The Formal ('Economic') Analysis of Arms Races: What—If Anything—Have We Learned Since Richardson?

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1. Introduction

The dominant "action-reaction" model of arms races can be traced back at least to Montesquieu (Luterbacher 1985a, p. 41), but—as far as is known—its first formalization is due to Lewis Fry Richardson whose truly seminal work, *Arms and Insecurity*, became available as a micro-film in 1947-1949, to be published as a proper book in 1960 only.[2]

The purpose of this paper is to try and ascertain to what extent the core of Richardson's formal model has survived since then, and/or in which ways it has been extended, completed, or possibly falsified; and to suggest areas where more work is needed. Since a nearly exhaustive survey of the field has just been published (Isard-Anderton 1985 and Anderton 1985), no systematic scanning will be attempted here. Instead, the analysis will more or less revolve around the present writer's own work in this area over the past twenty years, the opportunity being used for a *critical* retrospective examination. The paper's organization around the author's own contributions does not reflect any propensity at self-absorption or self-aggrandizement, as it was adopted in response to a specific invitation from the organizer of the meeting where this paper was first presented.[3]

The analysis will take up a number of relevant topics, in no particu-

lar order, while always trying to relate them to Richardson's major book whose richness was forcefully driven home by a fresh reading.

2. Progress in Model Estimation and Simulation[4]

This is one area where considerable progress has been achieved since the days of Richardson. Indeed, the parts of his book which today read least well are conceivably those where he tries to estimate his model's parameters, since the method used is truly rather primitive, ignoring matters such as degrees of freedom, two-variable or *a fortiori* multiple-variable regression, residual analysis, etc. Actually, nothing more is offered but the simplest type of curve-fitting.

Considering that the world's first econometric model was estimated on the eve of World War II by Jan Tinbergen (working in Geneva under the auspices of the League of Nations), considering further that the archetypical Klein-Goldberger model of the U.S. economy became well known in the early 1950s and that econometric forecasting became operational in the Netherlands and in the U.S. around the same time, it took very long before modern statistical tools were applied to arms race models. Indeed, one of the first "econometric" models to be published in this field appears to be this writer's model of the arms race in the Middle East, which was published as late as 1971.[5] This model was estimated by ordinary least squares (OLS) applied to a set of data in level form (ie. real defense expenditures and real GNPs in a number of Arab countries and in Israel); in other words, the model's equations were specified as difference equations, and not as differential equations *à la* Richardson, about which more presently.

Econometrically speaking, there were quite a few weaknesses in this 1971 model. The model did include the relevant GNPs among the explanatory variables (see below, section on the role of supply constraints), but since the trend values of GNP were taken there was no *prima facie* reason to fear a simultaneous-equation bias.[6] However, the efficiency of the estimation procedure could have been improved by applying Zellner's method for systems of seemingly unrelated equations. Given that the sample was limited to thirteen annual observations, and considering that the various equations each included up to five free parameters, there would appear to be a problem of degrees of freedom. The large number of parameters was mostly due to the use of several dummy variables, which in turn were meant to capture various punctual events whose quantitative effect on the arms race was thought to be of interest (e.g. the 1955 abrogation of the 1950 regional arms rationing scheme known as the "Tripartite Declaration," the short-lived 1958-1959 union between Egypt and Syria, Egypt's long and costly involvement in the Yemen war, etc.) In his 1975 survey, Urs Luterbacher aptly recalls in this context the old saying that "with four parameters you

can fit an elephant and with eight you can make him wiggle his tail" (p. 213). Fair enough, but it can be replied today that the particular sample used to estimate this model, although small, happened to be relatively information-rich and in fact close to orthogonality, so that the estimated parameters for the "systematic" variables (such as the actions of the other side, one's own resource contraint) generally turned out to be quite robust; ie. they hardly changed when the numerous dummies were excluded from the equations. However, it is true that this was not mentioned in the article, parameter robustness being less of a concern then than today.

Another weakness of this early model was the simple dynamic structure used, with one-period discrete lags only, which meant a high adjustment speed. Given that much of the military hardware involved could be bought "off-the-shelf," this was perhaps not completely implausible, but even so it takes more than one year to train and equip the mainpower necessary for, say, the creation of a new armored division. Similarly, expectations and the way they are formed were largely (but not entirely) absent from this Middle Eastern model. Finally, the model was simple enough to be solved and simulated by hand, which is no fault, but the evaluation of the simulation results stopped short of today's standards.

It is my impression that the estimation of arms race models has since then caught up with the state of the (econometric) art and that the appropriate available quantitative techniques are generally used when required; to that extent, "model estimation and simulation" is no longer an issue—or more accurately, it is no more of an issue with arms race models than with other types of models. It may even be that the formal analysis of arms races since Richardson has actually seen some pioneering work in the general field of model estimation—meaning the continuous-time techniques as applied (not accidentally) to systems of *differential* equations *à la* Richardson.

Continuous-time estimation methods had been proposed before in econometrics (see for example Wymer 1974), but they generally involved the use of cumbersome and inelegant approximation procedures. As far as I know, it was arms race analysts such as S. Bremer (see e.g. 1977) and most particularly Urs Luterbacher and his team in Geneva (see e.g. Luterbacher *et al.* 1979, 1982; Allan *et al.* 1984) who for the first time conceived and implemented a "clean" estimation method for continuous-time systems. (In their case, this was achieved by combining a very flexible Fortran-based simulation language, DARE-P, with MINUIT, a collection of numerical minimization routines thought up at CERN, the *"Centre européen de recherche nucléaire"* in Geneva).

The relative advantages and drawbacks of discrete vs. continuous-time models and hence estimation techniques is still very much an open issue, at least in my opinion. One advantage of continuous-time techniques as used by (e.g.) Urs Luterbacher and his team is that they readily

lend themselves to the estimation of genuinely *dynamic* equations, more readily in any case than the "nesting" procedures which have been proposed in traditional, discrete-time econometrics—see Judge *et al.* (1985), pp. 660-9 and 855-881, for a general description and Beenstock-Dicks (1983) for an example of an empirical application of these nesting procedures.[7] Another advantage of models specified in continuous time is that they avoid the kind of ambiguity which often arises in discrete-time econometric models when truly discrete observations (say, the interest rate at time t*) are mixed with variables which are really integrals with respect to time (eg. quarterly or annual figures for GNP), one of the better known consequences being that it then becomes difficult to compare econometric results obtained for the same sample period from, say, quarterly data with results calculated on the basis of, say, annual figures (see e.g. Houthakker-Taylor 1970, pp. 13 *et seq.* and *passim*).

On the other hand, it seems—by what I know—that the continuous-time estimation methods are generally applied *"one equation at a time"* whereas procedures for the simultaneous estimation of all the parameters of a multi-equation model exist and are nowadays used extensively in traditional econometrics.[8] Because continuous-time methods combine successive dynamic simulation runs over the entire sample period with some numerical minimization procedure for parameter estimation (eg. the Fletcher-Powell-Davidon algorithm), the task probably becomes simply too formidable as soon as more than two or three equations are involved.

Whatever the case may be on this count, continuous-time models and estimation techniques have by now been shown to be fully operational—see e.g. Lambelet-Luterbacher with Allan 1979[9]—although not all problems have been solved, even at the conceptual level. Thus, it is *a priori* not clear whether the stochastic term in continuous-time models should affect the differential equations themselves or their solution upon integration, which has a bearing on whether the estimation of "unstable" arms races entails special difficulties—a question to which we now turn as the last one in this section.

In his 1975 review article, Urs Luterbacher—taking his cue from Ferejohn (1970-1976)—shows that no reliable estimates of parameters can be found for a linear, endogenously unstable relationship, and he does so by using a one-equation model where the stochastic term is added to the differential equation, and not to its solution upon integration. The basic problem is then that, if the process is unstable, its variance "does not tend toward a constant term even though the variance of the errors has been assumed to be constant" (p. 215).[10] This means that estimation techniques break down just when they would be most needed, ie. in the case of unstable ("explosive") arms races . . . Furthermore, Urs Luterbacher conjectures that if the estimation techniques are nevertheless applied to an unstable race, the results of the estimation exercise will be

biased towards stability.[11] To my knowledge, there have been no further investigations into this matter whose importance will be underscored again in the next section dealing with war initiation among other things.

3. Supply Constraints, Unstable Races, and War Initiation

For Richardson, an arms race, to be worth the name, had to be unstable (explosive). This prompted the following comment by the authors of the preface to the 1960 edition of *Arms and Insecurity* (N. Rashevsky and E. Trucco, p. vi):

> A study of the solutions of [Richardson's] differential equations for armaments races shows that the amounts of armaments either tend to constant equilibrium values or they increase to infinity with time, depending on the values of different socioeconomic and psychological parameters. Richardson then *assumes* that when armaments can reach constant equilibrium values, no war results. On the other hand, if the armaments increase indefinitely, war eventually breaks out. (Emphasis in the original).

Richardson himself was not aware of the objections which these views of his were bound to call forth—to wit (p. 61):

> In a review (1939) of the author's monograph, which also contained those ideas, Professor H.T.H. Piaggio remarked: "An infinite cost of armaments is interpreted as denoting war, though it might have seemed more natural to have taken it as bankruptcy." This criticism deserves to be answered carefully.

Unfortunately, Richardson's fourfold answer fails to convince. For example, he states *(ibid.)*: "I do not know of any arms race that ended in bankruptcy. Transitions of this type must certainly be rare: a fact which requires explanation."[12]

Today, of course, possibly under the influence of microeconomic theory, we refer no longer to the extreme possibility of "bankruptcy," but rather to the *general resource constraint* to which any arms race must be subjected. And indeed, one way in which Richardson's original approach has since then been improved is precisely the explicit inclusion of such a constraint in (most) arms race models.

According to Luterbacher (1975), this enrichment goes back, at least insofar as formal analysis is concerned, to Caspary's 1967 article. However, "the parameters C and K, which [in Caspary's model] stand for maximum amounts available for defense allocations, could very well be functions of time" (*ibid.*, p. 203). Moreover, Caspary's ideas remained on a theoretical level and he did not try to test them empirically. It seems that one of the first attempts to allow for the resource constraint by making defense outlays a function of real income (GNP), with an elasticity

either smaller or larger than plus one depending upon whether external security is considered as a necessity or as luxury, is (embarassingly) again this writer's 1971 Middle Eastern model. However, the simple log-linear specification adopted there meant that defense expenditures could conceivably grow to be larger than total GNP, which is altogether impossible in the case of the superpowers and quite unlikely in the case of smaller powers (which can be recipients of significant amounts of military aid from abroad).

This defect was eventually corrected in Lambelet-Luterbacher-Allan (1979) where equations of the semi-logarithmic reciprocal form are proposed and used for empirical verification. Originally, this specification was adopted because it presents a number of features which are clearly desirable on *a priori* grounds—such as, precisely, total GNP seen as an absolute upper limit on defense expenditures. Since then, it was found that equations of this type can also be interpreted as *arbitrage* functions (meaning that the allocation of total resources as between civilian and defense purposes is made with respect to an explicit criterion), and also that these equations can be derived from variational principles in a utility-maximizing context—see Lambelet-Luterbacher 1985. Therefore, and until something better comes along (or comes to my attention), it may perhaps be assumed that the semi-logarithmic reciprocal specification is the most satisfactory known way of allowing for the overall resource constraint to which all arms races are subjected.

Given a supply constraint which changes over time as total available resources grow, an arms race can be viewed either as stable, in the sense that at any moment in time it has a finite steady-state equilibrium solution—or as "unstable", in the sense that each side's defense efforts will grow over time, possibly at a fast clip depending on economic growth, so that a race is indeed taking place or appears to take place. It can perhaps be surmised that in much of the recent formal literature the latter interpretation is basicaly how the arms race process is viewed, in the sense that the possibility of a steady-state solution at infinity is generally ruled out.

This however leaves unanswered a major question, ie.: If this is so (ie. if arms races are basically stable), *how come wars do break out*?

The question was very much in the front of Richardson's mind and it could even be maintained that war initiation, rather than the arms race process, is the central theme of *Arms and Insecurity*, at least in its non formal parts. However, as already mentioned, the answers to this question given or suggested by Richardson were not really satisfactory. Following a semi-formal and somewhat hasty first examination by Lambelet (1975b),[13] the subject matter was taken up again in a number of major contributions by Brito-Intriligator and Intriligator-Brito. In a series of papers (see e.g. 1985), these authors conceive of war as avoidable through wealth redistribution and they emphasize the role of errors

in the outbreak of war. Without denying the possibilities analyzed by Brito-Intriligator and Intriligator-Brito, which are real enough, their views seem to cover only a part of the subject matter. In other words, their work appears to be in need of being completed on a number of counts. First, many wars are planned and carried out deliberately, ie. they do not occur by accident, so that a more general theory should also allow for some wars breaking out as the outcome of an optimizing process by rational agents, in the vein of Bueno de Mesquita (1981). Secondly, some wars may result from long-term strategic calculations that preclude any "buying-off" (ie. wealth redistribution) possibility. Thirdly, the models of Brito-Intriligator and Intriligator-Brito are only partly dynamic since they are of the two-period variety.

An attempt to integrate the preceding possibilities about the outbreak of wars (possibilities for which there is good historical evidence) with the possibilities stressed by Brito-Intriligator and Intriligator-Brito can be found in Lambelet-Luterbacher (1985), together with a more general dynamic analysis. The model proposed in this latest (as of today) piece of research actually is intended as a synthesis of a number of previous models, ie.: An arms race model (or resource allocation model) using semi-logarithmic reciprocal functions, as described above; a conflict model drawn from Allan (1983) and from Luterbacher-Allan (1985b); and a (to my knowledge, novel) war decision-making model whose core is a differential game involving kinematic equations capable of producing abrupt changes in some key control variables, and hence capable of explaining the sudden outbreak of a war. To that extent, this latest model can be viewed as an attempt to reconcile the notion of arms races which are stable in the broad sense defined above, on the one hand, and the sudden "quantum leap" represented by the outbreak of hostilities, on the other. It should however be added that this particular model is probably too complex to be solvable analytically so that an investigation of its overall properties remains to be done by means of numerical and simulation methods.

4. Expectations

In its original version, Richardson's *formal* model implies that the actors in an arms race are altogether myopic, in the sense that they only react to what the other side is doing now and that they never try to anticipate, in thought or deed, the other side's future actions. (As often, Richardson's non formal analysis is a great deal more subtle and richer than his differential equations; for example, he states that the latter describe the mechanism of arms races "*if* men did not stop to think"; emphasis added). In any case, many formal models published since then allow for some departure from complete myopia, eg. via adaptive expectations—see the rubrics VII.10, VII.11, VII.18 and VII.19 in Anderton's

1985 bibliographical survey.

However, it seems that, just as it took a very long time for econometrics to cross-fertilize the modelling of arms races, the *rational expectations revolution* in economics (if that's what it is)[14] has not yet affected the formal analysis of arms races—at least not to my knowledge.[15]

This is all the more regrettable since there is (to my mind) good evidence to the effect that the basic idea of rational expectations is relevant for at least some arms races which were actually run. Specifically, it was shown in a "trilogy" on the 1905-1914 dreadnought race between Great Britain and Germany that the German side behaved in a way strikingly reminiscent of the rational expectations paradigm (Lambelet 1974, 1975a, 1976). Tirpitz, for one, had a shrewd (and, as it turned out, quite accurate) idea of how the *entire, overall mechanism* of the Anglo-German naval race was shaped, and what its ultimate outcome would be; and it also appears that he acted accordingly. Briefly put, Tirpitz figured that, given the two countries' comparative economic resources and given the competing claims on their resources, Germany could sustain a three-capital-ship annual pace to England's average four-and-half capital-ship rhythm. Except for a period when he became jittery (the so-called "danger zone"),[16] Tirpitz consequently went on steadily laying down three keels per year, no matter what England was doing. As to the latter country, it proved to be extremely reactive, and its annual naval programs fluctuated a great deal. In other words and on the formal level, England behaved (in a partly myopic fashion) according to the Richardson model whereas Germany basically took the rational expectations route. In fact, the way Germany acted is one instance where Richardson's action-reaction paradigm can be (to my mind) conclusively *falsified*; see particularly the 1976 paper, figure 4 on p. 55. There may be other such cases but, given the current state of research on arms races, we do not—or at least I do not—know of them.[17]

5. Underlying Conflicts, Motivations and Perceptions

Arms races must be about *something*!

That is, underlying any arms race there must be (or there originally must have been) some conflict, some grievance(s), some clash of interests or of *Weltanschauungen*, or what not. Richardson was very much aware of this, both in his formal model which includes one "grievance term" per actor[18] and in his non formal discussion, to wit (p. 231):

> AUTHOR: (. . .) The war of 1939 might never have begun if each side had confined itself to defensive armaments.
>
> CRITIC: Nonsense! Hitler intended war anyhow. If there had been no weapons, we should have fought with fists.
>
> AUTHOR: Then a careful study of grievances, ambitions, and greeds ought to be made.

Of course, underlying conflicts and motivations were not ignored subsequently—see for example the discussion on pp. 53-57 of the recent survey by Isard and Anderton (1985). Indeed, it has been recognized on several sides that the real problem with conflict and motivations is the possibility that the arms race itself will feed back positively on these underlying factors. Suppose that a mild initial conflict leads to an (initially mild) process of competitive arms build-up. As the latter becomes more and more tense because of Richardsonian dynamics, the race itself can become the main reason why each actor distrusts and fears the other one(s), and the race can go on and become ever faster even though the initial cause of conflict may have disappeared meanwhile. Put differently: conflict and motivations may be or may have become *endogenous* to the arms race.

Recent historical research in the traditional vein suggests that this feedback mechanism has actually been at work in a number of situations —thus S. van Evera (1985) on the events leading to the 1914-1918 World War (p. 81):

> (. . .) Europeans commonly overestimated the hostility of neighboring states; this *paranoia* eventually produced its own reality by justifying aggressive policies that provoked genuine hostility. (Emphasis added).

The problem, however, is that grievances and conflicts need not necessarily be endogenous in that sense. Genuine aggressiveness, real grievances and real conflicts not due to misperceptions of one type or another do exist. In the language of game theory, this means that one is no longer dealing with the game of "prisoners' dilemma" (or games that are relatively close to this one such as "stag hunt") but rather with the game of "deadlock." In the words of Kenneth A. Oye (1985, p. 7):

> When you observe conflict, think Deadlock—the absence of mutual interest—before puzzling over why a mutual interest was not realized.

From the *policy angle,* the importance of this issue cannot be overstated. If conflict and motivations are completely or even predominantly endogenous in a "prisoners' dilemma" context, then there is something to be said for a (cautiously implemented) strategy of *unilateral* de-escalation, possibly including progressive unilateral disarmament, as has been argued by *bona fide* pacifists; or for the less extreme "tit-for-tat" strategy as analyzed recently by R. Axlerod (1984). But these strategies and especially the first one would be dangerous, possibly suicidal, if one is faced instead with a deadlock-type situation—see Lambelet (1985a) for a more detailed discussion.

This does not mean that nothing can be done if an arms race stems from a deadlock-type situation. Indeed, Downs *et al.* in their recent essay on "Arms Races and Cooperation" suggest four types of unilateral strategies that can reduce the intensity of an arms race (pp. 123 *et seq.*).

Neither does it mean that a deadlock-type arms race will necessarily go on forever since, as the same authors put it (p. 119):

> The majority of arms races that have ended in cooperation have done so not because one side adopted a particular cooperative strategy, but because the basic character of the race was altered by events that were not directly connected with it.

It should be added that the possibility of endogenous conflict and motivations is just one particular instance of *misperception* and that there may be many others. For example, S. van Evera (1985) finds that

> World War I arose from a web of six remarkable misperceptions that were prevalent in Europe during the years before the war. Although all six ideas were especially popular in Germany, they flourished throughout the Continent (p. 81).

The possibility of endogenous motivations and various other misperceptions is part and parcel of the latest Lambelet-Luterbacher model (1985b). As a matter of fact, this model includes all the features of arms races discussed so far in this paper, with the exception of rational expectations.

6. Conclusions

There are other areas where significant progress in the analysis of arms races has been achieved since Richardson. For example, Richardson's model was highly aggregated—too highly so, in the opinion of many, and it is arguable that in most modern arms races a distinction should be made at least between conventional and strategic weapons; and indeed several recent models do just that (see Isard-Anderton 1985, pp. 48-49).[19]

As mentioned in the introduction, the purpose of this paper is not to identify and survey all new developments which have taken place since Richardson, but only the few ones which appear particularly relevant from this writer's perspective. I shall therefore stop here with a few general comments on the current state of arms race analysis as I see it.

First, there is today a least one social science where *knowledge really builds up,* namely economics. How things are in other social sciences, I do not know. But it is my impression that knowledge does not quite build up properly in the field of arms race analysis. As even a quick glance at the field will show, it is not uncommon for social scientists interested in arms races to propose insights, hypotheses and even full models in blissful ignorance of what has already been done before along the same lines. This writer does not plead innocent on this count—see eg. the end of note 5—and the present paper may indeed contain some examples of this sad state of affairs. One reason could be that most or even all social scientists who have worked in that field have done so "on the side," as it

were; this is certainly the case for me. As a result, the field may sometimes present a slightly amateurish look, which may prevent interested social scientists from concentrating full-time on it; so that things will tend to remain a little amateurish; etc. If this view is correct, it means that we have here the making of a vicious circle not entirely unlike some types of arms races.

Second, the following question is certainly worth being raised: How much of its income is the world spending on defense? Answering this question is not easy, not only because of the well-known difficulties in estimating defense expenditures and GDPs in Communist countries and most particularly in the Soviet Union. For even in the case of small and "transparent" Switzerland (for example) there are problems: According to the most reliable standard international source of information (International Institute of Strategic Studies, 1985), Switzerland spent a little more than 2 percent of its GNP on defense in 1984. However, each Swiss male must devote at least one year of its life to the army, without getting much by way of military pay. As a result, the defense budget significantly underestimates the (opportunity) cost of national defense in Switzerland, and the 2 percent figure is certainly much too low.

Trying to correct all such sources of distortion in a rigorous and scientific manner would be a major undertaking which, to my knowledge, remains to be done. In the meantime some rough calculations suggest that the world must currently be spending between 5 and 10 percent of its income on defense, ie. about the same amount as net investment (net capital formation). Now, if one compares the amount of research about capital formation, investment, their impact on growth and development, etc., with the amount of research about arms accumulation, war initiation, underlying conflicts, etc., the conclusion is inescapable: There is a major discrepancy between the resources devoted to defense *lato sensu* and more generally what's at stake in this field, on the one hand, and serious (ie. non ideological) research efforts concerning arms races and the like, on the other. Put differently: More than thirty years after Richardson's death, in spite of some significant advances since then, and notwithstanding the efforts of a handful of convinced and devoted social scientists[20], the scientific analysis of arms races in the broadest sense unfortunately still remains a relatively underdeveloped area, a "marginal" field at the periphery of today's social sciences, generally not recognized, particularly in the Universities, as a full-fledged branch of knowledge.

Notes

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[2] See Richardson 1960, pp. 289-90, for a listing of his earlier work (going back as far as 1919).

[3] Ie., the first Annual Congress of the European Economic Association, Vienna (Austria), August 29-31, 1986; session on "Conflict and Peace Economics" organized by Professor Walter Isard.

[4] See Luterbacher (1975) for an early survey which focuses on the virtues and pitfalls of econometric estimation as then applied to arms race modelling.

[5] Luterbacher (1975, pp. 199 and 206-08) mentions and discusses at some length an econometric model due to J. Ferejohn, the publication year indicated being 1970. Upon closer scrutiny, it seems however that this model was first presented in a paper delivered at the December, 1970, Annual Meeting of the American Statistical Assocciation, but that it was not published prior to 1976, at least according to Anderton's bibliographical survey (1985). By the same token, the paper by R. Strauss came out in 1972, and not in 1971. Concerning the present writer's initial paper on the Middle Eastern arms race, the following "local" information may be worth being put on record. As said, this initial piece of applied research was published in 1971, but work on it started in 1965-1966. At that time, I was a Ph.D. student in economics at Harvard, and also a research assistant to Professor Nadav Safran who was and is with the Harvard Department of Government. My job with Nadav Safran was to help with the research on his forthcoming book about the Middle East (Safran 1969). Since this book was to include a detailed—and entirely novel, as far as I can determine—analysis of the arms race in the Middle East (see in particular, chapter IV, "The Dynamics of Arms Buildup: Defense Expenditures," and chapter V, "The Arms Buildup: Evolution of Armed Forces"), I came to realize, as our work progressed and especially as we painstakingly and painfully gathered a fairly large body of quantitative data on defense expenditures and GNPs in the Middle East, that Nadav Safran's views about this particular arms race were eminently susceptible of being expressed as a formal model using the econometric tools which I was then learning as a graduate student in economics. This I then proceeded to do, mostly while at the OECD in Paris in 1968-1969 and with the strong backing of Nadav Safran and others. It was only after the model had been estimated, analyzed and simulated that I became aware (via Rapoport's 1960 book) of the formal work that had been done previously on arms races, especially of Richardson's . (See the references listed in the article). It should also be added that this first piece of work proved very difficult to publish and that I collected a few rejection slips till it finally came out in *General Systems* thanks, I understand, to Professor Rapoport.—The data situation may be one reason why econometric analysis was so late in reaching "peace science." Before the mid-sixties or so, ie. before the International Institute of Strategic Studies (IISS) was founded in London and before its publications became sufficiently encompassing, there were no standard statistical sources on defense expenditures, armament stocks, military manpower, etc. Hence, anyone interested in these data had to cull them from a variety of sources, at a great expense of time and effort.

[6] To the extent that resources going to defense are at the expense of investment rather than consumption, it is arguable that the (trend) growth rate of output, on the one hand, and defense expenditures, on the other, are not independent and hence that there may be a problem of simultaneous equations. It was however felt, rightly or wrongly, that this link was probably weak and could be safely ignored; ie. that the trend values of GNP could be considered as "weakly exogenous" in such a model. (On the concept of weak exogeneity, see Judge *et al.* 1985, p. 669, and the references given there).

[7] Consider the following simple first-order non homogeneous difference equation:

$$Y_t = \alpha + \beta X_t + \gamma Y_{t-1} + \varepsilon_t$$

If traditional econometric methods are used to estimate this equation (methods which can range from the simplest—OLS—to the most sophisticated), estimation will always proceed on the basis of a sample including the *observed* values for the lagged endogenous variable on the RHS (ie. Y_{t-1}). As a result, the equation may do a really lousy job when simulated over the sample period, ie. when the equation itself is used to generate succesive values of the lagged endogenous variable (except the first one, of course). This, in essence, is why the estimation of *dynamic* models raises problems of its own.

[8] Meant are primarily maximum-likelihood methods.

[9] As mentioned, this paper is also seen as an opportunity for an exercise in "self-criticism" [*"pour faire son auto-critique"*, as one would say in French; incidentally, is it just my impression that this fine art is on the wane in Soviet Russia where it once was so popular ?]. So, concerning this joint 1979 article, one empirical conclusion that emerged from the application of continuous-time estimation techniques was that, in the East-West arms races, the United States was mostly *re*acting (*à la* Richardson) whereas the Soviet Union was mostly acting—meaning that "self-stimulation" seemed to prevail in the latter country while "mutual stimulation" predominated in the case of the U.S. Whence it follows that it is really the Soviet Union which is driving the arms race. Actually, it is possible that this (strong) conclusion mostly reflected the data used for real defense expenditures: Whereas the data for the U.S. are quite reliable, those for the Soviet Union are more or less rough estimates, possibly derived partly by interpolation and hence possibly smoother than the true (but unknown) series. If this is so, the "self-stimulation" parameter in the Soviet equation would be biased upward. A clue in this direction is that Allan (1983), using basically the same model and the same estimation techniques but a shorter sample, found that the model's estimated parameters were quite sensitive to the choice of the various existing data sets for Soviet real defense expenditures.

[10] Neither does the expected value of the stochastic process tend towards zero (or some constant value), so that the problem is not limited to the variance of the process.

[11] As a small point, Luterbacher also writes (p. 215): "As I mentioned before, economists deal mostly with negative feedback models. As a result, they may tend to overlook the problem of endogenously unstable systems." While it is true that there are negative feedback loops in microeconomics (eg. if the quantity demanded goes up, it will drive the price up, which will have a negative feedback on quantity via the supply side), there are also positive feedback mechanisms in microeconomics and above all in macroeconomics, where positive loops (such as the multiplier) would appear to dominate.

[12] Later on in his life, Richardson apparently came to change his mind—*vide* his 1951 note in *Nature* where he complicates his model somewhat so that "an arms race might end without any fighting," the newly introduced idea being that of possible "submissiveness" on the part of the actors, and the price being the addition of two extra parameters. As to the possibility of an arms race ending in "bankruptcy," the latter notion is too clear cut and drastic to be applied straightforwardly to countries as opposed to individuals. In general countries do not go bankrupt, but—if they are unwilling to cut civilian expenditures in proportion to the increase in defense outlays—they will experience more and more difficulty in securing credits from abroad, or they will have to agree to higher interest rates because of a larger risk premium, or they will be obliged to resort to currency controls, etc. All this generally makes up a rather more gradual process than the abrupt step of being declared bankrupt. Nevertheless the country's freedom of action will be increasingly curtailed as it spends more and more of its resources on defense, and at some point this constraint will undoubtedly start affecting its behavior. For example, it is certainly because their respective defense burden was becoming heavier and heavier that Egypt and Israel finally came to terms after the 1973 war.

[13] On an anecdotal level ("*pour la petite histoire*", as we say in French), this 1975 paper was hammered together in under one week (if memory serves) because an opportunity had arisen to have it published in—or to "plant" it on—the *Journal of Peace Research* which was bringing out a special issue on peace research in Switzerland. Since the people orbiting around this periodical seemed to me to share a set of *a priori*, politically tainted attitudes which definitely interfered with free scientific inquiry (in "peace research," the emphasis there was clearly on "peace" and not on "research"), the opportunity was too good to be wasted to try and drive home a number of points which seemed important to me, points which were investigated more thoroughly in later research and to which I shall return—such as "arms races as good things", in the sense that an arms race may very well *de*crease the odds that war will eventually break out.

[14] On the rational expectations revolution, which for all practical purposes broke out around the mid-seventies, see for example Begg (1982).

[15] It can however be argued that the *metagame* approach is akin to the rational expectations perspective. See for example Howard (1975).

[16] Interestingly, Tirptiz's bout of nervousness, which lead to a German short-term (partly myopic) reaction, can be shown to have been irrational or, to put it more mildy, pointless when considered *ex post*; see Lambelet (1975a), pp. 41-43.

[17] There is just one thing to be said under the heading of *ex post* self-criticism in the case of the Anglo-German naval "trilogy" (of which I will admit that I am modestly or not so modestly proud). In the first paper, which aims at sketching the historical backcloth for the formal analysis presented in the following two papers, mention is made of "the 1912 'SALT' agreement" between Great Britain and Germany. In fact, this expression could be misleading, in the sense that the 1912 Haldane mission to Berlin *was* an apparent failure and that no *formal* agreement was concluded between the two contenders. There however was a *tacit* understanding to slow down and stabilize the pace of the naval race. (A contributing factor on the German side was that the competing claims from the land forces were becoming ever stronger). The data are entirely compatible with the notion that such a tacit understanding came into being in 1912, as are both Churchill's and Tirpitz's own accounts. But, since the expression used ("SALT agreement") was undoubtedly too strong, this led to an exchange of letters with Volker R. Berghahn, the German historian. Without denying the weight of the evidence I had gathered, Dr. Berghahn tended to object to the approach I had taken; and he particularly objected to the conclusion that the strategy followed by Tirpitz regarding naval programs was not necessarily stupid and ultimately self-defeating. I suspect that the real source of trouble was that in the course of researching and writing these papers, I had become rather less unsympathetic to the German standpoint than I was when I started, and that it showed in the papers.

[18] Ie. the constants in his differential equations which can be either positive (an indication of basic hostility) or negative (an indication of readiness to cooperate).

[19] For the (seemingly) first formal attempt in that direction, see Lambelet 1973. However as rightly pointed out by Luterbacher (1975) and as acknowledged in the paper itself, the empirical (econometric results presented there are rather weak on several counts ("unclean" residuals, low significance of many estimated parameters). The SIMPEST model built by Luterbacher and his team (see e.g. 1979) includes a significantly more sophisticated level of disaggregation along similar lines.

[20] Foremost among these scientists is Professor Walter Isard who has untiredly done his utmost over several decades to promote the analysis of arms races ("peace science," defense studies or whatever one may choose to call it) as a serious field of study. Let this be an opportunity for me to state how much I owe to the unfailing support and interest of Walter Isard since we first met back in 1969 (if memory serves).

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