

Remarks on "Political equilibrium"

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Mr. Nilson correctly points out the severe limitations of Richardson's model of international politics, which I have also attempted to do in the article he discusses. I feel, however, that certain important aspects of Richardson's position need to be clarified. Richardson can no longer speak for himself, and it may seem presumptuous for someone else to do this for him. However, there is considerable unanimity among all builders of mathematical models in behavioral science on what the values and the limitations of such models are, and so in my reply I shall be doing no more than reiterating a position which has been stated many times by many workers, including Richardson.

First, a mathematical model is not a metaphor but a logical scheme. Its conclusions are not analogies drawn between apparently similar phenomena but deductions from a set of postulates. If the conclusions are manifestly false, so must the assumptions be. If the conclusions are true, the assumptions are corroborated but not necessarily proved to be true, because it may be possible to derive the conclusions from different assumptions.

Therefore, any interpretation of Richardson's model as a metaphorical analogue to a mechanical system misreads the purpose of the model, and this applies to all mathematical models. The mathematical model is strictly circumscribed. It asserts no more than is explicitly stated, but also no less. This usually turns out to be both *more*

and *less* than is apparent from a gross observation of the phenomenon in question. For example, to compare the business cycle to the motions of a frictionless pendulum in the *mathematical* sense is to say both more and less than can be said about either a pendulum or a business cycle from gross observations. The mathematical model says more, because it not only asserts that the pendulum swings "back and forth" but also describes its motion with mathematical precision. But the mathematical model also says less because it says nothing about the "causes" of the motion, as "cause" is conceived in the common-sense view (e.g., whether the impulses come from "inside" or from "outside"), nothing about the material from which the pendulum is made, nothing about the use to which it is put, etc.

The mathematical model subsumes under a single mathematical description all phenomena regardless of content which are described by the same set of equations. The pendulum model says only: "A quantity changes in such a way *as if* it were the horizontal displacement from a position equilibrium of a mass point suspended by a rigid rod; that is, *as if* its acceleration were proportional (for small deviations) to the amount of displacement and oppositely directed."

All quantities which change precisely in this way (namely, sinusoidally) in time are subsumed under the pendulum model. An-

other way of stating it is this: *If* a displacement is countered by a "force" (we must use quotation marks because elsewhere such "forces" may be purely symbolic) negatively proportional to the displacement, *then* the variation of the displacement in time will be sinusoidal.

Returning to Richardson, all that his arms-race model says is this: "*If* the rate of change of armament expenditures is a linear combination of three terms—one positively proportional to the armament expenditures of the rival, one negatively proportional to one's own armament expenditures, and one embodying the 'strength of existing grievances'—then the time course of the armament expenditures of the two rival states should be such-and-such. Moreover, if certain inequalities obtain among the proportionality coefficients, then the armament expenditures will be stabilized; if the reverse inequalities obtain, the expenditures will either grow without bound or, on the contrary, will vanish and become 'negative' (interpreted as excess of mutual trade over armament expenditures). Which of these two alternatives will obtain depends on the levels of armaments and trade at the start of the process."

This, and only this, is what the *model* says. A *theory* of arms races and trade volumes can conceivably be built on these foundations, if suitable, consistent indexes can be found to denote armament expenditures, trade volume, "amount of grievance," etc., provided sufficient data are on hand to plot these quantities over considerable intervals of time. The content of the theory would be an interpretation of the indexes, including, perhaps, an interpretation of "unbounded growth" of armaments. In Richardson's theory "unbounded growth," for example, is interpreted as an outbreak of war. The equations do not say this; it is an extra assumption of the theory.

As far as I know, only one unequivocal

but exceedingly weak corroboration of Richardson's model has been offered, namely, the expenditures of the great powers in the armament race of 1909-13. The corroboration was unequivocal, because the accuracy of prediction of the model was very good; it was very weak, because only four points were obtained on the graph, two of which served to fix the parameters.

The limitations of the theory based on the model, therefore, are obvious. Data are meager; the indexes are difficult to establish (e.g., unequal purchase power of money units, ambiguous budgets, etc.); the "dynamics" are complicated by imperfect polarity of the rival camps; quantification of "grievances" is all but impossible; etc. Moreover, there is an "embarrassment of riches" with regard to the choice of models. Why, for example, should the effects of mutual stimulation, of self-imposed restraints, and of grievances be additive? Why should they not combine in some other way? The number of free parameters will enable the theoretician to fit practically any model to any reasonably smooth curve of armament expenditures. How is one to choose among the various plausible models?

All these are legitimate objections to a mathematical model and to a theory based on it. But the objections offered by Mr. Nilson are of a different sort. The objection that the notion of "neutral equilibrium" is as important as that of stable and unstable equilibrium is not really relevant when offered as an objection to Richardson's mathematical model. For "neutral equilibrium" is a special case there: it obtains when a certain equality (instead of inequality) among the parameters is satisfied.

Actually, Mr. Nilson's objections are relevant not to Richardson's mathematical model at all but against Richardson's *conceptual scheme*. But that scheme is not the drawing of metaphorical analogies with mechanical systems (as Mr. Nilson seems to imply)

but rather the method of treating events related to international politics by systems of linear differential equations, with the underlying assumption of continuity, namely, that minute changes in one variable produce minute changes in another and that the effects of several variables can be considered as simply additive.

The examples offered all pertain to this criticism: Queen Anne's decision to support the one party or the other; the threats and counterthreats at the Congress of Vienna, at the bargaining table in labor disputes, etc. These "moves," "calculations," and "decisions" have no place in Richardson's conceptual scheme.

If, then, one maintains that it is such discrete, discontinuous events which are of crucial importance in political science, rather than the sort of "causality" which is described in physics by differential equations, one brings to bear a crucial critique upon Richardson's conceptual scheme. In a way, the theory of games uses a conceptual scheme much more conducive to taking decisions, calculations of consequences, etc., into account, and one could well argue that, if a rigorization and mathematization of political science is to take place, game theory will prove a more useful intellectual tool than the conceptual arsenal of mathematical physics, on which Richardson has drawn.

But, of course, game theory is beset with its own difficulties. Its theorems rest on a definition of utility which has never received sufficient empirical realization. It assumes "complete omniscience" of the participants with respect to all possible outcomes and with respect to the valuation of all the outcomes by all concerned and also "complete rationality" of the participants with respect to maximization of utilities under the constraints of the situation and many other implausible conditions.

Of course such limitations are the lot of any mathematical theory. The mathematical theorist must pay by sacrificing realism for what he gains in rigor and tractability of reasoning. The mathematical theorist also foregoes the privilege of asserting that something is "so." All he can say is that, if something *were* so, then something else would necessarily *have* to be so. The "if" part is always questionable, but the "then" part is always a compelling consequence of the "if" part.

The strength of the mathematical method becomes apparent as soon as there are two or more widely different models or conceptual schemes. For then there is hope of eventually settling at least in part some heretofore sterile debates conducted in the metaphysical realm.

Is there historical determinism, or do leaders and geniuses play a decisive role in history? Tolstoi held to the first alternative, and Carlyle to the second. Mathematics cannot presume to treat this subject on such a grandiose scale. But in a more modest setting significant answers can be given. To the extent that certain trends (most likely including mass behavior, economic indexes, regular ebbs and flows of attitudes) can be described by equations of the kind used in physics and chemistry, the first hypothesis is corroborated. To the extent that single acts and their valuations, strategies, counterstrategies, choices, and decisions enter the formal scheme in which the observed events are deduced as consequences, more credence can be given to the type of hypothesis of which Carlyle's formulation is a poetic expression.

Very likely both approaches are relevant in different sectors of human affairs and even in different aspects of the same sector. We should not allow our emotional commitments to disregard the possible importance of either.