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Testing the Economic Model of Crime:The National Hockey League's Two-Referee Experiment

Steven D. Levitt*

*University of Chicago Department of Economics, slevitt@midway.uchicago.edu

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Abstract

During the 1998-99 season, the National Hockey League randomly varied the number of referees used across games, seemingly providing a rare opportunity to test directly the deterrence model. Combining experimental parameter estimates with an economic model, there is little evidence that the rate of offending changed substantially with the addition of a second referee. The reason, however, appears to be that the second referee had little impact on the probability of punishment. As a consequence, the experiment ultimately turns out to be of limited use for testing deterrence.

KEYWORDS: deterrence, economic model of crime, crime, hockey

I. Introduction

In the three decades since the seminal work of Becker (1968), an enormous literature has arisen on the economic model of crime. The vast majority of this research has been theoretical in emphasis (e.g. Stigler 1970, Posner 1977, Polinksy and Shavell 1984, Andreoni 1991, Kaplow and Shavell 1999). Less progress has been made in empirically testing the economic model of crime, although there are a number of notable attempts to do so (e.g. Ehrlich 1973, Witte 1980, Cameron 1988, Tauchen, Witte, and Griesinger 1994) . One major difficulty in testing the Becker (1968) model and its numerous extensions is that many of the predictions of the model are empirically indistinguishable from other competing models. For example, except under special circumstances, it is difficult to separate deterrence (the basis of the economic model) from incapacitation effects (a reduction in crime that arises mechanically because criminals are behind bars, rather than due to a behavioral response to changing incentives).[1] Thus many supposed tests of the economic model of crime have little power to discriminate between competing models. A second difficulty that arises in testing the economic model of crime is identifying exogenous sources of variation in the criminal justice system that are necessary to identify a causal link between policies and changes in crime rates. The early empirical literature in this area, lacking exogenous variation, yielded results that are difficult to interpret and have been harshly criticized (Fisher and Nagin 1978, Cameron 1988).[2]

In this paper, I follow the lead of McCormick and Tollison (1984), using the quasi-

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[1] For special cases where it may be possible to distinguish deterrence and incapacitation, see, for example, Landes (1979), Kessler and Levitt (1999), and Levitt (1998).

[2] More recently, some progress has been made in this area. For instance, on the question of whether more police reduce crime, a series of papers using a range of different approaches have all come to a similar conclusion that more police substantially reduce crime (Marvell and Moody 1996, Levitt 1997, Corman and Mocan 2000).

experimental setting of athletic contests to investigate the empirical support for the economic model of crime.[3] McCormick and Tollison (1984), in their classic paper, exploit a between-season change in the number of referees assigned to college basketball games. Referees serve as "police" in their model, with fouls called the sports-equivalent of arrests. McCormick and Tollison find that the number of fouls called declines substantially (over 30 percent) when the number of officials is increased from two to three. This seemingly surprising result is, in fact, strong evidence in favor of the economic model of crime. As the probability of detection rises, criminals respond by committing fewer offenses, thus the actual number of arrests may fall, even though *arrests per crime* rise. Note, however, that McCormick and Tollison are unable to identify the parameter of greatest interest: the impact of additional referees on the number of offenses committed.

The particular "natural experiment" exploited in my paper is a rule change in the National Hockey League (NHL) that increases the number of referees from one to two.[4] The NHL case, however, has a number of important advantages over the earlier work of McCormick and Tollison (1984). First, the change in NHL refereeing is done is a truly-experimental form. As a means of evaluating the impact of changing the number of referees, NHL owners voted to play some games in the 1998-99 season with one referee and some games with two referees. The assignment of these games is essentially random. Moreover, since the games are all drawn from a single season of play, all other rules, as well as the identities of the players and coaches,

[3] Professional sports has proven to be a fruitful laboratory for testing economic models in a range of other applications also. These include labor economics (Ehrenberg and Bognanno 1990), tests of game theory (Chiappori et al 2000), and corruption (Duggan and Levitt 2000).

[4] In a recent paper written after this paper was originally submitted, Heckelman and Yates (2001) also analyze the NHL experiment. They find similar results to my paper, but like McCormick and Tollison (1984), make no attempt to use an economic model to identify the structural parameter of interest.

are held constant. The only thing that changes is the number of referees on the ice in a particular contest. In contrast, in McCormick and Tollison's earlier work, they had to rely on data across different years, during which time players, coaches, and other rules would have changed. A second advantage of this paper is that by combining the experimental estimates with a reasonable economic model, I am able to back out an estimate of the impact of added referees on the rate of total offenses committed. It is this parameter, rather than the elasticity of offenses detected by the referees (which McCormick and Tollison estimate), that is of greatest interest. A third advantage of the NHL case is that game-by-game data is readily available, allowing for an examination of any learning that takes place over time. Finally, in hockey there are different types of penalties called. While most penalties occur within the natural flow of the game's action (e.g. tripping or hooking), fights also take place. When a fight breaks out, the probability of detection by the referee is one. Thus, for the fights, one would not expect a direct deterrent effect of more referees.

The results obtained for hockey differ substantially from the earlier NCAA basketball findings of McCormick and Tollison (1984). With the addition of a second referee, the number of penalties *called* in the NHL actually rises slightly, in contrast to the dramatic decline in basketball. Under the assumption that teams are behaving optimally and all penalties are defensive rather than offensive in nature, I estimate that the total number of offenses *committed* is essentially unchanged, but so is the probability of detection.[5] Thus, it appears that the lack of behavioral response is the result of the second referee having little impact on the expected cost of offending, rather than a refutation of the deterrence model.

The remainder of this paper is structured as follows. Section II briefly describes the

[5] This result cannot be attributed to the second referee having no impact on the probability of detection since the number of penalties called rises with the additional referee.

game of hockey and the particulars of the NHL's experiment using two referees. Section III develops the theoretical model. Section IV presents the empirical findings. Section V concludes.

II. The National Hockey League's Two-referee Experiment

Hockey is a game played on ice wearing ice skates. Two competing teams are allowed six players at a time on the ice. The object is to score goals by propelling a rubber puck into the opponent's net. One point is awarded for each goal. The team with the most points at the end of three twenty-minute periods is the winner. If the game is tied, an overtime period is played. The first team to score in overtime wins. If no score occurs in overtime, the game is declared a tie.

A wide range of penalties may be called on players. Two other officials, known as linesmen, are also on the ice, but do not call penalties. "Minor" penalties include tripping, slashing, hooking, high-sticking, and a range of other infractions. These penalties require the offending player to spend two minutes in the penalty box, during which time his team plays shorthanded. If the opposing team scores while the penalty box is occupied, any remaining penalty time is waived off and the two teams return to full strength. "Major" penalties are given for the most serious infractions such as fighting and actions that draw blood from another player. Such penalties lead to five-minutes in the penalty box and the remaining time is not waived off if the other team scores. Only the referee has the authority to call penalties.

The cost of receiving a penalty is substantial. Each penalty called costs a team about .17 goals in expectation.[6] On average, teams score slightly more than three goals over the course of

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[6] Assuming two teams are of equal ability, the expected scoring differential over any fixed-period of time is zero when the teams are at equal strength. Teams on a power play score an average of 17 percent of the time. Shorthanded goals are extremely rare. Thus, the average cost of having a penalty called is approximately .17 goals.

a game.[7] Nonetheless, the behavior of hockey players and team management suggests that the type of behavior associated with penalties also confers substantial benefits. Penalties may derail an opposing team's scoring opportunity, intimidate or injure an opponent, or serve as a deterrent to the other team attempting to injure a team's star players. Some players, known as "enforcers" or "goons" appear to have collecting penalties as their primary role.[8] Penalties are frequent: an average of over ten minor penalties are called per game, and roughly one major penalty. Thus, about one-third of the typical game has players in the penalty box.

The NHL is the premier professional hockey league in North America. It has been in existence since 1917. Almost 20 million fans attend games in a typical season. In the 1998-99 season, there were 27 teams, each playing 82 regular season games. Prior to the 1998-99 season, NHL team owners voted to undertake an experiment in which each team played twenty (ten at home, ten on the road) of its games with two referees rather than one. All of the two-referee games were concentrated in the first half of the season to avoid possible controversy during the playoff-stretch run. Subject to these constraints, the assignment of one versus two referees to a game was for all intents and purposes random. With the exception of the number of referees, all other rules remained constant during the season. The full schedule of games and the number of referees for each game were made available prior to the start of the season. A handful of pre-season games were also played with two referees, so players did have some prior exposure. Following the completion of the season, the two-referee experiment was judged a success and all

[7] Thus, the cost of a penalty in hockey is far greater than it is in basketball, where depending on the specific circumstances, possession of the ball is awarded to the other team out of bounds, or up to three free throws are granted. Note, however, that in basketball a player is expelled from the game after obtaining a certain number of fouls. This is not the case in hockey.

[8] For instance, in the 1999-2000 season, Gordie Dwyer, a left-winger for Tampa Bay, tallied 135 penalty minutes, but had no goals and only one assist the entire season.

games since that time have had two referees.

III. A Simple Model for Estimating the Deterrent Effect of an Additional Referee

The NHL's experiment provides direct estimates of the percent changes in the number of offenses called and scoring rates, but does not directly identify the structural parameter of greatest interest, namely the responsiveness of total offenses to the addition of a referee. With additional assumptions, however, it becomes possible to back out that key parameter. The necessary assumptions are:

(A1) Hockey teams are behaving optimally with respect to committing penalties,

(A2) All penalties are committed defensively,

(*A3) Penalties are additively separable in the production function for goals*, and

(*A4) For a fixed number of referees, there is a constant probability P that any offense will be detected and a penalty will be called.*

Justification for the second assumption comes from casual observation of the game and from the fact that the greatest benefit to committing a penalty generally occurs in stopping an otherwise high probability scoring opportunity for the opponent. The bias induced if some penalties are not defensive is easily signed and is discussed below. The third assumption allows one to abstract from other possible margins along which teams alter behavior in response to changes in the number of referees (e.g. the amount of effort devoted to offense versus defense).[9]

An optimizing team commits offenses up to the point where the marginal benefit of the penalty (i.e., the immediate expected reduction in the probability the opponent scores) equals the

[9] To the extent that assumption *(A3)* is violated, penalties are likely to be substitutes for other inputs, leading the model presented here to overstate the change in the number of goals scored with the addition of a second referee.

marginal cost of the penalty (i.e., the probability that a penalty is called multiplied by the expected cost in goals of playing with a man in the penalty box).[10] Stated formally, offenses are committed up to the point that

(1) $$PC = -B$$

where P is the probability a penalty is called, C is the expected cost in goals of playing short-handed after a penalty is called, and B is the immediate benefit of committing the penalty (in opponent goals prevented).

With the addition of a second referee, the probability of detection presumably rises and some fouls that previously were worth committing are no longer cost beneficial. Under the assumption that all penalties are defensive, any reduction in fouls committed will result in higher rates of opponent scoring. Denoting ΔO as the change in offenses per minute when the two teams are at even strength and a second referee is added and ΔG as the change in the number of even-strength goals per minute with the second referee,[11] then

(2) $$\Delta OB \approx \Delta G$$

where the approximate equality in (2) is exact if B (the marginal benefit of the penalty) is

[10] Alternatively, one could model the marginal costs and benefits of penalties in terms of the impact on whether a team wins, loses, or ties the game. Note that the only reason this might make a difference is that the benefit to committing a penalty is to reduce the chance that the other team scores, whereas being shorthanded both reduces the chance that your team scores as well as increasing the chance the opponent scores. Thus, it is more costly to commit a penalty when trailing by a goal than when leading by a goal.

[11] An equation parallel to (2) also holds during power plays. I focus on the even-strength setting for simplicity of exposition. Note that if one fails to separately analyze even-strength and power play scoring rates separately, the results may be misleading do to a change in the fraction of time at even strength when a second referee is added. For instance, if the amount of time at even strength goes down with a second referee and there is more scoring during power plays, it will appear that scoring rates rise with the addition of the referee, simply due to the fact more time is spent on the power play.

constant over the relevant range. Equation (2), which simply says that the reduction in offenses multiplied by the benefit per foregone offense adds up to the change in scoring, follows directly from assumption *A2*.

Combining equations (1) and (2) and solving for *B* yields

$$(3) \qquad \Delta O \approx -\frac{\Delta G}{PC}$$

Although ΔG and C are both estimable, that nonetheless leaves two unknowns in equation (3): ΔO and P. Thus, the change in the number of offenses committed is not identified in this model. Note, however, that the percent change in the number of offenses can be identified. By assumption *(A4)* the probability of detection is equal for the marginal and average offense committed. Define *PENRATE* to be the number of penalties called per minute (i.e., $P*O$). Dividing both sides of equation (3) by O – the number of offenses per minute – eliminates one of the unknowns

$$(4) \qquad \frac{\Delta O}{O} \approx -\frac{\Delta G}{C * PENRATE}$$

The left-hand-side of equation (4) is the percent change in offenses with the addition of a second referee. All three of the values on the right-hand-side of the equation are either directly observable (*PENRATE*), easily calculated from available data (C), or estimated in a straightforward manner using the randomized experiment (ΔG).[12]

[12] To the extent that some penalties are committed on offense to enhance a team's chance of scoring, as opposed to the maintained assumption that penalties are done on defense to lower the other team's scoring likelihood, ΔG may understate the true benefit foregone after the addition of a second referee. Thus, equation (4) provides a lower bound on the true responsiveness of offending to the change in referees.

Having calculated the percent change in offenses, it is then possible to estimate the percent change in the probability of detection using the formula

$$(5) \qquad \frac{\Delta P}{P} \equiv \frac{\Delta(PENRATE / O)}{PENRATE / O} \approx \frac{\Delta PENRATE}{PENRATE} - \frac{\Delta O}{O}$$

where the approximation in equation (5) is close for small changes in the probability of detection. Substituting for $\Delta O/O$ in equation (5) using equation (4) yields

$$(6) \quad \frac{\Delta P}{P} = \frac{\Delta PENRATE}{PENRATE} + \frac{\Delta G}{c * PENRATE} = \frac{c * PENRATE + \Delta G}{PENRATE}$$

All of the terms on the right-hand side of equation (6) are observable in the data. Thus, equations (5) and (6) provide a means of backing out the structural parameters of interest under the maintained modeling assumptions. These structural parameters are useful for two reasons. First, they allow one to compute the elasticity of greatest interest: the response of illegal acts to changes in the probability of detection. Second, knowing these parameters actually makes it possible to *reject* the deterrence model. In particular, deterrence predicts that an increased probability of punishment leads to a reduction in the number of offenses committed. By equation (6), the probability of punishment rises if $c*\Delta PENRATE+\Delta G>0$. By equation (4), the number of offenses must fall if $\Delta G>0$ Thus, the deterrence model is rejected if $c*\Delta PENRATE+\Delta G>0$ (punishment rises) and $\Delta G<0$ (scoring falls, which implies that total offenses rose). In contrast, in the reduced form approach of McCormick and Tollison (1984), the deterrence hypothesis can *never* be rejected.because the outcome measure (number of penalties actually called or *PENRATE*) can rise or fall with an increase in the probability of punishment, depending on the underlying elasticity of offenses actually committed with respect to the probability of punishment.

IV. Empirical Findings

In this section, I present the results of the two-referee experiment, along with other estimates and calculations needed to solve the model presented in the preceding section. To make the comparison between the two sets of games as similar as possible, only one-referee games played during the interval in which some games had two referees (October 16, 1998 to February 28, 1999) are included in the one-referee game calculations. Roughly one-third of all games during this time period had two referees.

Table 1 presents a comparison of means for games with one versus two referees. Because the introduction of a second referee was done in a quasi-experimental way, this simple comparison should provide consistent estimates of the impact of adding a referee. The top two row presents results for minor penalties. In games with two referees, an average of 10.90 minor penalties were called per game, compared to 10.33 with one referee. The difference of .57 penalties per game – a six percent increase – is borderline statistically significant at the .05 percent level. Thus, adding a referee is associated with *more* minor penalties being called, in stark contrast to the earlier results for basketball from McCormick and Tollison (1984).[13] The economic model of crime has an ambiguous prediction on this coefficient, so it does not provide conclusive evidence for or against the model.

The second row of Table 1 presents a comparison of means for major penalties. Offenses leading to major penalties are almost certain to be called even if there is only one referee. Thus,

[13] In the basketball setting, McCormick and Tollison found elasticities on the order of -.60. The implied elasticity of penalties with respect to the number of referees in the hockey application is .06.

there is unlikely to be any direct deterrence effect of adding a second referee. One might reasonably conjecture, however, that major penalties often arise as the result of an escalation following minor illegal acts, particularly those that go unpunished. With two referees, perhaps fewer minor infractions go undetected, resulting indirectly in fewer fights. Indeed, the point estimate suggests a decline in the number of major penalties when a second referee is introduced, although the difference between games with one and two referees is again not statistically significant at conventional levels.

If deterrence is present and penalties arise in defensive situations as assumed above, the economic model of crime predicts that an increased probability of detection for illegal acts will unambiguously increase scoring since the number of penalties *committed* (as opposed to the number of penalties called) unambiguously declines. The second and third rows of Table 1, however, show little evidence to support that prediction. Even-strength scoring rates per minute fell slightly with the addition of a second referee, from .086 to .084. Power-play goals per minute rose marginally (.103 to .108). In neither case is the difference statistically significant. Moreover, the absolute magnitude of these changes in scoring rates is small. Roughly 45 minutes of the typical game is played at full strength, implying a per game decrease in even strength scoring of .09, holding constant the number of minutes at full strength. The increase in scoring on power plays (.08 goals per game) makes the total impact on scoring close to zero.

The results in Table 1 rest on the assumption that the assignment of games to one versus two referees is random. As a check on these results, Table 2 presents estimates including a range of covariates in an attempt to control for any non-randomness. The first column of the table simply reproduces the results from Table 1, with standard errors in parentheses. Column 2 adds a linear time trend to absorb any systematic fluctuations in penalties or scoring over the course of the season. Column 3 adds team-fixed effects to eliminate any differences across teams, and column 4 adds interactions between the two teams to capture any idiosyncratic circumstances that arise when two teams play one another. The identification of the coefficients in column 4 comes exclusively from a comparison of differences in outcomes involving the exact same two teams, but with a change in the number of referees. Since two teams play one another an average of less than four times per year and the experiment covered only about one half of the season, there are relatively few cases from which to identify these parameters. Only the point estimate on the indicator variable for the presence of two referees is included in the table.

The coefficients in the first three columns of Table 2 are virtually identical for each of the outcome variables, as would be expected if the number of referees were randomly assigned. The one minor exception is that in column 4, the presence of two referees is associated with a slight increase in even-strength scoring instead of a slight decrease. The point estimate remains statistically insignificant. Taken as a whole, there is little in this table to alter the basic conclusions from the simple comparison of means in Table 1.[14]

Combining the estimates in Tables 1 and 2 with information on the expected cost of an illegal act, equation (4) provides a means of calculating the percent change in the number of offenses and the probability of detection – the parameters of greatest interest when testing for deterrence. There are three estimated parameters in equation (4): ΔG, *PENRATE*, and C. The value of ΔG comes directly from Table 1 (or alternatively, if one prefers, one of the estimates from Table 2 could be used). The variable *PENRATE* is simply the number of minor penalties

[14] When the sample is divided into three time periods, there are no discernible trends in the results. Thus, there is little evidence that learning took place over the course of the season. Full results are available from the author.

called per minute, the sample average of which is .173.[15] Finally, the parameter *C* simply captures the expected cost of being called for a penalty and having to play a man short. As noted earlier, the best estimate of that cost is .17 goals per power play opportunity. Plugging these values into equation (4) yields an estimated percentage increase of 6.8 percent (standard error of 11.2 percent) in total offenses committed at even strength when a second referee is added. During power plays, offenses committed are estimated to have declined 13.6 percent (standard error of 26.2 percent).[16] Thus, the two estimates are of opposite signs and neither has a t-statistic greater than one. Taking a weighted average of the minutes in the game played at even strength versus on the power play, the overall change in offenses committed is an estimated 1.7 percent increase (standard error of 10.7%).[17]

While the result above might superficially appear to argue against the deterrence hypothesis, in reality, the true explanation for the lack of response seems to be that there was no discernible change in the probability of detection. Using equation (5), the percent change in the probability of detection is estimated to be the difference between the percent changes in the rate at which penalties are actually called and at the rate at which offenses are committed. The estimated percent increase in minor penalties actually called is 5.5 percent (standard error of 2.9 percent); the estimated percent increase in offenses committed, computed above, is 1.7 percent. Consequently, the estimated percent increase in the probability of punishment is 3.8 percent

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[15] Because the number of minor penalties does not vary much by the number of referees, in practice the estimates of equation (4) are not sensitive to using the average number of penalties in just one-referee games or just two-referee games instead of the overall average.

[16] In the calculation of the standard errors, I assume that the parameters *C* and *PENRATE* are known with certainty.

[17] On average, roughly 75 percent of the game is played at even strength and 25 percent on the power play.

(standard error of 11.1 percent). The null hypothesis of no change in the probability of detection cannot be rejected. Thus, the most reasonable interpretation of the data is not that they provide evidence against the presence of deterrence, but rather, that the addition of the second referee had only a minor impact on the probability of detection, rendering this experiment unable to provide a true test of the hypothesis. In contrast, if one observed large increase in the probability of punishment, but no increase in scoring, then the deterrence model would be rejected.

V. Conclusions

A quasi-experiment involving random assignment of referees conducted by the National Hockey League seemingly provides a unique opportunity to test the economic model of crime. Combining the estimated parameters from the natural experiment with a simple model, it is possible to estimate the underlying behavioral parameters of interest. In practice, however, doubling the number of referees had only a small impact on the probability of detection. Thus, it is not surprising that the apparent impact of adding an referee on offending rates is small and imprecisely estimated. Ultimately, there is little that can be learned from the experiment, despite its superficial attractiveness.

Table I: Comparison of Outcomes in Games with One vs. Two Referees

Variable	Two-Referee Games	One-Referee Games	Difference: Two-referee minus one-referee games
Total minor penalties called	10.90 (.24)	10.33 (.17)	.57 (.30)
Total major penalties called	1.09 (.10)	1.27 (.08)	-.17 (.13)
Total goals scored per even-strength minute	.084 (.003)	.086 (.002)	-.002 (.003)
Total goals scored per power-play minute	.108 (.006)	.103 (.005)	.004 (.008)

Notes: Values in table are sample averages for games played between the dates of October 16th, 1998 and February 28th, 1999. Over that time period, there were a total of 270 games with two referees and 510 games with one referee. Goals per even-strength minute are the combined total for both teams. Because the number of referees is essentially randomly assigned, the value in the far right-hand column of the table represents an estimate of the impact of adding an additional referee. Standard errors, allowing for unequal variances in one-referee and two-referee games, are in parentheses.

Table II: Regression Estimates of the Impact of Adding a Second Referee

Values reported in table are coefficients on an indicator for having two referees in a game

Dependent variable	(1) No Covanates	(2) Linear Time Trend	(3) Linear Time Trend and Team-Fixed Effects	(4) Linear Time Trend and Team Interactions
Total minor penalties called	.57 (.29)	.58 (.29)	.59 (.28)	.69 (.35)
Total major penalties called	-.17 (.14)	-.17 (.13)	-.17 (.13)	-.14 (.16)
Total goals scored per even-strength minute	-.002 (.003)	-.002 (.003)	-.002 (.003)	.003 (.004)
Total goals scored per power-play minute	.004 (.008)	.004 (.008)	.004 (.008)	-.003 (.010)
Degrees of freedom	778	777	750	424

Notes: Values reported in the table are coefficients on an indicator variable equal to one when there are two referees in a game and zero otherwise. Each table entry is a coefficient from a different regression. Column (1) includes no covariates and thus is identical to the last column of Table 1. A linear time trend is added in column (2). In column (3), dummy variables for each of the 27 teams in the league are included as controls. In column (4), interaction terms representing every possible combination of the 27x27different team pairings are added to the regression. Standard errors in parentheses.

Colophon

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