PENALTY AND CRIME: FURTHER THEORETICAL CONSIDERATIONS AND EMPIRICAL EVIDENCE

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ABSTRACT

Two different models of the game between police and public, one with two-sided incomplete information and the other with multiple strategies, lead to the same results as my previous research: changes in penalties at equilibrium do not affect crime but instead affect police behavior. I compare these models with alternative models of the police–public game which come to different conclusions. I provide reasons for the differences in the conclusions, as well as empirical evidence against which the different models can be evaluated.

KEY WORDS • crime • game theory • penalty

Punishment does not prevent crime. (Walter Reckless, 1967)

Punishment will indeed deter crime. (Gordon Tullock, 1974)

The papers by Weissing and Ostrom [(1991b) from here on W–O] and Hirshleifer and Rasmusen [(1992) from here on H–R] both accept the starting point of my argument: that crime (as well as other similar situations) should not be studied as simple decisions made by criminals, as previous research has done, but instead as interaction between criminals and the police. However, they disagree with one of my conclusions that changes in penalty have no effect on crime. They attribute my conclusions to particular assumptions in the ‘Robinson Crusoe’ paper (Tsebelis, 1989) and present models with different assumptions that come to the conclusion that increases in penalties have a deterrent effect on criminal behavior. Finally, in their discussions, they both criticize the realism of my assumptions.

W–O (1991b: 347) make the criticism that my results hold only when one assumes unified actors, or the interaction of ‘symmetric players’. H–R present two different criticisms: they argue that if the players move sequentially or if they have a continuum of strategies, my results do not hold. Both papers ask the obvious next question: how reasonable are these ‘unified player’, ‘symmetric player’, simultaneous play or two-strategies per-player

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assumptions? They come to similar conclusions: for W-O my result is local, it 'holds for some cases and does not hold in others'. (1991b: 349); for H-R it 'is only rarely applicable in actual policy-making situations'.

Let me state my position on the matter, and then outline the presentation of my argument. I do not disagree with either of the two critics' models, Moreover, I do not disagree with the point that the assumptions in the 'Robinson Crusoe' article are simplified and that reality is much more complicated. In fact, I have called this model 'simplified' and 'bare bones' myself (Tsebelis, 1990b: 261).

However, even in the first paper (Tsebelis, 1989) where the bare bones model is presented I claim that more realistic assumptions lead to the same results. Accordingly modified, these models are presented in the text and appendices of later papers (Tsebelis, 1990b and 1991 in particular), and another is set forth in my debate with Bianco and Ordeshoo (Bianco et al., 1990). Such models include more realistic assumptions, including different kinds of police and public1 (relaxation of the unified player assumption), as well as relaxation of the rationality assumption in favor of an evolutionary account, which is the most realistic.2 I return to the evolutionary model later. All these alternative and more realistic models lead to the same conclusion: changes in penalties have no effect on crime. That is why I have said that this result is robust.3 I continue to maintain this thesis, and I intend to demonstrate it further in the present paper. I have also said that 'robustness' does not mean that other models may not lead to different results.4 Obviously, I continue to maintain this thesis too.

How can my conclusions be robust and the models of my critics correct? The line of defense I take is that the assumptions of the more sophisticated models I have presented cover at least as reasonably as the models offered by my critics a wide variety of cases of crime.

2. 'Of all these modeling variations, I consider the one in which the players are assumed to possess parametric rationality to be the most realistic because it does not assume that the two players – the police and the public in my example – are unified or that they have capabilities of coordination and strategic calculation. Each member of the police and the public maximizes his or her own payoffs given what the others do, and the equilibrium emerges as the outcome of all these independent decisions' (Bianco et al., 1990: 576). And in Tsebelis (1990b: 267–8) I develop this argument further.
3. 'Given that game-theoretic models are usually highly sensitive to institutional or informational modifications, this stability of the equilibrium solution is remarkable indeed' (Bianco et al., 1990: 576).
4. 'However, it would be a mistake to conclude that all game-theoretic models of crime would necessarily lead to the same result as that presented in the Robinson Crusoe article. Additional modifications and complications of the model could alter its conclusions, just as my argument modifies the conclusions of the economic approach to crime' (Bianco et al., 1990: 584).
This paper is organized into three sections. In the first, I present a new model with multiple types of police and criminals. This model leads to the same conclusions as the Robinson Crusoe article, namely that increases of penalty have no impact on crime, but reduce police monitoring. Since this model relaxes the 'unified' player assumption, it will permit comparisons with W–O. In the second section I address H–R objections. First I discuss the question of sequence of moves and then their claim that for my argument to hold one needs changes in payoffs 'sufficiently small'; I present a model in which even large changes in the payoffs (like doubling penalties) do not affect my conclusions. Finally, since the empirical (what some call 'substantive') part of the debate is at least as important as the presentation of models, I offer some empirical evidence from the literature in favor of my model in the third section. In the conclusions I summarize the current state of the debate and point out what I believe are the important points of agreement following a series of articles and an even longer series of debates.

Response to Weissing and Ostrom

W–O make the claim that consideration of multiple police agents alters one of the results of my model. Consider two different types of police agents facing the same type of public. Each type of player follows the assumptions of my Robinson Crusoe article (I refer to them as restrictions of payoffs): both types of police agents prefer to monitor when there is crime and not to monitor when there is no crime; the public prefers to violate the law when there is no monitoring and not to violate when there is monitoring. Without loss of generality, I make the two types of police player differ in only one payoff: their respective payoff when crime occurs.5 The restrictions on payoffs are formally presented along with the game tree in Figure 1.

Figure 1 presents the following situation: police agents are of the first type with probability $\gamma$ and of the second with $(1 - \gamma)$. The different types of police agents meet randomly with the public [the probability of interaction of the public with the first type is $\gamma$ and the second is $(1 - \gamma)$]. The game tree of this game is a special case of the game tree I presented in the Bianco et al. controversy (1990: Figure 2); however, the payoffs are different. In the Bianco et al. controversy I considered two types of police and two types of public and assumed that some police agents had a dominant strategy to monitor all the time (regardless of the presence of crime); moreover, some

5. The conclusions of the model hold even if different types of players differed with respect to the payoffs (provide the payoffs do not violate the restrictions on the payoffs stated above).
members of the public had a dominant strategy not to violate the law under any circumstances. The result of that model was that in equilibrium (under a wide range of frequencies of types of players with dominant strategies) the frequency of crime and the frequency of law enforcement were exactly the same as if the honest members of the public and the good police agents did not exist. The strategic agents 'corrected' for the presence of such types. The strategic members of the public increased their frequency of violating the law and the strategic police agents reduced their frequency of monitoring, so that the final outcome was exactly the same as if everybody was strategic. These equilibrium results present the property that a change in the severity of punishment does not alter the frequency of crime; it reduces the frequency of law enforcement instead.

In the Figure 1 model there is only one type of public, and no type of police has a dominant strategy. All types of players are strategic, i.e. their behavior depends on the behavior of the other players. I demonstrate that this model leads to the same conclusions as the other six models I have presented so far (Tsebelis, 1990b), i.e. an increase in penalty has no effect on the aggregate rate of crime but instead affects the frequency of police monitoring.

In this model (like in the W–O model) the public is confronted with more than one police agent. W–O has a variable number of police agents, but this difference is minor, as I show below. In Appendix A I calculate the equili-
bria of this model. There are four different candidates for equilibria. In each of them the public mixes its strategies, while one of the police types mixes and the other plays a pure strategy. The frequencies of these mixed strategies are calculated in the appendix. Here I present the results verbally.

1. In all these equilibria the behavior of each player is affected by the payoffs of the other, so that an increase in the severity of punishment reduces the frequency of law enforcement. This result is not surprising; it happens in all two person games without pure strategy equilibria, as I noted in my response to Orsheshook (Bianco et al., 1990: 578–79).

2. The frequency of crime varies in the following way: the public takes into account the payoffs of the type of police that mixes strategies, so if the first type mixes, the payoff \( b_2 \) enters in the frequency of crime function; if type 2 mixes, the payoff \( b_2' \) is used to calculate the frequency of crime.

3. While the frequency of mixing the different strategies of different types of police varies, all these mixtures produce a frequency of law enforcement which is exactly the same as the one calculated in the Robinson Crusoe article. Consequently, all the qualitative results (including the crime and punishment one) remain the same. The same results can be replicated for a finite number of types of police agents. Moreover, the model can be expanded to include two-sided incomplete information, \( n \) types of police agents and \( m \) number of types of the public. In this general case, there will be multiple mixed strategy equilibria, each one having the property that changes in the payoffs of one player do not affect his equilibrium behavior.

Let us now compare the above model with the game presented by W–O. Their model introduces another police agent as an independent player and studies the properties of the equilibrium of a three-player game. It is well known that in games with more than two players, in general, the payoffs of each player affect the behavior of all players. In their model (unlike mine), an increase in penalty decreases crime; moreover, this increase decreases police monitoring (as in mine).

W–O use their model in a very creative way to identify the special conditions under which my conclusions hold. They find that if ‘two players interact and affect one another, but each is affected by different sets of players’, penalty will have no effect on crime.

In my response to Orsheshook (Bianco et al., 1990), I tried to identify the conditions under which third players would not affect the results of the

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6. In their article, Weissing and Ostrom (1991a) have extended the investigation to \( n \) independent police agents.
monitoring game. I found two such conditions, each of which I shall now detail.

First, my results hold if third players (such as the courts or the legislature) move before or after the police and the public. The argument of W–O about third players affecting only one of the two players of the monitoring game seems to me to generate a new area of applicability of my model. Even when the third player moves simultaneously with the other two, under the conditions specified in W–O’s article (1991b: 346) my results still hold. Following their suggestions, I modeled a three player game with one criminal and two police agents which generates results identical to those in my bare bones model. Each one of the police agents has the same payoffs as those in the initial article, and the criminal receives the same payoffs (when) regardless of whether one or both police agents monitors (payoffs which obviously depend on his action). In this model the equilibrium frequency of crime is exactly the same as in the Robinson Crusoe article, and the probability that either one or both police agents (i.e. the police as an organization) will monitor is also identical to my initial calculations.

A second application of my results is not covered by W–O’s argument. They argue that my results hold as long as ‘players in position A are not affected by other players in position A’ (1991b: 347). Here is their argument:

For example, if we take the Tsebelis game between the police and the public, then each policeman would be affected only by the behavior of players in the position of citizen, and not by other policemen. Each citizen is affected by the behavior of players who are policemen, and not by the behavior of other citizens.

However, in models with multiple types of players each type of player is affected by the behavior of all others. In the model I presented in the Bianco et al. controversy, the strategic part of the public modifies its strategy to compensate for the behavior of the non-strategic parts, so that overall the frequency of crime is the same as if all players were strategic. Further, the model I present in this paper demonstrates that when all players are strategic, then the multiple equilibria of the monitoring game may be exactly the same as in the bare bones model, and even if they are not, they all share the property that modification of the payoffs of one player affect the behavior of the opponent and, consequently, that penalty has no effect on crime.

Why this difference of results? Because in the W–O model third players are completely independent of the first two, while I model them as types of one of the two other players. Which is the better modeling strategy? Here I share their conclusion: it depends. I think that their model is perfectly appropriate for the case they study. They examine an irrigation game, where each one of the turnwaiters has an incentive to independently monitor the other players, and therefore the probability of non-detection of the cheat-
ing member of the community will be the product of the probabilities of non-detection by each one of the independent turnwaiters. As I understand it, they deal with a small community where each member can independently monitor the whole field, thereby detecting violations of agreements. Their model would not be applicable if, for example, the turnwaiters hired a police agent to monitor instead of them, or if they divided monitoring activities by time of day. In this case, a different turnwaiter would interact with one turn-taker at different times and my results would hold. 7

However, I question the applicability of a model with multiple, independent strategic agents monitoring each other for the police–public interaction. In the case of the police, each police officer has his or her own beat. Moreover, even when multiple police officers or agencies have the same jurisdiction, they coordinate their activities so that there is no or minimum overlapping. 8 Finally, some criminals select a particular area they know well to commit crime. 9 It seems reasonable to assume that each criminal knows the average rate of monitoring and each police agent knows the average rate of crime in the area, but each of them does not know the identity (i.e. the exact payoffs) of the other. If this is a plausible story, then the model I presented here is appropriate.

Response to Hirshleifer and Rasmusen

H–R raise two issues. The first is the sequence of moves; they argue that the police–public game is better modeled as sequential. The second is mixed strategies; they present models with continuous strategy spaces, or models where changes in the payoffs produce pure strategy equilibria. Let me address their criticisms one at a time.

7. In their article Weissing and Ostrom (1991a: 4.1) make a similar point: ‘In most systems, however, the players would include only those farmers who are located in the same field channel, who know who the turntaker is, who feel a direct loss when water is stolen, and who can positively or negatively sanction each other’s behavior. In many cases, it will only be the farmer whose turn will come in the next time period, who fulfills these requirements. In the later case, only two farmers would be included among the players even if the irrigation system itself were quite large’.

8. See Ostrom et al. (1978: 2): ‘There is little duplication of service. In most areas where two or more police departments conduct the same service they divide the work so that each complements the other’. The study covers 80 metropolitan areas and concludes that: ‘In only 16 of the 80 areas do more than 10 percent of the inhabitants receive duplicate patrol service. There are, however, two metropolitan areas where more than half the population receives patrol services regularly from two or more producers without alternation’ (Ostrom et al., 1978: 100).

Sequence

Simultaneity of moves is the formal way of expressing the idea that each player moves without knowing what his opponent is doing. Sequence of moves means that the second player knows the move of the first. Consider the police–public game unfolding in time; each one of the players knows what the other has done in the past, while ignoring the current move. What is the best way to model such a game in one shot? As a game of simultaneous moves (like I did) or as a sequential game (like H–R)? And if it is sequential, why not have the public move first instead of the police?

Let us first pursue a very compelling reason why the monitoring game may be sequential: it may be that the police find it advantageous to announce their strategy, in order to deter criminals. Along these lines H–R make the following remark: ‘The authorities may well attempt to publicize their actions. One of us recently heard a radio ad of the Chicago Transit Authority advising criminals (truthfully?) that enforcement had been increased.’ However the authorities have an incentive to issue such statements all the time because, if believed, they will save money, effort and time. So, such announcements have to be considered and analyzed as part of the strategy of the police. Elsewhere, I have analyzed such a game of incomplete information in which after the initial statement, the two actors play a simultaneous move game. Because of space constraints here I will report only the final conclusion: aggregate frequencies of crime and law enforcement are the same as in the simultaneous move game. Therefore, H–R are right to wonder whether the police announcement is ‘truthful’.

There are, however, two more arguments in favor of a sequential game where the police move first. Cox (forthcoming) models the game as a sequence of moves with the police moving first, because the police can be considered a unified player capable of strategic rationality and able to precommit in the game. In other words, the police will use the strategy calculated in the model, even if the rate of crime goes down to the desired level.

H–R (1992: 356) use a different argument in favor of sequential moves. The police move first because, as a strategic actor, they are able to antici-

10. For a discussion of modeling the game as iterated instead of one shot, see Bianco et al. (1990).

11. See Chapter 3, Appendix 3.B in Tsebelis (forthcoming). There is an infinity of equilibria that can be divided into two groups: (1) the initial statement of the police is completely ignored, and both actors play as if there was no statement; (2) the initial statement is believed (if it is issued with low frequency) but it does not affect the strategy of the public but of one type of police.

12. However, it could be sustained (along with infinitely many others) in an iterated game.
pate the reaction of criminals, while the criminals simply adjust to police behavior, like 'price-takers' in microeconomic theory.

As H–R point out, I have been sympathetic to both arguments and have considered this sequential move approach 'more realistic'. However, since the original 'Robinson Crusoe' article appeared, I have modified my position on the question of sequence. Now I consider sequential moves to be just one possible way to model the police–public game. Here is the reason why: it presupposes that the police would actually continue monitoring if crime were to fall to the level predicted by the sequential game. How reasonable is this assumption? Would some police agents not be tempted to shirk when crime goes down? And if individual criminals are price takers and do not think that their behavior affects the behavior of the police, why would individual police agents not think the same way? In other words, sequential movement is the technical way to express (faith) confidence in the internal enforcement mechanism of the police; and simultaneity is the way to focus upon the decision of individual police agents.

If the individuals that compose the aggregate actors – the 'police' and the 'public' – think as 'price takers', then crime goes down as the police monitor and police activity is reduced as crime goes down. This is the logic of the evolutionary model which produces cycles of crime as well as cycles of law enforcement. I will return to this model in the next section.

Mixed strategies

The continuous strategy space used by H–R produces a pure strategy equilibrium.13 They compare their continuous strategy model with one where players have discrete strategies, and come to the conclusion that even when a game does not have pure strategy equilibria my results hold only for 'sufficiently small' changes in the payoffs (in the sense of not shifting the strategy elements entering into the equilibrium). I do not disagree with their argument. However, from there, they jump to the conclusion that my results are rarely applicable in actual policy-making situations. It is with this conclusion that I take issue. I present two different points: first, that actual crime situations may involve exactly what they describe – lumpy choices and small changes in payoffs; second, I present a game theoretic model where doubling or tripling the size of penalty has no effect on crime.

1(a). Small changes in penalties. Wilson and Herrnstein (1985: 142, 388) argue repeatedly that for political reasons 'small changes are the only feasible

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13. As I have argued even in this case, my results hold if the payoffs of each player are linear functions of his strategies (Tsebelis, 1990b: 265).
changes' in criminal penalties. Similarly, Andenaes attributes the lack of empirical findings in favor of the deterrent effect of penalties to the small level of variation of penalties in the United States.14

1(b). Lumpy choices. One can imagine a series of crimes (distribution of illegal substances for example) where the distribution of the benefits is such that existing penalties fall between the two groups of public, violators and non-violators, and consequently the police face lumpy choices (small changes in police behavior have no effect). Wilson (1983: 403) offers an argument along these lines in his explanation of the failure of 'The Nation's Toughest Drug Law':

Not all forms of criminal behavior change with changes in sanctions, however. If the crime is sufficiently rewarding or its detection sufficiently unlikely, then little observable change in behavior will result from any feasible change in sanctions, a conclusion we inferred from studies of the effect of changing the penalties for large-scale heroin trafficking.

2. Big changes in penalties with no effect on crime. In Table 1 I present a police–public game where each actor has three strategies. The ranking of the payoffs and the calculation of equilibria are presented in Appendix B. Here I give the supporting story and the conclusions.

Suppose an inspector of OSHA selects whether to inspect a facility for violations of safety regulations ($M_1$), of health regulations ($M_2$) or not at all. Similarly, a firm can decide to violate regulations of safety ($V_1$), of health ($V_2$) or abide by the law.15 Assuming that the inspector has a small probability of detecting health (safety) violations even when he is monitoring safety (health), gives the order of payoffs presented in Appendix B. Under the assumptions presented there, this simple game has no pure strategy equilibrium.

Consider now that one doubles or triples the penalties for violations. This uniform increase in severity of penalties does not alter the ranking of the payoffs of the actors, and consequently does not affect the mixed strategy of the firm. Again, it leads the inspector to relax a bit in his law enforcement effort. Of course, technically speaking, the doubling or tripling

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14. 'It would be a serious misinterpretation of the findings to say that they indicate that certainty of punishment has a deterrent effect, whereas it seems doubtful that severity has such an effect. What the research does suggest is, first that the use of imprisonment acts as a deterrent for traditional crimes and, secondly, that differences in the length of imprisonment, at the levels of use in the United States, do not seem to have much impact on crime' (Andenaes, 1975: 347).

15. The whole point here is to give to each actor multiple strategies, so I will not examine the case where the inspector (firm) would prefer to monitor for (violate) both regulations. This case leads back to the two-by-two game of the Robinson Crusoe article.
### Table 1. Police–Public Game with Three Strategies Available to Each Player

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<td>$d_1, d_2$</td>
<td>$e_1, e_2$</td>
<td>$f_1, f_2$</td>
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<tr>
<td>$NV$</td>
<td>$g_1, g_2$</td>
<td>$h_1, h_2$</td>
<td>$i_1, i_2$</td>
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**Assumption:** $g_1 > d_1 > a_1$, $h_1 > b_1 > e_1$, $c_1 > f_1 > i_1$, $a_2 > b_2 > c_2$, $e_2 > d_2 > f_2$, $i_2 > h_2 > g_2$.

(I could have multiplied penalties by ten in this example) of penalties was in H–R’s terminology a ‘sufficiently small’ change.

The above points indicate that my model can produce the same results with indeed very large ‘sufficiently small’ changes. But such difficult tests may not be necessary in real cases, because the structure of the policy relevant situation may be such that only small changes are permitted, and choices are lumpy.

To summarize my argument: technically, what is required for my results is a two person game without pure strategy equilibria. One can replace the unified players by a finite number of types for each player, as several of my models, including the ones in this response, indicate; the results remain unchanged. One can introduce other players who move before or after the police and the public (Bianco et al., 1990: 579–81); again, the results are identical. One can use continuous strategies instead of discrete ones, provided the payoffs are linear functions of the strategies of the corresponding player (Tsebelis, 1990b: footnote 15); in such cases the first order condition of the Nash equilibrium will invariably reproduce my results: the payoffs of one player will affect the behavior of the other.

My critics produce models which violate some of these assumptions. They introduce non-linear utility functions (Hirshleifer and Rasmusen, 1992); they produce models where players have dominant strategies (Rapoport, 1990; Mayer, 1991); they consider an infinite number of players without mixed strategies (Cox, forthcoming); or they introduce third players who move simultaneously (Weissing and Ostrom, 1991b). It is not surprising then that some of their conclusions differ (see footnote 4). In all of my critics’ models, an increase in penalty reduces monitoring (as in my model), but unlike my model, it reduces crime too. What is surprising is that even in some of their models, under plausible specifications, all of my conclusions hold.

I have argued (Tsebelis, 1990c: 514) that my assumptions are domain assumptions, i.e. that my results hold whenever my assumptions hold.

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16. With the exception of H–R Figure 1, which, however, is presented as a possibility without a supporting story.
Nobody has disputed that part of my argument. I have also said that my assumptions are a legitimate simplification of real-life situations involving monitoring games, such as crime, regulation, etc. This part of my argument has been challenged.

How do we judge the reliability and validity of assumptions? There are two ways. The first is theoretical: to examine how plausible the assumptions are and how necessary they are for generating the results. My critics claimed that the necessary conditions for my 'penalty has no impact on crime' conclusion were the (implausible) assumptions of unified players 'all or nothing response' from the players. I have demonstrated in several places, including above, that this is not the case and I have presented different reasons why they come to different conclusions.

The second way to judge the validity of assumptions is indirect and empirical: by the results they produce. When H-R claim that my conclusions are 'rarely applicable' they refer to this indirect and empirical way of testing models. Let me now turn to this final point: that my model is defensible not only on theoretical, but also on empirical grounds.

Some Empirical Studies of Penalty and Crime

In my model there is a trade-off between the expected penalty and the frequency of police monitoring: when the expected penalty increases, monitoring decreases, so that the frequency of crime remains the same. In this sense, certainty and severity of penalty are inversely related.

Several empirical studies have argued that certainty and severity of penalty are inversely related. Bailey and Smith (1972) found a consistent inverse relation across offenses not only between certainty and severity but also between changes in the levels of these variables. However, in earlier literature this relationship was attributed to the effects of lawyers, prosecutors and judges, i.e. actors who interfere after the arrest of the offender. As the argument goes,

It is possible that severity is the enemy of certainty and speed. As penalties get tougher, defendants and their lawyers have greater incentives to slow down the process, and those prosecutors and judges who oppose heavy sentences for drug dealing may use their discretionary power to decline indictment, accept plea bargains, grant continuances, and modify penalties in ways that reduce the certainty and the celerity of punishment. (Wilson, 1983: 135)

I call this trade-off between certainty and severity of penalty 'expected utility preserving' because the changes in sanctions are nominal; they do not affect

the *actual* number of years a criminal spends in jail or the amount of fines, etc. once one is caught.

In my model the size of penalty once one is caught reflects both certainty and severity in Wilson's terms. Once a criminal is caught, changes in penalties are not 'expected utility preserving'. My argument is that even if the expected number of years in jail increases, crime remains the same. In this sense, I point out a different trade-off: increases in the expected value of the penalty, whether through changes in the law or the behavior of the prosecutors, judges, etc. will adversely affect the actor who moves simultaneously with the criminals, i.e. the police. The police reduce monitoring activities. This is a different idea, which, as far as I know, has not been noted in earlier literature. Moreover, this feature is also found in the game theoretic models of my critics: increases in penalty reduce police monitoring. I differ with my critics in that in my model this reduction is offset by changes in penalties, while in their models it is not.

In the remainder of this section I examine the findings of the empirical literature and focus on whether an increase in the severity of penalty reduces crime. A negative answer to this question can be understood as supporting either my argument or Wilson's (nominal but not actual change because of adjustments in the judicial system). If, however, sentences are measured in terms of *actual* number of years in jail, *actual* fines, etc. then the empirical evidence supports my thesis and not the 'expected utility preserving' one.

There is an extended econometric literature on crime. Most models analyze data sets from the US. It must be noted that while the number of studies is large, the number of independent data sets is small. Some of these studies report results congruent with my expectations.

Frost (1976), using data from 1970, replicates a similar study by Ehrlich (1973), who had used 1960 data. Frost finds no impact of penalty on crime, but his results are sharply divergent from Ehrlich's, as well as from other studies: 'Ehrlich estimated the elasticity of aggregate crime rate with respect to the average time served in prison to be -1.12, and I estimate it at .01' (p. 479). Brier and Fienberg (1980: 171) reanalyze Frost's data, use different indicators and come to the same conclusions as Frost. Antunes and Hunt (1973), analyzing 1960 data, come to the conclusion that certainty but not severity matters, just as my models predict.

Avio and Clark (1976: 50) analyze Canadian data and come to conclusions confirming my predictions.

The single pervasive result that is *not* consistent with the predictions of the [deterrence] model is that whereas an increased risk of capture and conviction leads to a reduction in recorded property crime rates, an increase in the severity of sentence (as measured by sentence length) does not appear to reduce the recorded crime rate. The policy implications of this result are profound.
Other aggregate studies of tax evasion (Witte and Woodbury, 1985: 7), abortion (Zimring, 1972) and draft evasion (Blumstein and Nagin, 1977) come to similar conclusions: while certainty of punishment has a negative effect on crime, severity does not. And as far as the death penalty is concerned, there is no supporting evidence of its effectiveness (Wilson, 1983; Tullock, 1974).

However, most of the econometric papers come to the conclusion that 'deterrence works', and find that police activities and certainty of punishment, as well as severity, have negative effects on crime.¹⁸ Nonetheless, their methodologies as well as the quality of their data are so objectionable that literature reviews have come to the conclusion that there is no reliable evidence on the deterrent effect of punishment. Some researchers have argued that the negative correlation between crime and punishment demonstrates the negative impact of crime on punishment, and not vice versa, as deterrence theories predict (Nagin, 1978b; Pontell, 1978). Nagin says that

...the results reported in the prior section provide no reliable evidence that risk of imprisonment or time served has a measurable deterrent effect on the index crime rate. Rather the analysis makes a strong case for the argument that the negative association between the index crime rate and imprisonment risk, which has been so thoroughly documented in the literature, is attributable to the negative effect of crime rate on imprisonment risk.

However, he adds that his results should not be interpreted as indicating absence of deterrent effects, but could be due to small variations of the variables or to measurement errors.

After reviewing the existing econometric literature (and repeatedly rerunning models), Brier and Fienberg (1980: 151) came to the conclusion: 'There is no empirical evidence to warrant an affirmative conclusion regarding the deterrent effect of punishment'. More recently, Cameron (1988: 323), after examining 127 entries, summarizes his argument as follows:

The empirical work is reviewed with the conclusion that studies using aggregate data fail to convincingly demonstrate deterrent effects. Studies of individual prisoners and victims, on the other side, suggest that deterrent effects on the supply of crime are null or positive.

Other authors (mainly sociologists) have followed a different route. They analyze survey data, examining whether higher penalties affect the self-report rates of offenders. This literature concludes that while certainty of penalty has a deterrent effect, severity of penalty has no effect on crime. Here is how Waldo and Chiricos (1972: 536) summarize their findings:

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¹⁸ They are enumerated in Nagin (1978b), as well as in Brier and Fienberg (1980). For a more recent review see Cameron (1988).
The data for marijuana use and theft indicate that no relationship exists between perceptions of severe punishment and admitted criminality. This finding runs counter to deterrence theory, but is in accord with several earlier studies of deterrence (Schuessler, 1952; Sellin, 1967; Gibbs, 1968; Tittle, 1969; Chiricos and Waldo, 1970). While these and earlier findings cannot be held conclusive, they strongly question the assertion that crime may be deterred by increasing penalties.

Several other studies should be added to the list of those finding evidence for the deterrent effect of certainty but not the severity of punishment: Salem and Bowers (1970); Teeven (1972); Chauncey (1975); Silberman (1976); Piliavin et al. (1986).

Finally, Paternoster (1989) distinguishes between the decision to commit a first offense and the decision to continue (where presumably a reassessment of the threat of sanctions takes place). The reason for this distinction is that perceptions of severity and risk may be the consequence rather than the cause of delinquency. He uses panel data from nine public high schools and examines four common offenses: marijuana use, drinking liquor, petty theft and vandalism. He concludes (1989: 37), 'The empirical tests indicated that the perceived severity of punishment had virtually no effect on the decision to offend or to quit offending'.

However, the findings of this whole body of literature can be questioned on the grounds that (self) reports of crime may not be reliable indicators for crime rates.

If there are objections concerning the conclusions of econometric studies and of survey research, where are we left in terms of our empirical knowledge on crime and punishment? There are a series of quasi-experiments that have been performed and analyzed over the years. They are closely controlled studies of particular changes in laws or behavior of specific actors (police, judges, etc.) in particular jurisdictions. What do they indicate?

Several studies of the behavior of the police indicate that an increase in monitoring activity has a deterrent effect. With respect to changes in the severity of penalties, the most famous study evaluated the New York Drug Law (1975). The title of the report issued in March 1978 (Joint Committee of New York Drug Law Evaluation, 1978) was: 'The Nation's Toughest Drug Law: Evaluating the New York Experience'. This tough law 'may have temporarily deterred heroin use' (1978: 9), but it did not affect either the price of heroin or the frequency of drug related crime. The final words of the report are: 'It is implausible that social problems as basic as these

19. In fact in half of the cases, the corresponding coefficients were positive.

20. For reviews of this literature see the corresponding chapters of the evaluation from the National Academy of Sciences (Blumstein et al., 1978) and Wilson (1983). Both of them evaluate the effects of sentences as well as police behavior. The first review comes to the conclusion that middle range theory and further studies are necessary. Wilson (1983: 137) reports some additional evidence and comes to the conclusion that 'it is difficult, but not impossible, to achieve increased deterrent effects through changes in the law'.
can be effectively solved by the criminal law’ (1978: 30).

Two studies on gun control laws come to contradictory conclusions. The first (Beha, 1977) examines the Bartley–Fox gun law in Massachusetts; the second (Loftin et al., 1983) examines a similar law in Michigan. The first law imposed a mandatory sentence of one year for carrying a firearm without a permit. It deterred casual carriers and decreased the rate of gun related crimes (Wilson, 1983: 136). The second imposed a two-year add-on sentence for defendants convicted of possession of a firearm in the commission of a felony. It had no effect. Wilson (1983: 136) explains the different outcomes by arguing that the Michigan Law was not enforced by the judges (expected utility preserving thesis). This is not the conclusion of Loftin et al. (1983: 312), however. ‘This is a tough law. There were no obvious loopholes in its formulation, and the Prosecutor’s policy of enforcing it was not just a “grandstand” act. The policy was enforced and cases were vigorously prosecuted.’

Can my model help explain the difference in outcomes produced by these two laws? The answer is ‘yes’. One of the assumptions of my model is that the players have no pure strategy equilibrium, i.e. the public prefers not to violate the law in the presence of the police. If there is no deterrent effect at the beginning of the process, everybody violates the law, and an increase in penalties will make the members of the public mix (thus decreasing crime rates). This is what appears to have happened in the above case. In Massachusetts the increase of penalty transformed the monitoring game from one of a pure strategy equilibrium to one with mixed strategies.21 In the Michigan case however, the law became more severe when the payoffs already were such that mixed strategies were prevailing, and so it had no effect.

Finally, there is a series of quasi-experiments that are especially illuminating from a cross-national point of view: studies of drunk driving in various countries. In 1936 and 1941 the Norwegian and the Swedish Parliaments, respectively, introduced new drinking-and-driving laws which were characterized by increased certainty and severity of punishment. The results were considered very encouraging and eventually various other countries followed their example. Scandinavian-type changes occurred in the UK (1967), France (1978), the Netherlands (1974), Canada (1967), New Zealand

21. A more complete account can be given if one divides the public into three groups: one that has a dominant strategy not to bear arms, one that has a dominant strategy to possess arms, and one that has preferences leading to mixed strategies. If the number of people in the second group is sufficiently high compared to the third, there will be no effect. I would expect a similar phenomenon to occur when one criminalizes and introduces mandatory prison sentences for white collar crime.

Ross (1982) reviews the available evidence for all these cases. He distinguishes between certainty and severity of punishment. His conclusions with respect to severity are unequivocal (1982: 96): 'The experiences reported here do not support the deterrence model in the matter of severity of penalty'.

However, Ross, unlike the authors of econometric studies and self report studies of criminals, refers to the nominal severity of punishment. So, his findings can be understood to support either my thesis or the 'expected utility preserving' thesis. Further examination of the behavior of judges, prosecutors and the criminal system in general (which Ross classifies as 'certainty of punishment') is required. If 'certainty of punishment' turns out to affect the frequency of crime, my argument that (expected) penalty has no effect on crime will be severely weakened.

In this regard, after reviewing all the evidence, Ross observes that immediately after the adoption of Scandinavian-type laws, the frequency of accidents, particularly fatal accidents, diminishes, but later resurges to its previous level. 'Simple deterrence, the modification of legal punishment, has not endured in the experiences reviewed' (Ross, 1982: 70). And later, Ross gives a more comprehensive statement of the effect of Scandinavian-type laws:

In sum, these studies support the proposition that the perception of increased certainty of punishment on the part of potential violators of a law can deter the threatened behavior. This result is good news for proponents of the deterrence proposition. Bad news can be seen in Figure 1 in the slope of the curve following its initial drop. Its tendency to return to the status quo ante has been noted in all of the studies where a deterrent effect was found. In brief, the deterrent effects produced by the adoption of Scandinavian-type-drunk-driving laws have all been temporary, with their effects lasting a few months to a couple of years. (Ross, 1984: 29, emphasis added).

A brief account of the method and findings of a drunk-driving law study will be helpful for understanding which of the rival models of police and public best captures reality. In 1967 the British Parliament adopted the British Road Safety Act, which permitted the police to perform breath tests in order to see whether alcohol consumption was below a specified limit. The increase in penalties specified by the 1967 law and meted out by the courts was marginal. However, the law clearly changed the expected utility of punishment, since it increased the probability of a conviction once caught. Ross (1973) uses interrupted time series analysis to control for other factors

22. He examines cases where a statutory change in penalty occurred. His cases are Finland, Chicago and Traffictown Australia.
such as seasonal variations, length of the month, day of the week (there is a higher probability of drunk driving on the weekend), etc. The percentage of fatally injured drivers with alcohol in their blood fell from 32 percent in 1967 (before the adoption of the law) to 20 percent in 1963, but then increased steadily after that. By 1973 it was 33 percent, and by 1978, 38 percent (Ross, 1982: 34). A similar picture is presented by the absolute number of fatalities on weekends: the dramatic drop of 1967 disappeared by the end of 1970 when the time series ended (Ross, 1973: 33). Ross also reports the number of breath tests administered. It started at around 3000 per month after the enactment of the law, and increased more or less steadily to 7000 or 8000 in the last months of 1970. The number of tests given was far fewer than had been envisaged by the government and most of the test kits originally ordered expired before they were ever used. Here is Ross’s conclusion:

From one perspective the Road Safety Act was betrayed by the police. The Act seemed to offer an opportunity to increase the number of arrests of drinking drivers, as well as the opportunity to dispose of the arrested more efficiently. But while the latter opportunity was seized, the former appears not to have been. (1973: 49) Also important in the British experience was the decision of the police to use restraint in patrol under the Act. It is difficult to locate the level at which this decision was made. (1973: 76)

My evolutionary model (Tsebelis, 1990b: 266–68) can account for the phenomena described by Ross. In this model, neither the police nor the public are unified or endowed with strategic rationality. This model produces slow adaptations by each aggregate player to the behavior of the other, and is more realistic than the other models since it provides a plausible story about how real-life actors could produce cyclical behavior that oscillates around the same equilibrium, as that in my bare bones model. In this evolutionary model, when crime pays, or when monitoring pays more, members of the public, or of the police, adopt that corresponding strategy. The model produces cycles of crime and law enforcement since neither the police nor the public adopt their equilibrium strategies but oscillate around their corresponding equilibrium values.

This evolutionary model, which I have always considered the most realistic,23 not only explains why crime went back up in Ross’s time series, but also helps us understand Ross’s puzzle: at what level was the decision of the police not to enforce the law made? It did not need to be a conscious decision. Once crime is reduced, patrolling does not pay, according to the evolutionary model. Finally, there is one even more puzzling event that Ross did not identify: examining the evidence from 1967 to 1970 one observes that there is a positive correlation between crime and law enforcement. The police

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23. See footnote 2.
increase monitoring activities at the same time that drunk-driving is going up. This positive relationship between crime and monitoring runs counter to any deterrence model, but can be explained as a part of the cycles generated by an evolutionary model. In such a model one expects periods of positive as well as negative correlations.

There is even stronger evidence in support of an evolutionary argument. In a noteworthy article Kohfeld and Sprague (1990) try to merge sociological and economic approaches by creating a model of interaction between criminals and police and estimating it for areas with different demographic characteristics. Their model assumes that the police will have a positive and immediate response to crime (indicated by the number of arrests), while criminals will have a negative and delayed response to police action. The reader will recognize the similarities between Kohfeld and Sprague's formulation and the aggregate behavior produced by my evolutionary model. In fact, the only differences between Kohfeld and Sprague's model and mine are that they use difference instead of differential equations, and they use arrests as a proxy for police monitoring while in my model arrests are the interaction between police behavior and crime.

Kohfeld and Sprague estimate the same model for neighborhoods with different demographic characteristics and find systematic differences in the interaction of criminals and police. For example, in racially mixed areas the interaction between criminals and police is extremely important: '10 arrests lead to almost 8 fewer burglaries' (1990: 129). Reading from their estimated coefficients, 10 more (fewer) burglaries lead to 2.5 more (fewer) arrests (1990: Table 6).24 The aggregate findings are less pronounced, but follow the same pattern (1990: Table 1).

Conclusions

The issue of penalty and crime has been controversial for centuries. It is interesting to note that utilitarians (Bentham and Beccaria) argued for reductions in punishment during a period when punishment was cruel.25 Today utilitarian arguments are used for the opposite purpose: to justify increases in penalties, the death penalty, etc. At the beginning of this paper, I used two excerpts to indicate the opposing positions that prevailed two decades ago. My substantive position on the matter (that penalty has no impact on crime) can be traced back at least one hundred years to

24. The coefficients are extremely significant (t-statistics above 3 or 4).
25. For Bentham 'All punishment in itself is evil' (1789, ch. 15, section 1). It is justified only by its deterrent effect.
Durkheim. It is natural for the reader to ask him/herself a series of questions. Are we going through another cycle of the crime and punishment debate? Can one construct formal models to defend any conclusions? If the answer to the previous questions is positive, what is the use of modeling social phenomena?

Herein, I have given my answer to the question of penalty and crime, and I have defended it on both theoretical and empirical grounds. Nonetheless, I understand that given the very strong priors held by the participants (including scientists) to the debate, readers may still cling to their previous position. In this conclusion, instead of summarizing my substantive arguments, I prefer to give my (personal) impression of the state of the debate and some answers to the remaining questions.

In my models the public and the police, rather than deciding independently, are involved in a game. Therefore, the probability of monitoring is not independent of crime; understanding the behavior of players who are involved in a game as if the behavior were independent of the opponent is what I have called the Robinson Crusoe fallacy. My critics agree with me on this: police and criminal behavior should be examined as part of a game, and determined in such a way that they are optimal responses to each other. This point of agreement differentiates the participants in this debate from the decision theoretic literature on crime.

A consequence of this difference between decision theory and game theory is that all the participants in this debate agree that an increase in penalties adversely affects the behavior of the police (by reducing monitoring in equilibrium). This result is different from earlier expectations. When Ross claims that the Road Safety Act was ‘betrayed’ by the behavior of the police, he commits the Robinson Crusoe fallacy, because he expects police behavior to be independent of the behavior of the public. Similarly, when Wilson (1983: 133) argues that ‘A tougher, and for policy purposes, more useful test of deterrence would be to alter the sentences a person gets without altering police conduct’, he expects police behavior to be independent of the behavior of the public. Consequently, there are points of agreement between myself and my critics which indicate that we are not repeating previous debates, but rather building upon them.

26. In 1893 he wrote: ‘Punishment does not serve, or else serves quite secondarily, in correcting the culpable or in intimidating possible followers. From this point of view, its efficacy is justly doubtful, and, in any case, mediocre’ (Durkheim, 1964: 108).

27. For example, in his review of the econometric literature on crime Cameron (1988: 308) asks: ‘Why are economists so keen to endorse deterrence when they could reasonably suppose it doesn’t work? Prior social conditioning could be responsible as economists grow up in a particular culture and are no more immune to its myths than anyone else’.
What about the disagreements? Here too there is progress. Although we defend different substantive positions, our debate generally occurs on a different level than do disagreements of principles. Our discussion revolves around the realism of our assumptions. Issues like use of mixed strategies, number of players (finite vs infinite), modeling multiple police agents as independent players or as types of the same player, etc. are more likely to lead to agreement down the road about which model is a better approximation to the reality of different types of crime. In this sense, our debate about modeling parallels the debate about the best way of estimating effects of different variables in the econometric literature on crime (Cameron, 1988). Focusing more narrowly, inside the rational choice tradition, a debate about the realism of assumptions is a net progress compared to previous epistemological arguments where what mattered was the accuracy of predictions alone.  

So, modeling police–public interaction as a game presents important advantages for understanding crime. All the participants in this debate agree that a change in (expected) penalties adversely affects the behavior of the police. We have been debating whether this change in police behavior exactly offsets the change in penalty. This last part of the debate may seem old, but the fact that century-old positions re-emerge in this debate should not distract the reader from realizing that the whole debate here is based on a new conception of the monitoring of crime.

APPENDIX A

Assumptions: \( c_1 > a_1, b_1 > d_1, \)
\[ a_2 > b_2, a_2 > b_2', d_2 > c_2 \]

Reminder: in the Robinson Crusoe article the equilibrium frequencies of crime and police enforcement, respectively, were:

\[
p^* = \frac{d_2 - c_2}{a_2 - c_2 + d_2 - b_2} \quad (1^*)
\]
\[
q^* = \frac{b_1 - d_1}{c_1 - d_1 + b_1 - a_1} \quad (2^*)
\]

In equilibrium the public will use a mixed strategy and violate with probability \( p^* \) (to be calculated). Since there are two types of police, only one of them can be indifferent between the two strategies and mix. The other type will play a pure strategy. Therefore, there are four candidates for equilibria.

1. The public mixes (\( V, NV \)); the first type of police plays \( M \), the second type mixes (\( M', NM' \)). The equilibrium strategies are the following:

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28. The ‘as if’ argument proposed by Friedman (1953). For a discussion see Tsebelis (1990a: ch. 2).
\[ p(V, NV) = \frac{(d_2 - c_2)/(a_2 - c_2 + d_2 - b_2)}{q(M) = 1, q(M', NM') = \frac{(b_1 - d_1)/(c_1 - d_1 + b_1 - a_1)(1 - y)}{y/(1 - y)} \]

The frequency of monitoring in equilibrium is \( q^* = yq(M) + (1 - y)q(M', NM') \). After algebraic calculations, the frequency of monitoring is the same as in (2*).

2. The public mixes \((V, NV)\); the first type of police mixes \((M, NM)\), the second type plays \(M'\). The equilibrium strategies are the following:

\[ p(V, NV) = \frac{(d_2 - c_2)/(a_2 - c_2 + d_2 - b_2)}{q(M') = 1, q(M, NM) = \frac{(b_1 - d_1)/(c_1 - d_1 + b_1 - a_1)y}{y/(1 - y)} \]

The frequency of monitoring in equilibrium is \( q^* = yq(M, NM) + (1 - y)q(M') \). After algebraic calculations, the frequency of monitoring is the same as in (2*).

3. The public mixes \((V, NV)\); the first type of police mixes \((M, NM)\), the second plays \(NM'\). The equilibrium strategies are the following:

\[ p(V, NV) = \frac{(d_2 - c_2)/(a_2 - c_2 + d_2 - b_2)}{q(M') = 0, q(M, NM) = \frac{(b_1 - d_1)/(c_1 - d_1 + b_1 - a_1)y}{y/(1 - y)} \]

The frequency of monitoring in equilibrium is \( q^* = yq(M, NM) \). After algebraic calculations, the frequency of monitoring is the same as in (2*).

4. The public mixes \((V, NV)\); the first type of police plays \(NM\), the second mixes \((M', NM')\). The equilibrium strategies are the following:

\[ p(V, NV) = \frac{(d_2 - c_2)/(a_2 - c_2 + d_2 - b_2)}{q(M) = 0, q(M', NM') = \frac{(b_1 - d_1)/(c_1 - d_1 + b_1 - a_1)(1 - y)}{y/(1 - y)} \]

The frequency of monitoring in equilibrium is \( q^* = (1 - y)q(M', NM') \), which is the same as in (2*).

Some of these candidates for equilibria may not be feasible, because they produce values of \( q(\ldots) \) outside the \([0,1] \) interval; however, the frequency of law enforcement is always the same as in (2*).

**APPENDIX B**

The game in normal form is presented in Table 1 (the players move simultaneously).

Assumptions: \( g_1 > d_1 > a_1, h_1 > b_1 > e_1, c_1 > f_1 > i_1 \)
\[ a_2 > b_2 > c_2, e_2 > d_2 > f_2, i_2 > h_2 > g_2 \]

Calculation of equilibrium strategies: at equilibrium each player is indifferent among his three strategies. Consequently, \( EU_{V1} = EU_{V2} = EU_{NV} \) and \( EU_{M1} = EU_{M2} = EU_{NM} \). These equalities translate to the following system of 4 equations with 4 unknowns:

\[ (g_1 - a_1)q_1 + (h_1 - b_1)q_2 + (i_1 - c_1)(1 - q_1 - q_2) = 0 \]
\[ (g_1 - d_1)q_1 + (h_1 - e_1)q_2 + (i_1 - f_1)(1 - q_1 - q_2) = 0 \]
\[(a_2 - c_2)p_1 + (d_2 - f_2)p_2 + (g_2 - i_2)(1 - p_1 - p_2) = 0 \quad (3B)\]
\[(b_2 - c_2)p_1 + (e_2 - f_2)p_2 + (h_2 - i_2)(1 - p_1 - p_2) = 0 \quad (4B)\]

Solving the system (1B)–(4B) gives the following equilibrium strategies:

\[q_1^* = \frac{[(c_1 - i_1)(h_1 - e_1) - (h_1 - b_1)(f_1 - i_1)]}{[(g_1 - a_1 + c_1 - i_1)(h_1 - e_1 + f_1 - i_1) - (h_1 - b_1 + c_1 - i_1)(g_1 - d_1 + f_1 - i_1)]} \quad (5B)\]

\[q_2^* = \frac{[(g_1 - a_1)(f_1 - i_1) - (c_1 - i_1)(g_1 - d_1)]}{[(g_1 - a_1 + c_1 - i_1)(h_1 - e_1 + f_1 - i_1) - (h_1 - b_1 + c_1 - i_1)(g_1 - d_1 + f_1 - i_1)]} \quad (6B)\]

\[p_1^* = \frac{[(e_2 - f_2)(i_2 - g_2) - (d_2 - f_2)(i_2 - h_2)]}{[(a_2 - c_2 + i_2 - g_2)(e_2 - f_2 + i_2 - h_2) - (b_2 - c_2 + i_2 - h_2)(d_2 - f_2 + i_2 - g_2)]} \quad (7B)\]

\[p_2^* = \frac{[(a_2 - c_2)(i_2 - h_2) - (b_2 - c_2)(i_2 - g_2)]}{[(g_2 - a_2 + c_2 - i_2)(h_2 - e_2 + f_2 - i_2) - (h_2 - b_2 + c_2 - i_2)(g_2 - d_2 + f_2 - i_2)]} \quad (8B)\]

Necessary and sufficient conditions for existence of this mixed strategy equilibrium are that the numerators of (6B) and (8B) are positive.

Comparative statics: it is obvious that the payoffs of player 1 affect the mixed strategies of player 2 and vice versa. Consider now the following modification of payoffs: violations 1 and 2 if detected carry double the previous penalty. In our model this modification doubles the values of \(a_1, b_1, c_1\) and \(d_1\) (the reader is reminded that these payoffs are penalties, and therefore negative). These modifications do not affect the assumptions of the model, or the conditions for existence of the equilibrium. Consequently, in this model doubling penalties has no effect on the behavior of the public.

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