers to enter and leave the sample as they age. So, the finding for

coefficients range from -.07 for the interaction of after with the property crime dummy to -.18 for the interaction of after with the white collar crime dummy. This indicates that criminals actually have a larger response to the fine increase than everyone else.

The interaction between after and age 17-30 is -.69 with a z-statistic of 43.6. This confirms that young people have a larger elasticity with respect to the fine increase[19]. The interaction between "after" and "car is less than 5 years old" is .12 with a z-stat of 13.5. This shows that holding driver characteristics constant, people with newer cars are less responsive to the fine increase. The simplest explanation for this finding is that wealthier people are less sensitive to the fine increase because the fine is a smaller percentage of their wealth or disposable income. Column (4) adds driver specific fixed effects. The coefficients are virtually unchanged from column (3).

Results Using Aggregate Data

Now we switch to examining aggregate data collected by police agencies in the US and Israel. In Table 6 we see how the total number of violations responded to the installation of cameras in Fairfax, Virginia. We look at violations per hour rather than per driver because we have exact information on hours of camera operation and only annual estimates on traffic flows. Town estimates show that traffic levels are roughly similar in the before and after periods, so the transformation to violations per hour should not matter much for our conclusions. In row 1, we see that violations per hour

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younger drivers is not an artifact of a fixed sample of drivers getting older.

fell by 45% in the camera intersections one year after the cameras were introduced. Violations per hour fell by 29% in non-camera intersections in Fairfax. This reflects the fact that the locations of the cameras are not public knowledge. Both drops are large and statistically significant. Virtually no drop is recorded in the control intersection; nor would we expect one. The diff in diff (treatment change minus control change) shows a 50% reduction in the camera intersections and a 34% reduction in the non-camera. Both drops are statistically significant.

Using a very rough approximation, we estimate the probability of apprehension before the program at 1.1% (see Table 2, row 6). This is the ratio of actual tickets hand written in 1996 to violations in 1997 *in the intersections* that were fully monitored pre-treatment. During the pre-treatment period, these intersections were monitored with a camera in 1997 (even though tickets were not being given). We assume that the number of violations in 1996 is identical to those recorded in the pre-treatment monitoring in 1997. This yields the estimate of a 1.1% chance of apprehension.

We then estimate how much the probability of apprehension rose from the installation of the cameras. We know that each camera can only cover one approach to an intersection. We assume that the presence of a camera raises the probability of apprehension for that approach to 100% but that drivers do not know which intersections and approaches are covered by cameras. The new probability of apprehension is estimated to be 1.1% + (# of cameras/ # major approaches in the city). This implies a new probability of apprehension of 3.6% which is 223% increase.

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[19] And the coefficients from the two samples are very similar.

The elasticity with respect to probability of apprehension is then either -.22 if you consider the drop in violations for the camera intersections or -.15 if you consider the drop for the non-camera intersections.

Table 7 repeats this exercise for the data from the Oxnard, California experiment. We switch from violations per hour to violations per car because we have different data from Oxnard. In the nine camera intersections, there is a drop of 44% in violations per car in the camera intersections and 54% in the non-camera intersections in Oxnard. The decrease in the control intersections is 5% and is not statistically significant. The diff in diff shows decreases of 39% and 49% in the camera and non-camera intersections respectively. All drops in camera and non-camera sites are statistically significant.

In Table 8 we examine how aggregate violations respond to an increase of 150% in the fine in Israel (from 400 to 1000 shekels) and 161% in California (from $104 to $271). Here the cameras function solely as a way to get complete monitoring of various intersections before and after the fine shift.

The first four columns of Table 8 present regression results using the Israeli data. In regression 2 we regress the log of violations per day on the log of the fine, using the single large shift in the fine to identify the coefficient. We use intersection months as the unit of observation and include intersection fixed effects. We estimate the elasticity of the violation rate with respect to the fine to be -0.17. When we aggregate up to quarterly data as in regression 1, we see that the elasticity of violations with respect to the fine is -0.21. In both regressions the elasticity is statistically significant.

In regression 3 we included the number of months since the camera was first installed in the intersection. The coefficient of this variable is intended to capture the degree to which drivers learn about the locations of the cameras and reduce violations in those specific intersections. This coefficient is -0.02 and is statistically significant.

In regression 4 we made an attempt to estimate the effect of an exogenous shift in the probability of an accident. For that purpose we included the number of intersecting roads (one or two) in each intersection. A larger number of intersecting roads creates a larger probability of having an accident while running a red light. The coefficient of this variable is negative and significant, which indicates that the violation rate falls with a higher probability of an accident.

In regression 6 in Table 8, we use the aggregate data in Oxnard to estimate the response to the fine increase and obtain an elasticity of -0.56. Naturally we worry about the fact that the shift in the fine came within a year after the cameras went into place. But as Figure 1 demonstrates, the drop occurs directly after the fine change. In San Francisco we have individual intersection data by month and so we are able to run a panel regression with intersection fixed effects. In the case of San Francisco (regression 5) we estimate elasticity of the violation rate with respect to the fine is -0.26.

Our results from aggregate data can be summarized as follows: Introduction of red light cameras that increased the probability of ticketing reduced the number of violations in both Oxnard and Fairfax to about one half of the pre-camera levels. Fine increases in California and Israel lowered the number of violations such that the elasticity of violations with respect to the fine hike is around –0.20. We have also found some indication that exogenous increase in the probability of an accident reduces the number of violations.

Additional Caveats and Interpretation of Results

Thus far we have only considered the one time cost of the traffic fine when calculating the percentage change in the cost of a red light ticket. In reality, there are other costs to receiving a ticket including increased insurance premiums, time costs, and feelings of guilt. If we include these other costs into our calculations of the elasticity, we would estimate higher elasticities (since the fine shift becomes a smaller percentage change of the base cost of a ticket).

For example, in California a single traffic ticket would raise the insurance premiums of the average driver by approximately $160 per year for three years.[20] At a discount rate of 8 percent, this is a present value of $445. The fine shifted from $104 to $271, but the total costs (fine plus insurance costs) shifted from $549 to $716 or a 30.4 percent increase. If we then run the same regression for San Francisco as in Table 8, but use the shift in total costs versus the shift in the fine only, we obtain as elasticity of violations with respect to total costs of -.94 (versus the -.26 for the fine only).

So, which number is the best estimate? We prefer the lower estimate for several reasons. First, the insurance costs may be overstated. Violators may use driving school to avoid the insurance costs, or may be relying on their parents to pay for car insurance, or may already be paying the maximum rate on their insurance due to previous violations. Secondly, the Israeli data do not contain this problem of insurance rates rising with tickets. And the various data sets from Israel deliver robust estimates of the fine elasticity that are in the -.20 to -.30 range.

Our analysis focuses on the reduction *in violations* that results from increases in fines and probability of apprehension. The percentage change in accidents and fatalities with respect to the fine may be different than the percentage change in violations. Suppose there are two types of violators. Type one violators are those who sneak through lights at the end of a cycle only when they are certain that there is no oncoming traffic. Type two violators are those who drive at high speed through an intersection with no concern for safety or the presence of drivers coming from intersecting roads. If Type one violators are the entire source of reduction in violations, but type two drivers cause all the fatal accidents, then violations may fall without affecting fatalities.

Existing studies indicate that the number of accidents and fatalities does fall along with the number of red light violations that occurs from the introduction of cameras. For example, the San Francisco Department of Parking and Traffic claims that red light enforcement provided a 42 percent reduction in violations per month and a 10 percent reduction in red light running accidents over a six month period. Oxnard experienced a 32 percent reduction in "front into side" collisions after cameras were introduced. (Insurance Institute for Highway Safety). These numbers confirm that accidents fall with violations, though it is unclear whether accidents actually decrease as much with violations (as they appear to in Oxnard) or fall by a smaller percentage (as in San Francisco).

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[20] This was estimated by USAA Property and Casualty. Note that the median driver may be different that the median violator.

A natural question to ask is whether the results imply anything about risk aversion, risk neutrality or risk loving behavior on the part of drivers. Our estimates of the elasticity of violations with respect the probability of getting caught (-.22) and the size of the fine (-.21 from Israel) are so similar that the numbers are certainly consistent with risk neutrality.[21] However, given the imprecision of the first elasticity estimate and the possible downward biases to the second estimate (discussed above), we are unable to reject risk aversion or risk loving behavior on the part of drivers.

V. Conclusion

We have used data from several experiments to show that additional deterrence is created both by increases in fines and by the probability of being caught. People's behavior seems quite similar across various cities in the US and in Israel. Interestingly, in comparison to non-criminals, people with criminal records are just as sensitive (or even more sensitive) to changes in the magnitude of the fine. This evidence supports the view that criminals make rational choices regarding law breaking activities and that criminals respond to incentives with a finite discount rate.

Young people and people with older cars respond to fine increases more than older people and people with new cars. This suggests that a driver's perception of the fine is relative to one's own wealth and that the optimal fine might be one based (in part) on the wealth of the perpetrator. Our results support the point made by Polinsky

[21] For example, if we have a 10 percent decrease in the fine coupled with a 10 percent increase in probability of apprehension so as to keep the expected fine roughly constant, there is no effect on violations.

and Shavell [1991][22] regarding deterrence, its relationship to wealth and the optimal level of fines. The simplest theory of deterrence would suggest that the socially optimal fine is a maximal one imposed with low probability as in Becker (1968). But if the level of the fine must be constant for all drivers and wealth varies greatly, the optimal fine may substantially less than maximal.[23]

Overall, the empirical work is quite supportive of the economic model of crime. The results offer further reason to believe that policy makers have effective tools at their disposal to combat crime and that changes in deterrence may be able to explain changes in crime rates.

[22] See also Garoupa [1998].
[23] Note that if the fine is larger than the wealth of the poorest people, an increase in the fine coupled with a decrease in probability of detection would reduce deterrence for those people.

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Table 1
Means in Israeli Sample of Drivers

Variable	Obs	Mean	Std. Dev.	Min	Max
number of red light tickets (before)	21677	0.092	0.324	0	5
property criminal	21677	0.053	0.223	0	1
violent criminal	21677	0.047	0.212	0	1
white collar criminal	21677	0.034	0.182	0	1
dwi indictment	21677	0.004	0.062	0	1
stop sign tickets	21677	0.707	1.064	0	12
jew	21608	0.891	0.311	0	1
male	21625	0.755	0.430	0	1
married	21677	0.812	0.391	0	1
age 17-30	21677	0.145	0.352	0	1
age 31-40	21677	0.263	0.440	0	1
age 41-50	21677	0.279	0.449	0	1
migrated <20 years ago	21677	0.042	0.200	0	1
speeding tickets	21677	1.333	1.804	0	27
age of car	2995	6.168	5.393	0	47

The data are from a 1% random sample of all Israeli drivers. The mean of red light tickets is .05 in the after period.

Table 2
Mean Number of Tickets By Various Groups
(Before Period Truncated to 14 Quarters)

The table shows the mean number of tickets (ie tickets per driver) during a 14 quarter period before the fine increase and a 14 quarter period after the fine increase. The rows show the means for various subgroups of the data including criminals, men and women, and various age categories.

	Before Increase	After Increase	Elasticity (std error)
All drivers	0.073	0.050	-0.21
			(0.02)
No criminal indictment --property	0.071	0.048	-0.22
			(0.02)
Criminal indictment-- property	0.119	0.074	-0.25
			(0.09)
Criminal indict--violent	0.102	0.078	-0.16
			(0.10)
Criminal indict -- white collar	0.095	0.093	-0.01
			(0.05)
Female	0.065	0.042	-0.24
			(0.04)
Male	0.076	0.052	-0.21
			(0.02)
Unmarried	0.093	0.057	-0.26
			(0.04)
Married	0.068	0.048	-0.20
			(0.02)
Not recent migrant	0.072	0.049	-0.21
			(0.03)
Recent migrant (20 yrs)	0.102	0.057	-0.29
			(0.04)
Non-jew	0.071	0.060	-0.10
			(0.07)
Jew	0.074	0.049	-0.23
			(0.02)
Age 31+	0.065	0.049	-0.16
			(0.03)
Age 17-30	0.123	0.056	-0.36
			(0.04)

Notes: Before and after periods are each 14 quarters long. Standard errors are computed by using the z- stat from a separate Poisson regression of number of tickets on a dummy for "after" for each group of drivers.

Table 3: Poisson Regressions

The table uses individual micro data on Israeli drivers. These are Poisson regressions in which the dependent variable is the number of tickets obtained by a given driver. The order of the regressions is as follows: 1 = baseline regression; 2 = adds driver characteristics ; 3= with interactions for characteristics ; 4= with interactions & person fixed effects.

	(1)	(2)	(3)	(4)
	number red lights	number red lights	number red lights	number red lights
after increase	-0.381	-0.380	-0.034	-0.059
	(10.03)**	(9.98)**	(0.18)	(0.31)
age 17-30		0.729	0.931	
		(13.38)**	(14.11)**	
age 31-40		0.202	0.261	
		(3.89)**	(4.00)**	
age 41-50		0.203	0.214	
		(4.07)**	(3.36)**	
criminal indictment property		0.221	0.289	
		(3.10)**	(3.27)**	
criminal indictment violent		0.180	0.159	
		(2.35)*	(1.64)	
criminal indictment white collar		0.158	0.060	
		(1.84)	(0.53)	
# of dwi		-0.260	-0.258	
(driving while intoxicated)		(0.90)	(0.89)	
jew		0.082	0.180	
		(1.38)	(2.35)*	
male		-0.102	-0.129	
		(2.20)*	(2.27)*	
married		-0.162	-0.146	
		(3.62)**	(2.67)**	
migrated <20 yrs ago		0.444	0.532	
		(5.68)**	(5.74)**	
no yield violations in 1992		0.241	0.241	
		(5.15)**	(5.14)**	
number speeding tickets in 1992		0.091	0.081	
		(11.84)**	(8.20)**	
stop sign tickets in 1992		0.185	0.185	
		(13.68)**	(13.69)**	
after *married			-0.047	0.008
			(0.50)	(0.09)
after*speeding tix			0.025	0.031
			(1.74)	(1.88)
after*age 17-30			-0.635	-0.645
			(5.39)**	(5.44)**
after*age 31-40			-0.160	-0.169
			(1.49)	(1.56)

after*age 41-50			-0.027	-0.039
			(0.27)	(0.37)
after*crime			-0.191	-0.151
			(1.27)	(1.00)
after*jew			-0.265	-0.280
			(2.16)*	(2.25)*
after*male			0.084	0.078
			(0.85)	(0.76)
after*migrate <20			-0.281	-0.289
			(1.63)	(1.66)
after*violent			0.052	0.059
			(0.33)	(0.37)
after*white collar			0.234	0.287
			(1.35)	(1.66)
after*dwi				-0.534
				(0.79)
after*stop sign violations				-0.022
				(0.70)
after*yield sign violations				0.077
				(0.78)
Constant	-5.276	-5.780	-5.909	
	(235.65)**	(64.49)**	(52.91)**	
Observations	43354	43210	43210	5244
Number of id				2622

Absolute value of z-statistics in parentheses
* significant at 5% level; ** significant at 1% level

Table 4
Means in Sample of Tickets

The table shows means of driver characteristics from the entire universe of red light tickets issued during 1992-1999.

	before					*after*	
Variable	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean
property criminal	143198	0.070	0.256	0	1	78672	0.063
violent criminal	143198	0.060	0.238	0	1	78672	0.054
white collar criminal	143198	0.035	0.185	0	1	78672	0.030
yield sign violations	143198	0.140	0.347	0	1	78672	0.124
car <5 years old	143180	0.484	0.500	0	1	78670	0.495
age of car	143180	6.139	5.448	0	61	78670	5.901
dwi	143198	0.005	0.072	0	1	78672	0.004
jew	142711	0.876	0.329	0	1	78450	0.841
male	143198	0.746	0.436	0	1	78672	0.753
married	143198	0.738	0.440	0	1	78672	0.685
migrated <20 years	143198	0.644	0.479	0	1	78672	0.682
age 17-30	112220	0.305	0.460	0	1	52334	0.213
age 31-40	112220	0.247	0.431	0	1	52334	0.232
age 41-50	112220	0.234	0.423	0	1	52334	0.265
age 51+	112220	0.214	0.410	0	1	52334	0.290
age	112220	39.875	13.948	16	94	52334	43.369

Table 5
Sample of All Drivers Who Received a Ticket
Poisson Regressions

Here we take the universe of tickets and create a sample in which the unit of observation is the individual driver. However, drivers are only in the sample if they received a red light ticket during 1992-1999.

1 =basic
2 = with characteristics
3= with additional characteristics on driving record and criminal record
4= interact after w/ characteristics
5= interact after w/ characteristics, add individual fixed effects

	(1) number red lights	(2) number red lights	(3) number red lights	(4) number red lights
after increase	-0.339	-0.508	-0.010	-0.019
	(76.36)**	(95.83)**	(0.37)	(0.74)
age 17-30		-0.015	0.202	
		(2.14)*	(23.02)**	
age 31-40		-0.005	0.116	
		(0.71)	(13.04)**	
age 41-50		-0.009	0.048	
		(1.31)	(5.32)**	
car < 5 years old		0.012		
		(2.41)*		
criminal indictment property		0.029	0.050	
		(3.00)**	(4.33)**	
criminal indictment violent		0.029	0.061	
		(2.85)**	(4.91)**	
criminal indictment white collar		-0.001	0.055	
		(0.12)	(3.68)**	
dwi		0.014	0.072	
		(0.42)	(1.87)	
jew		-0.011	0.071	
		(1.42)	(7.53)**	
male		0.013	-0.030	
		(1.94)	(3.76)**	
married		-0.022	0.026	
		(3.64)**	(3.61)**	
migrated <20 yrs ago		0.050	0.142	
		(4.70)**	(11.50)**	
number of no yield tickets in 1992		0.014	0.039	
		(2.13)*	(4.73)**	
number of stop sign tickets in 1992		0.018	0.028	
		(9.00)**	(11.59)**	
number speeding tickets in 1992		0.011	-0.001	
		(9.12)**	(0.41)	
after*age 17-30			-0.688	-0.691
			(43.62)**	(43.48)**
after*age 31-40			-0.350	-0.351

			(22.92)**	(22.91)**
after*age 41-50			-0.153	-0.153
			(10.33)**	(10.28)**
after*car < 5 years			0.121	0.163
			(13.50)**	(14.82)**
after*crime			-0.065	-0.064
			(3.11)**	(3.04)**
after*dwi			-0.195	-0.197
			(2.66)**	(2.67)**
after*jew			-0.268	-0.279
			(16.48)**	(16.94)**
after*male			0.148	0.154
			(10.14)**	(10.47)**
after*married			-0.159	-0.161
			(12.28)**	(12.32)**
after*migrate <20			-0.340	-0.355
			(13.94)**	(14.47)**
after*no yield violations			-0.078	-0.080
			(5.37)**	(5.47)**
after*speeding tix in 92			0.030	0.029
			(12.22)**	(11.63)**
after*stop sign tickets			-0.031	-0.032
			(7.37)**	(7.47)**
after*violent			-0.097	-0.100
			(4.36)**	(4.46)**
after*white collar			-0.176	-0.177
			(6.53)**	(6.50)**
Constant	-3.221	-3.184	-3.363	
	(1213.58)**	(258.44)**	(227.11)**	
Observations	395116	290870	290870	290870
Number of id				145435

Table 6
Reduction in Violations, Implied Elasticities From Fairfax, VA Experiment

Mean (Violations Per Hour) Across Intersections				
		Before	*1 year After*	*Percentage Change*
Camera		0.59	0.33	-45%
		(0.13)	(0.09)	(9%)
	N	5.00	5.00	5.00
Non-camera		1.96	1.35	-29%
		(0.58)	(0.29)	(6%)
	N	2.00	2.00	2.00
Control		0.36	0.37	5%
		(0.05)	(0.02)	(9%)
	N	2.00	2.00	2.00
Difference in Difference				
Camera minus Control				-50%
				(16%)
Non-camera minus control				-34%
				(11%)
Probability of Apprenhension				
tickets per violation		0.011	0.036	223%
		(0.005)		
Elasticity of Violations per hour wrt Δ Prob				
Camera minus Control				-0.22
Non-camera minus control				-0.15

Notes:
Data provided by City of Fairfax Dept. of Public Works and Police Dept; Insurance for Highway Safety.
Intersection specific data are provided in Retting et. al. 1998. We have added standard errors, estimates of the probability of apprehension and estimates of the elasticity.
Standard errors shown in parentheses. Violations per hour are averages across intersections.
Standard error shown is std(across intersections) / (n^.5)
Standard error for diff in diff is calculated as the standard error for a difference in means.
Probability of apprehension before = [annual number of tickets written (hand enforcement) at the 5 intersections used as "camera" intersections] / [annual number of violations -- estimated from "before" data collected during experiment]

Table 7
Reduction in Violations, Implied Elasticities From Oxnard, CA Experiment

Mean (Violations Per 10,000 Cars) Across Intersections		*Before*	*4 months After*	*Percentage Change*
Camera		14.35	8.38	-44%
		(1.72)	(1.37)	(5%)
	N	9.00	9.00	9.00
Non-camera		16.40	7.40	-54%
		(6.13)	(3.22)	(8%)
	N	3.00	3.00	3.00
Control		7.00	6.70	-5%
		(0.20)	(1.20)	(14%)
	N	2.00	2.00	2.00
Difference in Difference				
Camera minus Control				-39%
				(13%)
Non-camera minus control				-49%
				(19%)
Probability of Apprehension				
tickets per violation				200%
Elasticity of Violations per hour wrt Δ Prob				
Camera minus Control				-0.20
Non-camera minus control				-0.25

Notes:
Data are from Oxnard Police Dept and Retting et. al. 1999 which contains raw data for each intersection.
Standard errors shown in parentheses. Violations per car are averages across intersection.
Standard error shown is std(across intersections) * 1/ (n)^.5
Standard error for diff in diff is calculated as the standard error for a difference in means.

Table 8
Regression of Violations Per Day On Indicators For Shift in Fine
Includes Intersection Fixed Effects

	Log(viol./day) quarterly Israel	*Log(viol./day) monthly Israel*	*Log(viol./day) monthly Israel*	*Log(viol./day) monthly Israel*	*Log(viol./car) monthly San Fran*	*Log(viol./day) monthly Oxnard*
log(fine)	-0.21	-0.17	-0.17	-0.16	-0.26	-0.56
	(-2.66)	(-2.82)	(-2.70)	(-1.70)	(-1.62)	(-1.95)
time trend	-0.05	-0.01	0.01	-0.01	-0.01	-0.04
	(-5.43)	(-6.22)	(0.92)	(-4.08)	(-1.53)	(-2.26)
months since camera first installed in intersection			-0.02			
			(-3.21)			
number intersecting roads				-0.35		
				(-2.09)		
constant	2.65	2.36	2.17	2.91	1.65	5.06
	(5.85)	(6.63)	(6.03)	(4.62)	(2.22)	(3.54)
R-squared	.12	.08	.14	.13	.05	.56
N (time periods*intersections)	589	1,519	1,519	675	124	21

Notes:
T-statistics in parentheses.
Regressions 1-4 are OLS with intersection fixed effects on Israeli data.
Fine shifts in 1/12/96 from 400 shekels to 1,000 shekels.
Number of intersections is 73 in regressions 1-3, 32 in regression 4 (rural intersections only) and 8 in regression 5.
Regression 5 is OLS with intersection fixed effects on San Francisco, CA data.
Regression 6 is OLS on aggregate Oxnard, CA data.
Fine shifts in CA from $104 to $271 on 1/1/98.

Appendix Table 1: OLS Regressions with Israeli Drivers Sample

1 = baseline
2 = with characteristics
3= with additional characteristics on driving record and criminal record
4= interact after w/ characteristics
5= interact after w/ characteristics, add individual fixed effects

	(1)	(2)	(3)	(4)	(5)
	number red lights	number red lights	number red lights	number red lights	number red lights
after increase	-0.043	-0.043	-0.043	-0.001	-0.001
	(15.86)**	(15.92)**	(15.96)**	(0.11)	(0.11)
age 17-30		0.049	0.057	0.100	
		(11.27)**	(12.99)**	(16.07)**	
age 31-40		0.008	0.013	0.022	
		(2.13)*	(3.60)**	(4.21)**	
age 41-50		0.011	0.013	0.016	
		(2.99)**	(3.55)**	(3.27)**	
criminal indictment property		0.030	0.021	0.034	
		(4.81)**	(3.35)**	(3.83)**	
jew		0.004	0.006	0.018	
		(1.00)	(1.44)	(2.75)**	
male		-0.004	-0.010	-0.014	
		(1.33)	(2.88)**	(2.92)**	
married		-0.014	-0.013	-0.015	
		(3.93)**	(3.56)**	(3.04)**	
migrated <20 yrs ago		0.037	0.037	0.060	
		(5.38)**	(5.33)**	(6.17)**	
number speeding tickets in 1992		0.013	0.010	0.010	
		(17.36)**	(12.40)**	(9.26)**	
criminal indictment violent			0.014	0.015	
			(2.06)*	(1.62)	
criminal indictment white collar			0.019	0.010	
			(2.53)*	(0.96)	
dwi			-0.026	-0.016	
			(1.16)	(0.53)	
noyield			0.020	0.023	
			(5.11)**	(4.18)**	
stoptix			0.018	0.024	
			(13.49)**	(12.68)**	
Adwi				-0.018	-0.018
				(0.42)	(0.43)
Amarr				0.005	0.005
				(0.74)	(0.76)
Aspeed				-0.001	-0.001
				(0.67)	(0.69)

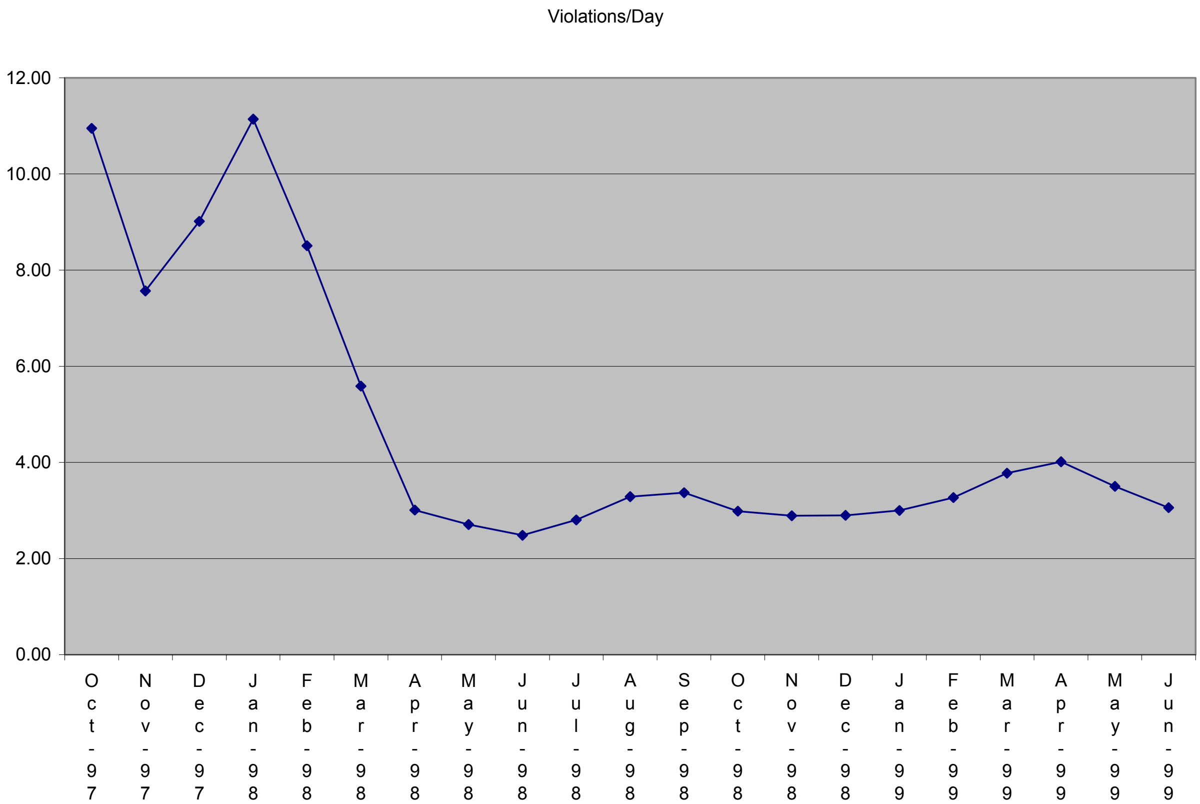
Astop				-0.012	-0.012
				(4.41)**	(4.55)**
Ayield				-0.006	-0.006
				(0.80)	(0.82)
after*age 17-30				-0.085	-0.085
				(9.72)**	(10.02)**
after*age 31-40				-0.017	-0.017
				(2.35)*	(2.43)*
after*age 41-50				-0.008	-0.008
				(1.08)	(1.11)
after*crime				-0.026	-0.026
				(2.06)*	(2.12)*
after*jew				-0.022	-0.022
				(2.45)*	(2.52)*
after*male				0.008	0.008
				(1.24)	(1.28)
after*migrate <20				-0.047	-0.047
				(3.40)**	(3.50)**
after*violent				-0.003	-0.003
				(0.22)	(0.23)
after*white collar				0.018	0.018
				(1.18)	(1.22)
Constant	0.092	0.070	0.056	0.035	0.092
	(47.87)**	(10.48)**	(8.32)**	(3.76)**	(49.84)**
Observations	43354	43216	43216	43216	43216
R-squared	0.01	0.02	0.02	0.03	0.54

Appendix Table 2
Israeli Driver's Sample Without Trucation of Before Period
Mean Number of Tickets By Various Groups

	Before Increase	After Increase
All drivers	0.092	0.050
No criminal indictment --property	0.089	0.048
Criminal indictment-- property	0.138	0.074
Criminal indict--violent	0.123	0.078
Criminal indict -- white collar	0.115	0.093
Female	0.084	0.042
Male	0.095	0.052
Unmarried	0.115	0.057
Married	0.087	0.048
Not recent migrant	0.09	0.049
Recent migrant (20 yrs)	0.144	0.057
Non-jew	0.088	0.060
Jew	0.093	0.049
Age 17-30	0.159	0.056

Notes: Before period is 18 quarters long and after period is 14 quarters long

Figure 1
Violations Per Day in Oxnard, CA

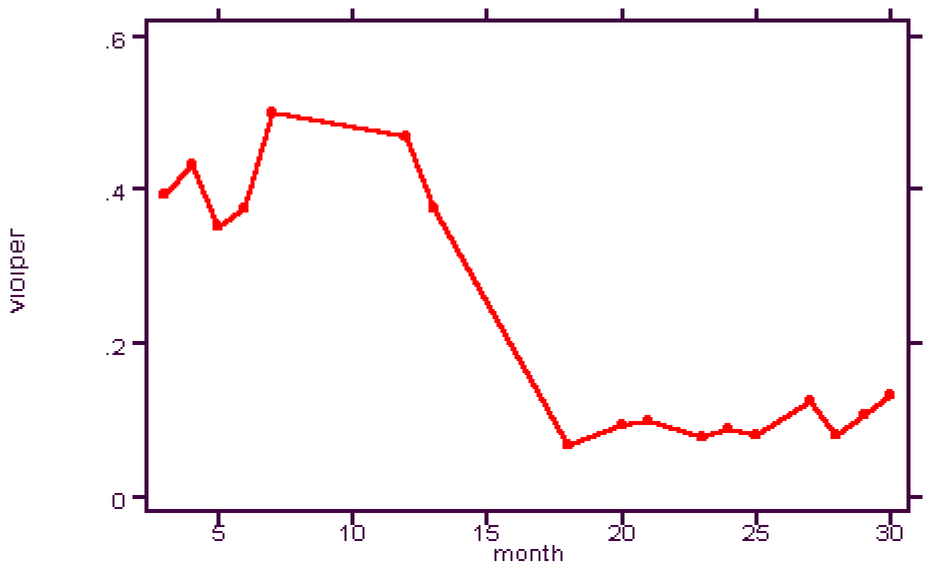


Notes: Violations per day = (total violation all intersections / hours of observation all intersections) * 24
Data are from Oxnard Police Dept. and Martin Marietta. Fine increased from $104 to $271 on Jan 1, 1998.

Figure 2
Violations Per Car in San Francisco, CA

Violations Per

1,000 Cars



Data provided by San Francisco Department of Parking and Traffic. Data are from the camera at 19th and Sloat southbound which is the highest volume intersection in the data set. Fine increased from $104 to $271 on Jan 1, 1998 (month 13).

Figure 3
Number of Red Light Tickets Per Quarter: Israeli Drivers Sample

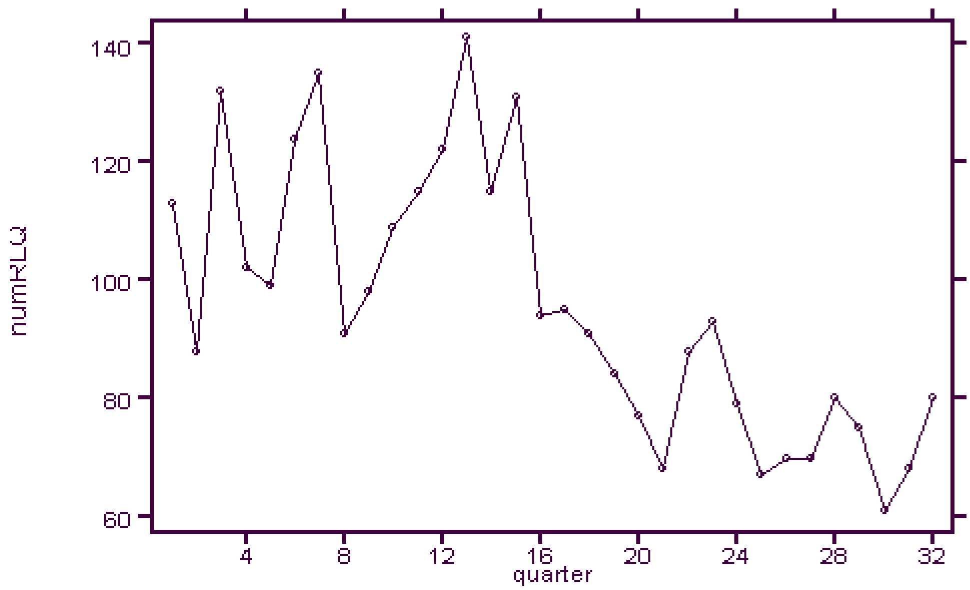


Figure 4
Red Light Tickets Per Driver Per Year: Israeli Drivers Sample
Bold line Drivers 17-30, Thin Line: All Others

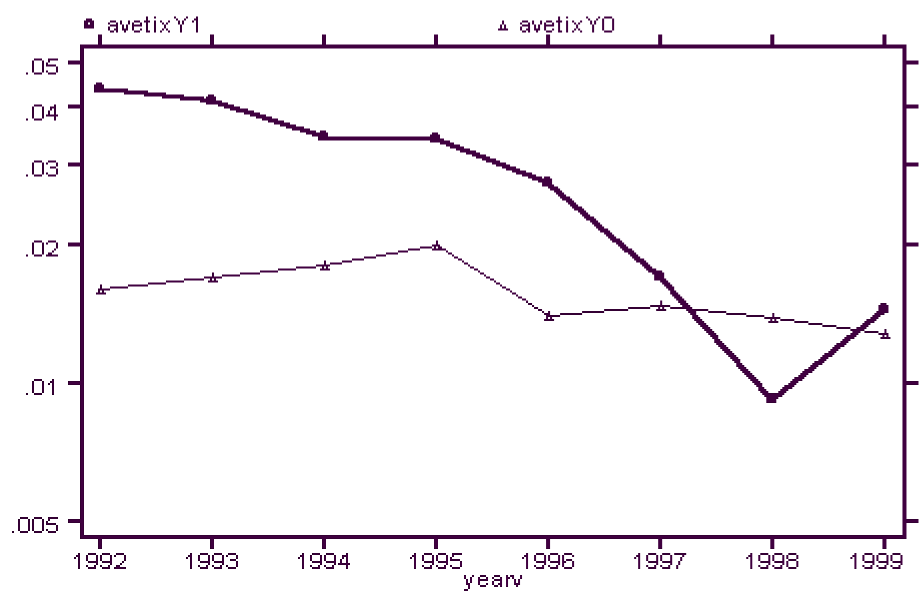


Figure 5
Red Light Tickets Per Driver Per Year: Israeli Drivers Sample
Bold line: Jews, Thin Line: All Others

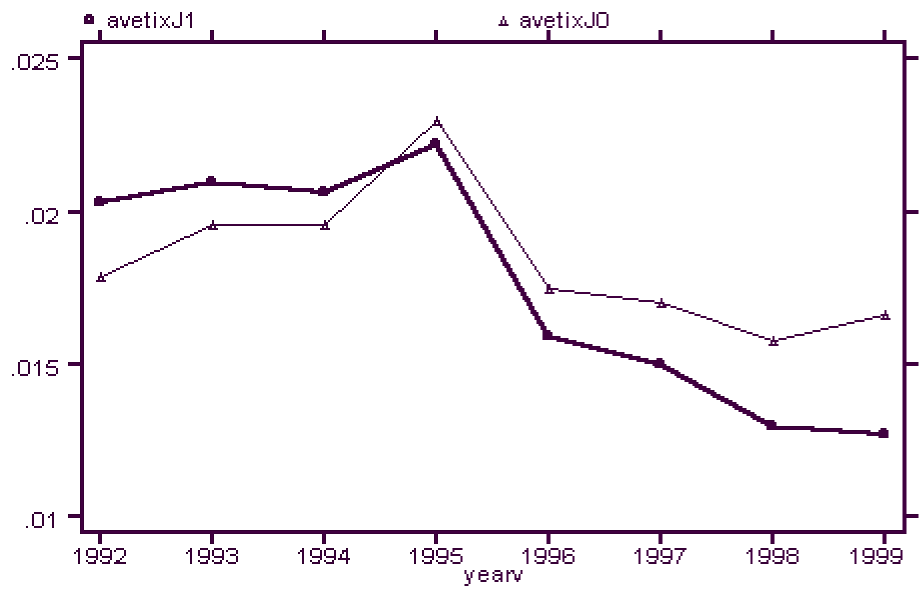


Figure 6
Number of Red Light Tickets Per Quarter: Israeli Tickets Sample