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# Alliance Behavior in 19th-Century Europe

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is too interesting and too important for our understanding of policy consequences from realignments to leave the matter unresolved.

PAUL ALLEN BECK

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TO THE EDITOR:

I think Professor Beck's criticisms very well taken. Indeed, they reflect certain caveats which I myself noted in my article. While his reservations about the analysis are appropriate, however, the vigor with which he presses them suggests that he has, in part, missed the point of the essay.

After three paragraphs which reasonably accurately summarize "The Supreme Court and Critical Elections," Professor Beck's central point (italicized, lest the unwary reader miss it) is made at the beginning of his fourth paragraph. I agree with that point. But I fail to see how it materially adds anything to what I wrote (p. 807):

... The use here of some years before the elections which brought new coalitions to power as part of the realignment phases may very probably have artificially deflated the indexes for the critical periods. . . . Perhaps future research might attempt to determine the year of the ascendancy of the new coalition. . . . Such attempts to determine precise years of ascendancy, however, not only must contend with the scholarly contentiousness about which elections actually involved realignments but also will be liable to the charge that they have defined the critical periods so as to inflate the statistical results. . . . If the results reported here can be achieved by using realignment phases not inherently favorable to the thesis, they may perhaps be considered to be even more persuasive.

As for Beck's remarks concerning the New Deal period, I would simply refer him to my footnote 67.

"The Supreme Court and Critical Elections" was intended—and, I think, can and should so be read—as a synthetic rather than an original analytic effort. It was an attempt, as I stated in several places, to both integrate and test the assertions of certain leaders in our profession—most notably Dahl, but also Burnham—*on their own terms*. This effort at replication was dictated by my belief that we social scientists too little attempt to build upon one another's work as do our brethren in the natural sciences.

I could not more agree with Professor Beck that such an integrative statement as is represented by my article is but the beginning not the end of analysis, and I very much concur in his judgment that such an analysis must "examine the background of each case individually." I said as much in the article (p. 808):

Ultimately, the subject calls for an extended, systematic, historical examination of the Court's decision making explicitly based upon the electoral classification

scheme devised by the S.R.C. and elaborated by others, a treatment of the Court similar to that which Chambers and Burnham have done for the parties. . . . Any study such as this, based upon aggregate data, necessarily sacrifices situational focus for numerological standardization, but Supreme Court decisions are not fungible goods.

Indeed, my conception of what is required is apparently much more ambitious than Beck's; I think we need nothing less than a definitive intellectual history of the Court; something we do not now have. (See also my note 84.) I am encouraged that someone with Professor Beck's analytic skills also thinks the pursuit important.

RICHARD FUNSTON

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### Alliance Behavior in Nineteenth-Century Europe

TO THE EDITOR:

In their recent analysis of nineteenth-century European alliance activity, McGowan and Rood allude to two separate dimensions of alliance aggregation—spatial and temporal. "If these [Kaplan's] rules are violated, either by a rigid enmity, such as existed between France and Germany after the annexation of Alsace Lorraine [spatial], or by a decline in the system alliance formation rate [temporal], then a loss of system flexibility will result and system-changing events are likely."<sup>1</sup>

Despite their repeated assertions that independence and flexibility in the matter of "whom the alliance partners are"<sup>2</sup> are vital to the maintenance of a balance of power system and their quoting Kaplan in the same vein ("the 'balance of power' system postulates that any alignment is as probable as any other alignment prior to a consideration of the specific interests which divide nations. Moreover, any particular alignment should not predispose the same nations to align themselves with each other at the next opportunity"),<sup>3</sup> the authors go on to ignore the spatial dimension of alliance aggregation in their data analysis. They test, instead, three hypotheses which concern the times at which alliances in nineteenth-century Europe were formed. These are

- H1: in a balance of power international system, the occurrence of alliances will be stochastically distributed (the number of alliances formed per unit of time is a Poisson random variable), and
- H2: in a balance of power international system, the time intervals between alliances are randomly distributed (the distribution of interalliance intervals is a negative exponential random variable).<sup>4</sup>

<sup>1</sup> Patrick J. McGowan and Robert M. Rood, "Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe," *American Political Science Review*, 69 (September 1975), 862.

<sup>2</sup> *Ibid.*, p. 861.

<sup>3</sup> *Ibid.*

<sup>4</sup> *Ibid.*

and

H3: in a balance of power international system, a decline in the systemic rate of alliance formation precedes system changing events, such as general war.<sup>5</sup>

Past scholarship, however, hardly agrees with McGowan and Rood that the time at which alliances are formed is the most important aspect of their relation to the operation of balance of power systems. Edward Gulick's account of European alliances is generally accepted as a classic account of the operation of a balance of power system (Robert Jervis's observation that Gulick's subject matter is more properly to be considered a "concert system" notwithstanding). In citing an example of the requisite (especially for the "holder of the balance") "mobility of action" for the maintenance of a balance of power system,<sup>6</sup> he emphasizes the identities of allies and opponents at least as much as, if not indeed more than, the times at which the alliances were concluded. Richard Rosecrance argues that

the balancing system of Europe required states to ally or oppose each other according to the presumed distribution of power: if ideological bonds or animosities had arisen, states could no longer have charted their courses on power considerations alone; states would have refused to balance against their ideological *confreeres* or to align themselves with ideological opponents, regardless of the configuration of power.<sup>7</sup>

Dina Zinnes assumes the importance of spatial flexibility when she constructs her analytic model of a "hypothetical world" of possible alliance combinations in such a way that "the labels of the nations are unimportant. The relevant factors are the number of alliances of various sizes and the placing and number of power equalities and inequalities."<sup>8</sup> Nicholas Spykman offers simply that "he who plays the balance of power can have no permanent friends. His devotion can be to no specific state but only to balanced power. The ally of today is the enemy of tomorrow. One of the charms of power politics is that it offers no op-

portunity to grow weary of one's friends."<sup>9</sup>

It seems likely, then, that if threats to a very polarized (spatially nonrandom) system occur randomly (or according to a Poisson distribution), causing alliance formation within the system, most students of the balance of power "theory" would say that the system itself is neither fluid nor flexible (i.e., it would be a rigid system responding to random stimuli). On the other hand, a system of spatially random alliances would usually be considered fluid and flexible whether it was responding to random or nonrandom (temporally) stimuli. Therefore, the more important of the two dimensions of alliance aggregation would seem to be the spatial one.

Although McGowan and Rood have given us an excellent analysis of the temporal randomness of nineteenth-century alliances in Europe, it would be far more interesting and important to know whether these same temporally Poisson-distributed alliances exhibit spatial randomness as well. If they do, then McGowan and Rood's analysis (not to mention balance of power "theory" itself) would gain much more credibility. If, however, spatial alignment during the period is shown to be nonrandom—and more especially if the resulting pattern of alliance partners is indistinguishable from that of the 1910–1914 period's rigidity (which has been said to have been the cause of the disintegration of the European balance of power system and its transformation to a more-or-less bipolar one), then not only might McGowan and Rood's analysis lose much of its credibility and theoretical usefulness but also the usefulness of structural theories for the study of international politics in general might be brought into question.<sup>10</sup>

McGowan and Rood's data consist of 54 alliances among European states during the period 1814–1909. Although other states occasionally are included in these alliances, most of the activity concerns only five states—Austria, France, Great Britain, Prussia, and Russia. Their alignments during the period can be summarized according to partners as follows:

State	Prussia	Russia	Aligned with		
			France	Great Britain	other
Austria	20	12	3	13	12
Prussia		11	1	6	9
Russia			7	9	3
France				9	6
Great Britain					10

<sup>5</sup> McGowan and Rood, p. 862.

<sup>6</sup> Edward Vose Gulick, *Europe's Classical Balance of Power* (New York: Norton, 1967), p. 68.

<sup>7</sup> Richard N. Rosecrance, *Action and Reaction in World Politics* (Boston: Little, Brown, 1963), p. 28.

<sup>8</sup> Dina A. Zinnes, "An Analytical Study of the Balance of Power Theories," *Journal of Peace Research* 4 (1967), 273.

<sup>9</sup> Nicholas Spykman, *America's Strategy in World Politics* (New York: Harcourt, Brace, 1942), p. 103.

<sup>10</sup> Arthur L. Stinchcombe's ideas concerning theory testing should be apparent here, if only in some twisted form. See especially his *Constructing Social Theories* (New York: Harcourt, Brace and World, 1968).

From this table, the frequencies of bilateral relations within the system can be seen. These can then be listed according to frequency (starting with the most frequently-found partnership):

Austria-Prussia	20
Austria-Great Britain	13
Austria-Russia	12
Prussia-Russia	11
Russia-Great Britain	9
France-Great Britain	9
Russia-France	7
Prussia-Great Britain	6
Austria-France	3
Prussia-France	1

Since there are ten possible pairs of states in the data set, the probability that any one pair will occur, given that any one pair is just as likely as any other, is 1/10. Ninety-one bilateral ties occur in the matrix above. Therefore, an hypothesis of spatial randomness among the states under consideration would predict that each cell frequency would be 9.1. It is immediately apparent from both the matrix and the list presented above that this is not the case. The alliances which formed the McGowan-Rood data set are not spatially random.

One finds instead that Austria is allied with Prussia more than twice as often as random distribution would predict. On the other hand, a Prussia-France alliance almost never occurs at all and an Austria-France one is very little more frequent. Only the Great Britain-Russia and France-Great Britain ties occur as frequently as balance of power "theory" would expect.

If Austria-Prussia alliances predominated during the 1814-1909 period in Europe while Great Britain-Russia and France-Great Britain alliances occurred at least no less frequently than they would by chance, the modal alliance pattern for the system as a whole would seem very little different from the pattern which obtained in 1914.<sup>11</sup> Balance of power "theory" (and all structural theories of international politics) then faces the

<sup>11</sup> If the grand coalitions which were formed just after the Congress of Vienna are dropped from the McGowan and Rood data set, then the general bipolarity of the European state system during the remainder of the nineteenth century becomes considerably more evident. With an hypothesis of randomness expecting identical frequencies of 5.3 for each bilateral combination, the following frequencies are actually observed:

Austria-Prussia	14
France-Great Britain	7
Austria-Russia	7
Austria-Great Britain	7
Russia-France	6
Prussia-Russia	6
Russia-Great Britain	4
Prussia-Great Britain	1
Austria-France	1
Prussia-France	0

question of why pre-1910 European alliance rigidity did not cause international system transformation whereas post-1910 rigidity did. The only satisfactory answer to this question may have to be found at the individual-nation-as-actor level of analysis.<sup>12</sup> Although this is another matter deserving further empirical research, it should be noted that Kaplan himself considered balance of power systems to be "subsystem dominant,"<sup>13</sup> in other words, not amenable to systems (structural) theories and explanations at all: but rather dependent upon the actions and orientations of individual states.

Finally, an even more fundamental criticism can be made of both the McGowan and Rood analysis and this very brief one. Both analyses are in some fundamental sense after the fact of alliance, concerning themselves only with the point at which the "specific interests" of states have already shown themselves and the states in the system have taken up sides according to those interests. Kaplan's classic statement bears repeating: "the 'balance of power' system postulates that any alignment is as probable as any other alignment prior to a consideration of the specific interests which divide nations (emphasis added)." By the time alliances form, it is too late to determine this probability. Any study which concerns itself solely with alliances which have already come into being cannot adequately address itself to the task of testing balance of power "theory."

FRED H. LAWSON

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<sup>12</sup> For a very precise delimitation of the two levels of international political analysis, see William B. Moul, "The Level of Analysis Problem Revisited," *Canadian Journal of Political Science* 6 (September 1973), 494-513.

<sup>13</sup> Morton A. Kaplan, *System and Process in International Politics* (New York: Wiley, 1957), pp. 27, 125.

#### TO THE EDITOR:

As we understand Mr. Lawson, he makes six points in his criticism of our recent study of balance of power alliance behavior in nineteenth-century Europe:

(1) the choice of alliance partner (and, by implication, opponent) is an important aspect of balance of power theory;

(2) we ignored this "spatial" dimension of alliance aggregation in our exclusively "temporal" study;

(3) if alliance pairings among the five great European powers in the nineteenth century are approximately random, then our analysis and balance of power theory "would gain much more credibility," but if "spatial alignment . . . is . . . nonrandom," then our study and other "structural theories" of international politics "might be brought into question";

(4) according to his presentation of our data,<sup>1</sup> alliances "are not spatially random";

(5) moreover, *ex post facto* research, such as our original study and his comment, cannot "test" balance of power theory;

(6) therefore, our study and balance of power theories are of dubious value.

We appreciate Mr. Lawson's interest in our paper and we welcome this opportunity to respond to his interpretation of our work and thereby to extend research in this important area of international relations theory.

Before we discuss each issue Mr. Lawson has raised, two general comments are in order. First, although he cites Moul's fine paper,<sup>2</sup> we do not think he has fully understood its relevance to his critique. The units of analysis and the treatment of time are completely different in our article and in his comment. We presented a longitudinal study of one international system, whereas Lawson works with a single aggregated cross-section of time (96 years!) and ten pairs of states. In Moul's language, for Lawson to claim that his critique says *anything* about the findings of our article is a clear instance of "confounding the ecological fallacy with a cross-sectional—longitudinal fallacy."<sup>3</sup> However, Lawson's presentation of our data is of interest in its own right, a point we shall consider. Second, Lawson has not reanalyzed our data, he has simply presented *without citation* and with several counting errors<sup>4</sup> information reported four years ago in Table 7 and Appendix A of Rood's dissertation.<sup>5</sup>

(1) Without question, *at the levels of states and pairs of states*, patterns of choice of alliance partner are a fundamental aspect of balance of power theory. However, to examine this issue in an entirely static, cross-sectional fashion as Lawson does obscures the dynamic, sequential nature of alignments, making it difficult to discuss in an intelligent manner alliance flexibility and also making more probable a spatially skewed distribution of the behavior between pairs of states, or any other interacting units for that matter.<sup>6</sup>

(2) It is obvious that we did not examine the *dyadic level* problem of alliance choice in our exclusively *system level* paper. It is also obvious that Mr. Lawson has not read carefully or has not understood fully our article and Rood's dissertation from which he obtained our data. To repeat what we said in our article,<sup>7</sup> Rood's study was an explicit test of the *dyadic level* proposition that "any alignment is as probable as any other alignment prior to a consideration of the specific interests which divide nations."<sup>8</sup> Rood was unable to falsify this proposition when a probabilistic model of agreement among pairs of actors developed by Brams and O'Leary was applied to the data.<sup>9</sup> Moreover, subsequent research has shown that deviations from this model may be explained statistically by the magnitude of war within each pair of states,<sup>10</sup> a violation of its assumptions that balance-of-power theory predictions should affect choice of alliance partners *and* opponents.<sup>11</sup>

(3) We strongly agree that if it can be demonstrated that alliances occur on both a spatially and temporarily random basis, then our study and balance-of-power theory will have their credibility enhanced. The problem with Lawson's critique is that skewed cross-sectional data *are not necessarily evidence of nonrandomness in statistical models*, particularly (a) when one works with a simplistic probability model, equal probability, (b) when the *N* is small, only ten dyads, and (c) when the author merely "eyeballs" the data rather than applying the appropriate statistical goodness of fit test. Lawson, in his "analysis" of our data, makes the same mistake Londoners did during World War II when they were sure that German rocket bombs fell in clusters in certain neighborhoods. A Poisson-based analysis of the spatial distribution of south London bomb hits indicated perfect randomness.<sup>12</sup> We would advise Mr. Lawson to heed Feller's conclusion regarding "the established fact that to the untrained eye randomness appears as

<sup>7</sup> Patrick J. McGowan and Robert M. Rood, "Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe," *American Political Science Review* 69 (September, 1975), 863.

<sup>8</sup> Morton A. Kaplan, *System and Process in International Politics* (New York: Wiley Science Editions, 1964), Preface.

<sup>9</sup> Rood, "Agreement in the International System," pp. 159–163; Steven J. Brams and Michael K. O'Leary, "An Axiomatic Model of Voting Bodies," *American Political Science Review* 64 (June, 1970), 449–470.

<sup>10</sup> Robert M. Rood, "The Dyadic Distribution of Alliance Commitments," Columbia: Department of Government and International Studies, University of South Carolina, mimeo, 1975.

<sup>11</sup> That is, the patterned relationships of international conflict (magnitude of war within dyads) certainly are "specific interests which divide nations."

<sup>12</sup> William Feller, *An Introduction to Probability Theory and Its Applications*, 3rd ed. (New York: Wiley, 1968), 160–161.

<sup>1</sup> We made these data available to Mr. Lawson at his request, a fact he fails to acknowledge.

<sup>2</sup> William B. Moul, "The Level of Analysis Problem Revisited," *Canadian Journal of Political Science* 6 (September, 1973), 494–513.

<sup>3</sup> *Ibid.*, pp. 501–502.

<sup>4</sup> The errors are: (1) our alliances number 55, not 54; (2) the Austria-Prussia pair has 21, not 20 alliances, and (3) the Austria-Great Britain pair has 12, not 13. These correct figures are used in the new analysis we report in this communication.

<sup>5</sup> Robert M. Rood, "Agreement in the International System" (Ph.D. dissertation in Political Science, Syracuse University, 1973), 88, 183–201.

<sup>6</sup> Edwin Kuh, "The Validity of Cross-Sectionally Estimated Behavior Equations in Time Series Applications," *Econometrica* 27 (1959), 197–214.

regularity or tendency to cluster."<sup>13</sup>

(4) In point of fact, alliance choice *is* randomly distributed in the five power nineteenth-century European balance of power system. Table 1 shows this when the spatial *t*-axis is defined in terms of pairs of states and grouped in units of four alliances. We have also calculated goodness-of-fit tests for cutting points of two and three alliance units with similar results. Substantively, contrary to Lawson's reading of our data, we can report that the hypothesis, *in a balance-of-power system, pair-wise choice of alliance partner will be stochastically distributed* (the number of alliances formed per dyad is a Poisson random variable), cannot be rejected and is independent of how the number of alliances is categorized. Methodologically, Table 1 is cross-sectional (spatial) with all the attendant

**Table 1. The Pair-wise Choice of Alliance Partners in Europe, 1814-1914, as a Poisson Distribution**

Number of alliances per dyad	<i>p</i> <sup>a</sup>	Number Observed	Number Expected
0-3	.0198	2	.2
4-7	.2924	2	2.9
8-11	.4709	3	4.7
≥ 12	.2068	3	2.1
		10	9.9

$\chi^2 = 4.48$ , d.f. = 3,  $p = .21$

Kolmogorov-Smirnov test<sup>b</sup>,  $p > .20$

<sup>a</sup> Calculated from T. Kitagawa, *Tables of the Poisson Distribution* (Tokyo: Baifukan, 1952).

<sup>b</sup> Calculated from Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences* (New York: McGraw-Hill, 1956), pp. 47-51.

<sup>13</sup> Feller, *Probability Theory*, p. 161.

problems we have discussed regarding such analyses, and the *N* is very small. Nevertheless, we appreciate Mr. Lawson's stimulus and we hope that this original demonstration of the spatial randomness of alliances leads him to find more credible our original study as well as balance of power theory and structural theories of international politics in general.

(5) It is difficult to determine what Mr. Lawson means by his concluding criticism that balance of power theory cannot be "tested" by nonexperimental methods including historical research and quantitative international political analysis. If he is correct, then the "theory" is no more empirical than the statement "everything which happens is God's will." We think Professor Kaplan intends to say more than such truisms and that in making statements about possible alliance partners and opponents, one must include the specific interests which divide nations as part of the statement of initial conditions from which the explanation (postdiction) or prediction is deduced. Could Kaplan mean anything else by his "classic statement" and still claim to advocate a "scientific" approach to the study of international relations as he so often does?<sup>14</sup>

(6) For all of these reasons, including his changes in the level of analysis and temporal organization, we cannot accept Mr. Lawson's criticisms. We do, however, thank him for his interest in our work.

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<sup>14</sup> Morton A. Kaplan, "The New Great Debate: Traditionalism vs. Science in International Relations," in K. Knorr and J. N. Rosenau, eds., *Contending Approaches to International Politics* (Princeton: Princeton University Press, 1969), pp. 38-61.