

## Response to Rapoport

Professor Rapoport's comments raise the very important issue of the realism of assumptions in modeling. His argument is that it is only in the rare cases where utilities can be assigned to outcomes with reasonable accuracy that game theory can be used in a descriptive or normative way; in the vast majority of social situations, it can be used only heuristically.

Estimation of frequencies aside, I agree with his argument. Moreover, I consider the concern with the realism of assumptions a very positive development in the epistemological foundations of modeling for the following reason.

The standard answer that builders of rational choice models have given to the question of the realism of assumptions is (along the lines of Friedman)<sup>1</sup>: "It does not matter if assumptions are unrealistic, so long as the predictions are correct; it is 'as if' the assumptions were true." This answer is intellectually unsatisfactory and can generate unnecessary and rigid divisions inside the social sciences. It is unsatisfactory because the assumptions of a theory are, in a trivial way, also conclusions of that theory. Therefore, scientists who care about the realism of their conclusions should also care about the realism of their assumptions. It can generate unnecessary and rigid divisions because some modelers may feel justified to use "wildly inaccurate assumptions" (to use Friedman's terminology), while other social scientists will thereby mistrust models and dismiss them as irrelevant to the real world.

In my own work, I have tried to substitute the concept of "rationality as model" of human behavior with the concept of "rationality as subset" of human behavior:

The change in perspective is important: I do not claim that rational choice can explain every phenomenon and that there is no room for other explanations, but I do claim that rational choice is a better approach to situations in which the actor's identity and goals are established and the rules of the interaction are precise and known to the interacting agents. As actors' goals become fuzzy, or as the rules of the interaction become more fluid and imprecise, rational choice explanations will become less applicable. (Tsebelis 1990, 32-33)

So, concerning the realism of assumptions issue, I am on the same wavelength as Professor Rapoport.

However, I believe that the two categories with which he classifies game-theoretic models should be understood as extremes of a continuum. Game-theoretic models may be more than just exercises in mind stretching (I would prefer the word "heuristic"), even if utilities cannot be "estimated with reasonable accuracy on sufficiently strong scales." Alan Musgrave (1981), for example, in a discussion of the role of assumptions in models, distinguished three different kinds of assumptions: negligibility, heuristic, and domain. The first two correspond to the two categories proposed by Professor Rapoport and to the two extremes of my scale.

Negligibility assumptions are those that exist for reasons of simplicity; violating them does not matter. An example is the assumption of vacuum when physicists study free-fall. In the case of Rapoport's example of European transportation, it does not matter whether we speak about streetcars or subways, whether buses run in the morning or in the evening, what their schedule is, and so on. These aspects of the problem are secondary compared to the essential characteristics of the situation: the game between public and controllers.

Heuristic assumptions are those which are not true but are essential to the construction of a (preferably) preliminary version of a model in order to demonstrate fundamental aspects of an interaction. For example, the unified actor assumption in my police-criminals game is a heuristic assumption. It is used to describe what I call the bare-bones model, and it is relaxed in subsequent models.<sup>2</sup>

The third kind of assumption in Musgrave's terminology is the domain assumptions. They correspond to the middle of the continuum in my understanding of assumptions, but they do not exist in Rapoport's classification. Domain assumptions restrict the areas of applicability of a theory. It seems to me that Rapoport's terrorism example, as well as my article, provide illustrations of domain assumptions.

In most of my article, I assumed that potential criminals prefer to violate the law when the police are not around and not to violate in the presence of the police. Similarly, the police prefer to monitor when there is crime and not to monitor when there is no crime. The preceding statements were assumptions 1 through 4 of my article. Terrorists in Rapoport's example have compelling (psychological or other) reasons to perceive their payoffs in such a way as to give them a dominant strategy: They are willing to engage in their activities regardless of the presence of law enforcement officials.<sup>3</sup> I use a similar example (on page 272 of my article) concerning illegal immigration

to the United States. In this case, too, immigrants have a dominant strategy to cross the border. If terrorists (or immigrants) have a dominant strategy, it is obvious why terrorist acts (or illegal immigration) occur. Moreover, in the case of terrorism, if terrorists and law enforcing agencies have a continuum of strategies available, escalation is likely.

This is not simply an exercise in mind stretching. It is a means of making sense of the most important characteristics of a frequent and complicated phenomenon. The fact that strong scales for measurement of terrorist utilities do not exist made precise numerical conclusions impossible but did not impede our understanding Rapoport's argument and agreeing with his expectations (predictions). The assumptions of my article could not and were not meant to cover this particular kind of crime. Therefore, their modification was necessary to understand the interaction between terrorists and law enforcement agencies and then to form expectations about the relation between the size of penalties and the frequencies of crime.

In my article, I used a generic (and unspecified) kind of crime. This seems to have generated some misunderstandings. I did not mean to lump all crimes into the same group. I meant rather that my argument applied to a series of crimes for which assumptions 1 through 4 held: speeding, jaywalking, burglary, robbery, theft, selling of illegal substances (alcohol, drugs, and so on), white-collar crime, and others. In all these cases, my conclusions will also hold. It is true that I cannot measure the payoffs either of the police or of the potential criminals precisely for the reasons that Professor Rapoport specifies. However, I believe that I am on safe ground if I assert that for many people (both potential criminals and police) involved in corresponding situations, assumptions 1 through 4 hold. It is also true, as he asserts, that I cannot measure the percentage of these people either within the police force or in the general public. However, as I demonstrate in my article, most of the time it does not matter: The existence of such types of police agents and public is sufficient to drive the outcome of the police-public game to the same mixed-strategy equilibrium, even if law-abiding citizens and good police agents form majorities of their respective populations. So, assuming this heterogeneity of police and public neither modifies the equilibrium nor, therefore, the qualitative conclusions of my model.

One important advantage of modeling is that it proceeds deductively. It starts with simple assumptions, ignoring complications that at a certain level can be considered inessential, and if the conclusions do not match reality, it can modify some of the assumptions or add new ones. I believe that in my article I made a constant effort to increase the realism of my assumptions. I started from the economic and the sociological approach to crime and

introduced one additional assumption: the police as a rational agent. The results changed dramatically. Then, after a series of models, I presented a more complicated case where both police agents and public were composed of two different categories: good and bad police agents, and law-abiding citizens and potential criminals. My results remained the same.

It is true that even Model 6, which is the most complicated in my article, makes important simplifications of reality. Professor Rapoport points out one of them: Soldiers of organized cartels do not care about the presence of the police, only about orders from their superiors. I think that there is another one: In all my models, the police never face a budget constraint. Such complications can be grafted onto my model to generate more “context-specific” models. For example, a model with organized crime and a budget constraint for the police would be more appropriate for the study of a problem like drugs.

To conclude, I share Professor Rapoport’s concern with the realism of assumptions. However, assumptions cannot be classified dichotomously as either realistic (leading to empirical tests) or unrealistic (leading to mind stretching). There is a whole continuum. Quite frequently, assumptions fall between these two extremes; in Musgrave’s terminology, they are domain assumptions. Absence of strict measurement scales notwithstanding, I believe that Professor Rapoport was moving between these two extremes in his terrorism example, as was I in my article.

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## NOTES

1. Friedman’s seminal article “The Methodology of Positive Economics” appeared in 1953 and has since generated a remarkable volume of literature.
2. See Models 3, 4, 5, and 6 in Table 3 of my article.
3. In the terminology of my article, they violate Assumption 2.

## REFERENCES

- Friedman, M. 1953. The methodology of positive economics. In *Essays in positive economics*, edited by M. Friedman. Chicago: University of Chicago Press.

Musgrave, A. 1981. "Unreal assumptions" in economic theory: The F-twist untwisted. *Kyklos* 34:377-87.

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