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U.S. Foreign Aid and U.N. Voting: Did Reagan's Linkage Strategy Buy Deference or Defiance?

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The Reagan administration's 1986 policy initiative linking the allocation of U.S. foreign aid to recipient voting behavior in the U.N. General Assembly is evaluated. Aid levels and voting patterns are examined prior to and after the implementation of the declared bargaining policy. To maximize validity, the data are subjected to a variety of statistical treatments, including construction of a cross-lagged path model. None of the results emerging from these treatments produced statistical evidence that a relationship was present. The data demonstrate that the strategy did not produce the effects envisioned by its framers: The policy fell short of its goal of eliciting compliance behavior through threats of economic sanctions. Contending reasons for the strategy's failure are advanced in a concluding interpretation.

In an age when "economic statecraft" (Baldwin, 1985) has received increasing emphasis as a foreign policy instrument for the pursuit of global influence, foreign assistance has acquired renewed salience for U.S. foreign policy.[1] The Bush administration, seeking to reverse recent declines in aid allotments, pressured Congress in 1990 for major increases to support new initiatives in Central America and Eastern Europe.[2] Although constrained by record budget deficits, in the post–Cold War era the administration has revived foreign aid as a potent tool for the promotion of American interests worldwide.

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[1] Postwar allocations have exceeded $350 billion (U.S. Agency for International Development, 1989:4), and in Fiscal Year 1989 the U.S. transferred nearly $15 billion in loans and grants to 117 countries, or to about 70 percent of the system's sovereign states (U.S. House of Representatives, 1989:3). For a description of U.S. leaders' use of aid leverage as both inducement and threat, see Laufer (1989).

[2] The two major categories under which U.S. aid is distributed include the Economic Support Fund and the Military Assistance Program. Our assessment of the distribution of American foreign assistance will include both economic and military assistance, distributed through these and other programs.

But the purposes and efficacy of American foreign assistance programs remain uncertain. As Hans J. Morgenthau (cited in Liska, 1960:vii) observed long ago, "Of the seeming and real innovations which the modern age has introduced into the practice of foreign policy, none has proved more baffling to both understanding and action than foreign aid. . . . Nothing even approaching a coherent philosophy of foreign aid has been developed." His view remains relevant today and is reflected in widespread public and elite skepticism about America's foreign aid program.[3]

In this context, the Reagan administration's posture toward U.S. foreign assistance offers valuable lessons about its uses and limitations. Reagan ordered a reassessment of U.S. involvement in multilateral institutions (Hughes, 1985-1986) and close monitoring of recipient states' foreign policy behavior. This effort was provoked by the erosion of support for American diplomacy in the United Nations, where defiant Third World states—many receiving large amounts of U.S. foreign aid—routinely voted contrary to U.S. positions (see Jacobson, 1984).[4] This deeply troubled the administration and Congress, especially in light of proportionate increases in most members' voting positions with those of the Soviet Union.[5] Backed by a compliant Congress, the Reagan administration vowed to hold these states accountable for their allegiances in a revived Cold War.[6]

Jeane Kirkpatrick, Reagan's first U.N. ambassador, evinced a "greater interest in questions of anticommunism than in issues of human rights, and tended to regard the United Nations principally as an instrument of U.S. foreign policy" (Fasulo, 1984:278). As Kirkpatrick testified before a Senate subcommittee in 1983,

> We must communicate that it is not possible to denounce us on Monday, vote against us on important issues of principle on Tuesday and Wednesday, and pick up our assurances of support on Thursday and Friday. . . . Voting behavior . . . should be one of the criteria we employ in deciding whether we will provide assistance. (*The Washington Post,* 1983:A14)

In May 1986, Kirkpatrick's successor, Vernon Walters, recommended that U.S.

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[3] This skepticism is captured by recent public opinion polls which demonstrate that more than half of the American public and more than 90 percent of American leaders favor foreign aid programs, but that nearly 90 percent also believe that "a large part of American aid is wasted" (U.S. Agency for International Development, 1989; see also Wittkopf, 1990).

[4] Within the U.S. General Assembly, the voting coincidence rate of all members with U.S. positions plummeted to less than 20 percent during Reagan's first term (U.S. State Department, 1986). In comparison, from their peak of about 70 percent in the mid-1950s, coincidence rates averaged about 50 percent in the mid-1960s and 30 percent in the mid-1970s (Riggs and Plano, 1988:87).

[5] These voting trends were influenced by the full range of developments within the U.N. General Assembly that transpired during this period, including an increase in member states, the shifting agenda, and other factors. We do not suggest that such percentages exclusively reflected, per se, states' "agreement" with U.S. positions; many votes were cast without regard to the U.S. position, and without attention to the competition between the United States and the Soviet Union during the Cold War. Therefore, we use the more appropriate term "voting coincidence" throughout this study. Measurement of states' foreign policy orientations and alignments through analysis of U.N. roll call voting has been legitimately criticized, because how states vote has demonstrably been influenced by pressures that have little to do with their true political allegiances. But this limitation arguably does not apply here, given the Reagan administration's definition of that voting as indicative of political allegiance (of voting coincidence as voting "agreement"). To some degree, however, such behavior can be seen as a salient indicator of foreign policy positions. Through U.N. voting, states take a public stance on important and often divisive global issues. As Bruce E. Moon (1987:41–42) argues, "The positions taken on individual issues are highly patterned and generally sum to a visible, coherent and stable outlook on the interests, values, and perspectives that dominate. . . . For most nations the predominant goal of position taking is not resolution of the issue itself but rather expression of the stance of the nation. These positions communicate alignment to external actors which can be of great significance in a variety of ways."

[6] Largely as a result of Congressional pressure, in 1984 the U.S. State Department began publishing annual reports on U.N. voting patterns as they relate to U.S. positions in the General Assembly. These lengthy reports include extensive data on which much of our subsequent policy evaluation is based.

foreign aid be explicitly linked to the recipients' U.N. voting (*The New York Times,* 1986). Senator Robert Kasten, Jr. (R-Wis) agreed, complaining that "Many countries to whom we dispense aid continue to thumb their noses at us" in the United Nations (Kasten, 1986:A31). Shortly thereafter, through Public Laws #98-164 and #99-190, Congress authorized the president to restrict aid to states found to be routinely in opposition to U.S. positions in the United Nations. What began as a series of complaints and threats culminated in a formally announced strategy.

In the following study, we pursue three interrelated goals: to employ and demonstrate the utility of *policy evaluation* techniques (see Raymond, 1987) as they apply to this case study; to evaluate the linkage strategy using this approach; and to draw inferences regarding the U.S. foreign aid program, U.S. relations within the U.N., and the credibility of economic threats. We will consider voting and aid patterns before and after implementation of the linkage initiative and advance various interpretations of the results. Throughout, we will explore the central questions of whether the strategy bought deference or defiance, and more generally whether in the paradoxical realm of foreign aid politics it is advisable to bite the hand one feeds.

Policy Evaluation as a Research Challenge

Within the comparative study of foreign policy, the systematic evaluation of policy initiatives is a central research need (see Hermann, Kegley, and Rosenau, 1987). This challenge has not been adequately confronted because of the difficulties in identifying a policy initiative's true purposes and the criteria by which its efficacy can be impartially measured.

But in the case of this linkage strategy, these obstacles exist to a much lesser degree. Because the Reagan administration and the U.S. Congress explicitly identified a problem (low levels of coincidence with U.S. positions in U.N. General Assembly votes), proposed a tactical solution (linking subsequent U.S. aid to these votes), and specified an ultimate policy goal (increased support for U.S. preferences), the policy's purposes are explicit and its efficacy is amenable to objective evaluation. Although many scholars question whether states' behavior in international fora is directly associated with their foreign aid relationships, this was the explicit underlying assumption of the linkage strategy. As such, it warrants systematic evaluation.

To examine the effectiveness of a strategy designed to increase the voting coincidence of major U.S. foreign aid recipients, we must observe changes in aid and voting patterns after the policy initiative was announced, in the context of the temporal setting which immediately preceded the "intervention" (see Cook and Campbell, 1979). For the policy to prove effective, aid recipients should have acted more deferentially to their U.S. donor in the years following the initiative than in preceding years, and this change should be statistically significant so that we can safely conclude the observed change was not attributable to chance.

In comprehensively assessing the history of the policy initiative, we shall first report its consequences as described by the U.S. government's reporting procedures.[7] This will be followed by a series of statistical treatments of the same data, to test the conclusions suggested by the government's own account and to illuminate more

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[7] Our measurements of voting coincidence follow the precedent (see Richardson, 1981:100–101) established for this purpose by Brams and O'Leary (1970), Wittkopf (1973; 1975), and Rowe (1969). Specifically, our index measures the percentage score of (1) identical voting positions between each pair of countries divided by (2) the total number of roll call votes on which the two voted. Note that this index conforms to that used to calculate "voting agreement" by the U.S. government itself.

precisely the policy's consequences. The statistical inquiry will advance in stages of increasing precision, addressing and resolving valid threats to inference as they arise.
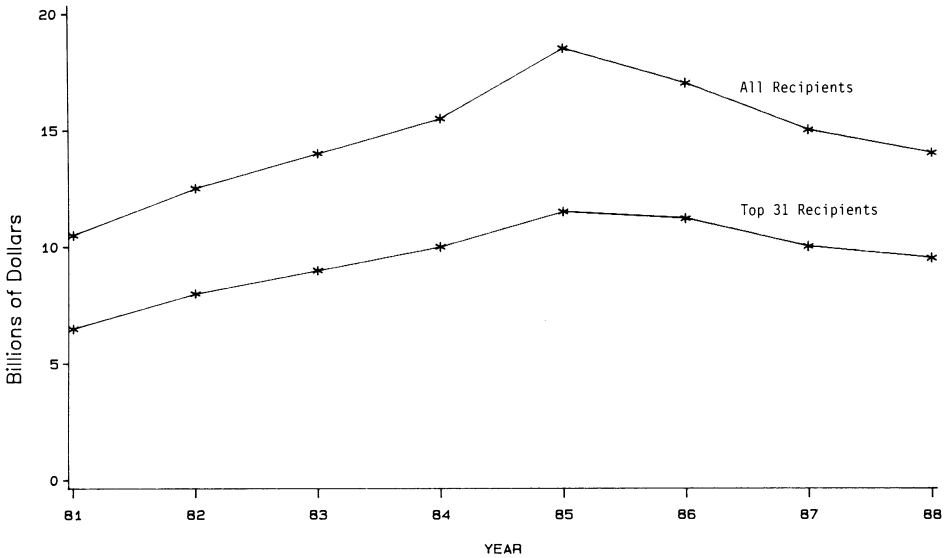

FIG. 1. U.S. foreign assistance, 1981–1988 (current dollars). Figures include economic and military assistance (loans and grants). Source: U.S. Agency for International Development (1982–1989).

The Aid/U.N. Support Linkage

A significant feature of the Reagan administration's pursuit of its declared policy was that, at the very time the U.S. government attempted to marshall political support through threats to restrict foreign aid, it began to reduce the aggregate level of its annual allocations. As Figure 1 displays, absolute levels of U.S. assistance had ascended to a high of $18.1 billion in FY1985 (in current 1989 dollars) before falling nearly 27 percent to $13.6 billion (estimated) by FY1989 (U.S. Agency for International Development, 1989:4).

This reduction did not apply equally to all of the top thirty-one foreign aid recipients.[8] Twenty-three of these states received increased American aid between FY1984 and FY1985, but the disbursements fell in eighteen cases the following year, in twenty between FY1986 and FY1987, and in twenty-one the following year. It is clear that a wide range of recipients suffered from reductions in the level of American assistance they received after the linkage strategy was implemented. What, then, was the corresponding pattern at the U.N.?

As aid fell, voting coincidence rates, as monitored by the U.S. government, also

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[8] The sample consists, in order of the level of FY1988 aid allocations, of Israel, Egypt, Pakistan, Turkey, El Salvador, Greece, the Philippines, Honduras, Guatemala, Morocco, India, Costa Rica, Portugal, Bangladesh, Bolivia, Jamaica, Thailand, Peru, Indonesia, Tunisia, Sudan, Kenya, Zaire, Dominican Republic, Sri Lanka, Jordan, Ethiopia, Malawi, Mozambique, Haiti, and Niger. Given the existence of a trimodal distribution of U.S. aid allocations, it was appropriate to concentrate on those collectively receiving the largest proportion of U.S. aid.
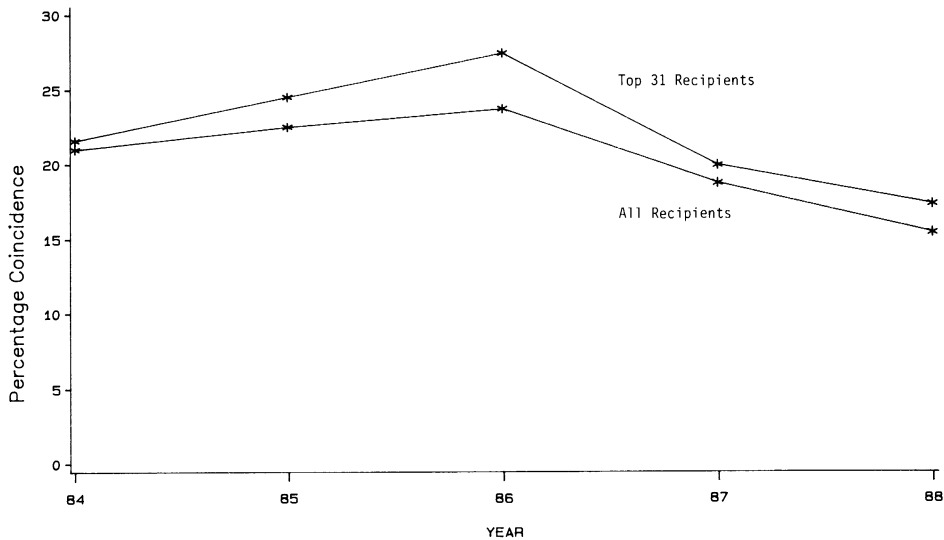
FIG. 2. U.N. General Assembly voting coincidence with U.S. positions, 1984–1988.
Source: *Annual Reports in Congress on Voting Practices in the United Nations*, U.S. Depart-
ment of State (1985–1989).

began to decline.[9] In 1984, the first year for which the mandated "voting-coincidence"
rate was calculated, the positions of other states matched that of the U.S. in 21% of
the cases. The level rose to 22.5% in 1985 and to 23.7% in 1986, but fell to 18.7% in
1987 and to 15.4% in 1988 (see Figure 2).

These voting cleavages broadly mirrored the state of U.S. foreign relations with
respect to geographic regions (see Table 1). Although the coincidence levels varied
widely across regions, modest changes within the regions were evident. In the two
years preceding the U.S. government's legislation linking foreign aid and U.N. voting,
the coincidence rates rose in all five regions of the world. The sharpest rise occurred
among African states, whose coincidence rate with the United States increased 32
percent from 1984 to 1986. Comparatively, the rates jumped 27 percent in Asia and
the Pacific, 19 percent in the Americas, 18 percent in Western Europe and 3 percent
in Eastern Europe. But this improvement proved temporary. Coincidence rates
dropped dramatically between 1986 and 1987—the very year that followed the formal
pronouncement of the U.S. initiative. Precipitous declines were registered in the
Americas (34 percent), Africa (21 percent), Asia (19 percent), Eastern Europe (18
percent), and Western Europe (15 percent).

As we have seen, this drop in the coincidence rates also applied to the top thirty-
one U.S. aid recipients (see Figure 2). For the largest recipients, the coincidence rates
climbed in twenty-four cases from 1984 to 1985, and in twenty cases the following
year. But the pattern was reversed between 1986 and 1987, when the coincidence

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[9] These coincidence rates represent the percentage of *all* General Assembly votes in which the member states
took the same position as the United States. They do not include issues which were resolved by *consensus*, which are
the least divisive and which often fail to reflect the foreign policy preferences of all states or cleavages in their
alignments. But in contrast, issues brought to a vote do reveal states' positions. For these reasons, the *aggregate*
pattern of voting behavior best captures the level of coincidence.

TABLE 1. Regional distribution of General Assembly voting coincidence with the United States, 1984–1988.

	Avg. %	High %	Low %
Western Europe	58.3	65.6	55.4
The Americas	21.4	27.5	14.4
Asia & Pacific	15.7	18.9	12.5
Africa	13.5	16.9	15.1
Eastern Europe	11.1	12.5	8.1

Source: U.S. Department of State (1985–1989).

rates fell in twenty-nine cases, and between 1987 and 1988, when the rates again fell in thirty cases (rising only in the case of Israel).

These distributions and the basic direction of change demonstrate that the imbalance that the administration pledged to correct failed to be rectified. If the thirty-one largest U.S. aid recipients are rank-ordered in terms of both their coincidence level and their level of foreign assistance in the first budgetary cycle in which adjustments were possible, we fail to observe a substantial strengthening of the linkage.

Calculating changes over time in the annual Spearman's *rho* rank-order correlation between voting coincidence and aid allocations provides a quantitative estimate of this.[10] In 1984 and 1985, the correlation stood at *rho* = 0.32 and *rho* = 0.31, respectively; in 1986 it rose to *rho* = 0.36 and again in 1987 to *rho* = 0.41, but in 1988 it receded to *rho* = 0.40. This indicates a modest but uneven increase in the relationship after formal announcement of the linkage strategy in 1986. This perturbation in the overall pattern suggests that the relationship between the countries that ranked high in their support of American foreign policy and the countries that ranked high in U.S. assistance remained largely consistent and did not grow substantially stronger after the strategy was legally mandated.

This account, however, captures only a portion of the picture. The data can be probed further to describe the relationship more precisely. As the next step in that direction, the annual correlations between the level of aid and the degree of recipient coincidence with the U.S. positions can be calculated. Pearson correlation coefficients, based on interval measurement of the variables, measure these associations.

Perusal of the annual range and fluctuation of the bivariate correlation coefficients over time suggests the absence of a meaningful change in the bivariate relationship during the period of observation. Despite the formal policy declaration in 1986 that it was the intention of the United States to increase the correspondence between assistance and compliance, that association for the top thirty-one recipients actually *decreased* in 1986 ($r = 0.51$) and 1987 ($r = 0.54$) below the level ($r = 0.57$) observed in 1985. It did not increase until 1988, and only then by a fractional increment (to $r = 0.59$). This marginal increase, moreover, could have been a momentary fluctuation or episodic disturbance around the pattern established over a longer time period. Thus, the correlation between aid and compliance between 1985 and 1988 did not change appreciably (see also Table 2 below for elaboration and for evidence that the annual bivariate correlations displayed modest disturbances around an otherwise static pattern).

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[10] These rank-order correlations reflect a one-year lag, given that U.S. foreign assistance outlays are annually budgeted prior to the year in which they are actually distributed to recipients.

Although the Reagan administration did not articulate a criterion or standard for judging an "acceptable" level of voting coincidence with U.S. positions, the correlations suggest that, as the administration complained, the linkage between U.S. aid and recipient behavior was in fact consistently rather modest. The relationship between the level of economic support given and the degree of political support received was, as the administration claimed, far from direct or monotonic. This is particularly noteworthy given that these recipients were the greatest beneficiaries of American assistance.

Presumably, allies would be expected to align themselves frequently. But if the R^2 coefficient is calculated, we find that only 26 percent of the variation in recipient voting of American allies in 1986 could be predicted by reference to the level of American economic assistance. In short, the level of "linkage" between aid and compliance was not as high as the administration preferred; as a consequence, it initiated the strategy in 1986 to tighten the linkage.

Recall that the U.S. government's own account of aid recipients' voting coincidence displayed a substantial decline after the linkage strategy was officially declared in 1986 (see Figure 2). This indicates that the policy of tying aid allocations to diplomatic support did not improve the pre-existing modest empirical association. Our correlational treatment of the same data support but qualify this impression by showing that the balance between deference and defiance did not change substantially as a result of a new policy predicated on the quid-pro-quo strategy. Thus, whereas coincidence rates fell in the wake of the strategy's pronouncement in 1986, the correlations between aid and voting remained relatively steady and did not appreciably strengthen in the aftermath of the strategy's declaration.

From Description to Predictive Modeling

Can this inference be strengthened further? To capture more precisely the impact of U.S. assistance on the diplomatic response of recipient states, greater attention must be given to the threats to valid inference posed by the possible influence of temporal orderings and statistical outliers that could obscure the true nature of the relationships.

It is necessary to introduce time lags because it is unreasonable to expect the bivariate relationship between foreign aid and voting coincidence to change concurrently. Logically, economic incentives and threats cannot be expected to exert an immediate impact. Rather, the effects would be felt and the expected behavior modification witnessed only in subsequent sessions, after recipients were given an opportunity to adjust their diplomatic conduct in response to the American initiative.

Because a factor cannot predict something unless changes in it precede changes in the hypothesized effect, tracing the determinants[11] of behavioral modifications requires introduction of at least a year lag between the level of aid (time T) and the subsequent level of voting coincidence ($T + 1$), as well as inspection of the voting response of recipients and the response of the United States to these policy modifica-

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[11] The comparative study of foreign policy has not generated a cogent causal theory of the determinants of foreign policy behavior; at most, we only benefit from typologies of the potential influences on states' external conduct and crude impressionistic estimates of their relative potency (see Hermann, Kegley, and Rosenau, 1987; Hermann, 1990). In the absence of such a theory, attempting to definitely trace the *causes* of recipient states' reactions to the U.S. aid strategy from among the multitude of potential influences is unwarranted. But if claims to causality are not within reach, so that an explanation can be provided, a predictive model is more appropriate. Hence we cast this policy evaluation in a forecasting framework. More accurately, it is best seen as an exercise in "postdiction" to ascertain if the predicted consequences did indeed result in the aftermath of the linkage strategy that was announced in 1986.

TABLE 2. Coincidence and aid: a matrix of lagged correlations.

Coincidence Aid	1985	1986	1987	1988	1989	1990
1984	0.61	0.61	0.59	0.60	0.58	0.55
1985	0.57	0.57	0.55	0.56	0.54	0.51
1986	0.52	0.51	0.50	0.50	0.49	0.45
1987	0.57	0.57	0.54	0.55	0.54	0.51
1988	0.60	0.61	0.59	0.59	0.57	0.54

$N = 31$.

Note: Pearson correlation coefficients in cells are statistically significant at no less than the 0.05 level.

tions at the successive decision points (i.e., at $T + 2 . . . N$). To control for temporal orderings, regression techniques for analyzing longitudinal data with time lags are appropriate, following quasi-experimental procedures recommended for this purpose by Cook and Campbell (1979) and conventionally prescribed for and applied to the analysis of time-series data in international relations research (for an example, see Ostrom, 1978).

This treatment is reported below (see Figure 3), but as a preface, it is instructive to first calculate and inspect the lagged intercorrelations. The matrix in Table 2 above reports the evolving nature of these relationships so that the correlations between voting coincidence and aid at various intervals can be examined.

This matrix confirms vividly the impression conveyed by examination of the bivariate (contemporaneous) associations each year between aid and coincidence levels discussed above. Here we are presented with a vivid demonstration that changes in the allocation of U.S. aid were *not* followed by appreciable changes in the linkage between aid and recipient coincidence levels. Regardless of the number of years allowed for the impact to take effect, a meaningful fluctuation in the pattern of the correlation is not evident. And the converse of this relationship also is resistant to variation over time: regardless of the interval lapsing between coincidence levels and subsequent aid disbursements, the correlations did not vary substantially; they range across the possible lags within a narrow boundary.

This stability strongly supports the conclusion derived from the government's own reports—that the administration's diplomatic rhetoric in 1986 led to neither an immediate nor a longer-term substantial modification of the relationship between assistance and voting coincidence. Only marginal changes are evident in any of the relationships. This is clear because the correlations remain largely invariant throughout the observation period and regardless of the time allowed for change in either variable to affect the other. If we give the strategy the maximum time to exert an influence, we still fail to find evidence that the strategy produced the results its framers envisioned.

Not only is the general level of covariation essentially trendless; it clearly failed to vary in the aftermath of the strategy's proclamation. This is made apparent by observing the primary lagged coefficients after the 1986 declaration of the policy, that is, by observing the subsequent correlations displayed after the intervention (those underlined in the matrix in Table 2).

First, consider aid interpreted as an independent variable. We observe a very modest rise in the consequent correlation (from $r = 0.57$ in 1987 to $r = 0.61$ in

1988). This increase is of insufficient magnitude to infer that changes in aid levels precipitated subsequent improvements in coincidence levels. Second, conceptualize the level of voting coincidence as an independent variable potentially influencing the subsequent level of U.S. aid—so that the reaction of the U.S. to the changing pattern of recipient voting is observed. Here we also fail to detect a substantial change, although the exhibited decline in the correlation in the wake of the policy initiative (from $r = 0.51$ in 1986 to $r = 0.45$ in 1990) suggests, interestingly enough, that if anything the United States widened rather than closed the linkage in the years following the strategy's initiation. This indicates that the United States did not adjust assistance allocations in a manner that rewarded deference or punished defiance (in compliance with its professed policy), and, to the extent that adjustments were implemented, they proved ineffectual.

But could this conclusion be fallacious for still other reasons? Are there possibly other factors that affected the evidence in a way that prevents the true relationship to be revealed? For internal validation of these results, we need a dynamic model of the changing nature of these multiple relationships so their interconnections can be traced. A cross-lagged longitudinal multiple regression analysis removes the effects that simple synchronous correlations may mask because they are mediated by common influences that the two variables share. We gain clearer insight about the true relationship between aid and coincidence by exercising controls and presenting the linkages in path analysis form.

Accordingly, we now measure (1) levels of voting coincidence in successive years (controlling for levels of aid allocations) and (2) levels of aid in successive years (controlling for levels of coincidence), and then measure the linkages between coincidence and aid levels (and vice versa) in the following year with these effects removed. A cross-lagged model also enables the effects of time to be removed so that the relationships observed are unaffected by these lag effects. This permits us to better infer if the association between foreign aid and voting coincidence was truly different after the policy initiative than before. We will explore whether a discontinuous effect was produced so that it is safe to conclude that the policy had an impact.

Figure 3 presents the results of this data analysis. The cross-lagged path model reports the standardized regression coefficients, or beta weights, between the two variables in an unfolding chronological sequence through the observation period, displaying the relationship that is left over once the effects of time have been controlled. Note that the coefficients reported across the upper horizontal paths inform us of the impact of the level of recipient voting coincidence on voting coincidence the
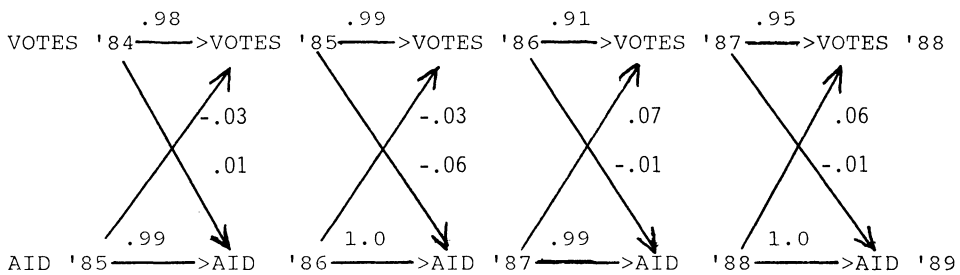

FIG. 3. A cross-lagged path model. All horizontal paths are significant at the 0.001 level; none of the diagonal paths are statistically significant ($N = 31$).

following year (controlling for the level of aid in the previous year). Similarly, the coefficients across the lower horizontal paths record the regressions of aid in each year on the aid level in the previous year (controlling for the level of coincidence in the previous year). The coefficients in the diagonals have been controlled for the impact of time (temporal erosion and autocorrelation), so that the effect of both the level of coincidence and of aid in the previous year are removed from these aid-coincidence relationships. In other words, this shows the impact of aid levels on voting coincidence, or coincidence levels on aid, after the effects of time have been removed.

These calculations further support the conclusion that the linkage strategy did not exert a meaningful impact. The coefficients along both horizontal paths demonstrate powerfully that both coincidence levels and aid allocations are predicted best by the levels in the prior year. Thus, by far the best way of forecasting variations in recipients' voting coincidence is by reference to the level of coincidence in the preceding year (controlling for aid); in fact, nearly all the variation can be predicted by this indicator. Similarly, changes in U.S. aid allocations are best anticipated by reference to the allocation in the previous year (controlling for coincidence variations); aid disbursements are accounted for and predicted fully by the prior year's disbursement, not by the recipients' U.N. voting conduct. Consistent with what is known about lags and inertia in most budgetary disbursements, the strongest predictor of a given year's outcome is the previous year's performance.

Even more dramatic is the unambiguous evidence provided by inspection of the beta coefficients in the diagonals. Here the model informs us unmistakably that there exists *no* statistical relationship between prior voting coincidence levels and subsequent aid allocations, or between prior aid commitments and subsequent voting behavior, once the previous levels of the respective dependent variables are controlled for. The virtual absence of a relationship in either direction (indeed, in some cases a very modest inverse relationship) demonstrates the empirical separation of the two processes. Neither affected the other.

Clearly, the absence of these relationships is continuous over time. The 1986 policy initiative did not alter the pattern; the pre-existing absence of an association between voting coincidence and aid was preserved after the intervention, with only slight changes detectable. This high degree of continuity supports the conclusions suggested by the preceding probes of the data.

Whereas this finding is strongly supported by the analysis of *levels* of aid and compliance, a fundamental threat to inference nonetheless still remains with this aggregate correlation approach: that a cross-lagged path model would wash out the effects of any variable other than the lagged endogenous predictor. However statistically powerful, measures of association between *levels* of aid and compliance may be incapable of detecting change. As aggregate measures they have a static bias built into them.

To guard against this possible error, it is necessary to probe beneath the surface of collective properties by examining *changes* in aid and compliance rates for individual cases. Explored at this disaggregated level, do changes in aid allocations still fail to predict changes in subsequent coincidence rates?

Investigation at the national level of analysis confirms the results emerging at the systemic level of analysis. The same minimal relationship is revealed when the annual change in the two variables is compared. Table 3 displays the correlations between the changes in U.N. voting coincidence rates and U.S. aid allotments.

This pattern of correlations refutes the hypothesis that changes in aid flows and compliance rates were related to changes in the U.S. aid distributions. Indeed, the evidence viewed in this way supports the preceding conclusion that the level of compliance does not predict the levels in American aid disbursements. Rather, the

TABLE 3. U.N. voting coincidence and U.S. foreign aid, comparative
changes (Δ), 1984–1989.

	ΔUN84–85	ΔUN85–86	ΔUN86–87	ΔUN87–88
ΔAID85–86	0.07	−0.28	0.13	0.02
ΔAID86–87	−0.52*	−0.01	−0.06	−0.02
ΔAID87–88	0.27	0.24	0.02	−0.09
ΔAID88–89	−0.26	−0.19	−0.04	0.08

* = Statistically significant at the 0.05 level.
$N = 31$.
Note: Pearson correlation coefficients in cells represent a one-year lag between U.N. activity and U.S. aid.

evidence suggests that the Reagan administration did not systematically implement the strategy it so vociferously announced. Note in particular that the recipients that moved toward relatively more supportive positions with the U.S. were not rewarded by a commensurate increase in U.S. foreign assistance[12]—a fact illustrated by the evidence that changes in the recipients' U.N. voting coincidence levels between 1984 and 1985 were *negatively* correlated ($r = -0.52$) with changes in U.S. assistance distributions between 1986 and 1987. This inverse relationship was statistically significant. Thus, those who increased their voting support for the United States were punished, not rewarded, by subsequent reductions in American aid allocations. This was not consistent with the linkage strategy of reward that the administration pledged. Conversely, countries that distanced themselves from U.S. positions did not suffer from proportional reductions in U.S. assistance as a result of their defiance—a lack of change that was similarly inconsistent with the threat of punishment that the strategy promised.[13]

As an aside to this policy evaluation, it is useful to take still another step in our analysis, using this level-of-change measure, by seeking to determine whether the marginal variations that did occur were associated with the relative gross national products of the recipient states. Even though the Reagan administration did not define the *need* for economic assistance as a criterion and component of its linkage strategy, the literature on the allocation of foreign assistance and the motives influencing its distribution treats the relative need of a country for aid as a critical factor. As Hufbauer and Schott with Elliott (1983:78) observed in their comparative study of economic sanctions, an important hypothesis concerns whether a correlation may exist between the "political and economic health of the target country and its susceptibility to economic pressure."

In the scholarly literature (Richardson and Kegley, 1980; Richardson, 1981), dependent states are often assumed to be politically compliant or deferential to those on whom they economically depend. Thus, as a contribution to this literature, it is useful to investigate whether such a relationship is applicable to this case, even though

[12] This result is strengthened further by nonparametric estimates of association, which again at a lower level of measurement fail to disclose the existence of a strong relationship.

[13] To expand confidence in this conclusion, a similar correlation analysis was conducted in which the same patterns emerged when Israel and Egypt, two countries whose enormous annual aid allotments (more than $5 billion) render them "outliers," were removed from the sample. While both of these countries received a disproportionate amount of U.S. aid each year (as a result of the Camp David and other accords reached under the Carter Administration), Israel in particular was an outlier in its voting agreement rates with the U.S.—averaging about 90 percent during this period. Egypt's average of about 13 percent was far closer to the overall average.

TABLE 4. Changes in U.N. voting coincidence predicted by per-capita income of aid recipients, 1984-1989.

	Intercept	*Slope*	R^2
Δ1984–1985	20.4	− 0.002	0.013
		(0.003)	
Δ1985–1986	14.5	− 0.001	0.009
		(0.002)	
Δ1986–1987	− 27.0	0.002	0.082
		(0.001)	
Δ1987–1988	− 28.4	0.005*	0.446
		(0.001)	

$N = 31$.
* = Significant at the 0.05 level.
Table entries are OLS coefficient estimates. Figures in parentheses are standard errors.

the Reagan administration did not make economic need an explicit component of its strategy. So let us examine here whether the countries politically vulnerable to economic threats—those with lower per-capita gross national products—were relatively more supportive of U.S. positions during the period under review.

As can be seen from Table 4 above, during the period under observation the economic need of the top recipients of U.S. assistance did not predict changes in these dependent states' voting coincidence with the United States. A simple linear regression illuminates the negligible amount of change in the U.N. voting coincidence rates that can be predicted by reference to recipient states' per-capita income. The regressions indicate that variations in recipients' per-capita income exerted practically no effect on their U.N. voting patterns. For the 1987–88 change, there is realistically no effect, although it was shown to be statistically significant. A slope of 0.005 is so small it can be dismissed, and whereas the regression line fit a relatively constricted point cloud, changes in voting coincidence did not change relative to the level of recipients' per-capita income.

This record suggests that patterns of deference and defiance evolved independently of the aid recipients' levels of per-capita income. In fact, after the United States signalled that it intended to reward those states that voted with it on key U.N. votes and punish those that voted in opposition to the U.S. position, those countries with the smallest per-capita GNPs tended to be slightly more defiant than those with the largest levels of national income.[14] The overall picture is one demonstrating that, contrary to the view prevalent in the political economy of international relations literature (for example, see Singer, 1972), between 1984 and 1988 levels of recipients' income did not covary with changes in the level of voting deference those states displayed toward the U.S.

It is also pertinent to consider a corollary hypothesis central to the literature on the foreign policy behavior of economically dependent states: in our context, whether countries for whom U.S. aid was relatively more important or salient (that is, those especially vulnerable states for whom U.S. aid represented a large share of their GNP) were more willing to deferentially modify their voting patterns toward U.S.

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[14] The countries with the lowest per-capita income (including especially Guatemala, Malawi, Niger, Thailand, and the Sudan) decreased their average support for U.S. positions by more than 50 percent between 1986 and 1988. Countries with higher per-capita income (such as Greece, Portugal, and Israel) were among those whose support for U.S. positions either rose (Israel) or fell slightly during this period.

TABLE 5. Changes in U.N. voting coincidence predicted by U.S. aid
as percentage of recipients' 1987 GNP.

	Intercept	Slope	R^2
Δ1984–1985	22.9	− 185.7	0.051
		(149.6)	
Δ1985–1986	10.6	108.8	0.025
		(125.7)	
Δ1986–1987	− 22.1	− 91.3	0.038
		(83.7)	
Δ1987–1988	− 25.7	136.7	0.093
		(79.1)	

$N = 31$.
    Table entries are OLS coefficient estimates. Figures in parentheses are
standard errors. None of the relationships are significant at the 0.05 level.

preferences because their dependence on U.S. assistance was great. To control for this intervening variable in our observed results, we also can evaluate this hypothesis through linear regression by measuring the relative changes in U.N. voting coincidence associated with the proportions of the recipients' U.S. aid receipts to their annual output.

As Table 5 clearly illustrates, the variability in U.N. voting coincidence levels is also only negligibly predicted by the proportion of the recipients' 1987 GNP that was represented by U.S. foreign aid, and the relationship did not vary widely during the period of observation. The evidence indicates that, contrary to the belief that dependent states act deferentially to their benefactors, states for whom U.S. aid represented a relatively large proportion of their GNP (e.g., El Salvador) were no more likely to defer to the U.S. appeal than were states for whom the proportion of U.S. aid was less significant (e.g., India).[15]

The Failed Linkage Strategy in Retrospect: Contending Interpretations

The conclusions, drawn from our various statistical analyses of the data, point collectively to the absence of an empirical association between aid and voting coincidence before and after enactment of the 1986 linkage strategy. The consistency of the findings across these various treatments is much too great to be attributed to chance. In conjunction with the U.S. government's legislatively mandated accounting that showed that the voting agreement of U.S. aid recipients declined after the linkage strategy was implemented, the statistical results of the evaluation identify a policy that failed.

Why did the 1986 linkage strategy fail to produce the effects it sought? We will advance several complementary interpretations. But first a caveat is in order.

On the surface it might appear that the linkage strategy is not amenable to evaluation because the Reagan administration never fully exercised its discretionary authority granted by Congress and thus did not follow through with its threat to link aid

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[15] As an added precaution, in order to test for the possible presence of an interactive effect between need and compliance, we calculated a multiple regression in which compliance is predicted by need (income per capita), salience (aid/GNP), and an *interactive* term representing the multiplicative effects of these variables. These tests mirrored those reported in Tables 4 and 5; the interactive term was never statistically significant and produced virtually no change in the variance explained. Thus the combined interactive effects of the two independent variables failed to increase the capability of this compound measure to mediate the variability in voting coincidence.

allocations tightly to recipients' diplomatic conduct. But that lack of attention to implementation does not render the policy initiative nonexistent, nor policy evaluation moot. The declared initiative comprised a prominent policy. Indeed, the U.S. government spent considerable energy and resources promoting its intentions to punish defiant states and reward deferential ones in the hope that its actions would induce support within the United Nations. This threat was representative of what may be classified a tangible "foreign policy undertaking" (Rosenau, 1980:61). Accordingly, we can safely assume that the targeted state leaders took seriously a policy pronouncement that was stated so vociferously and repeatedly, even if they could not estimate the threat's credibility. Consequently, a policy proclamation of this sort, embedded in public law, is not a random act; it is a bold one whose consequences virtually cry out for analysis.

If words mean anything in diplomatic discourse—as assuredly they do—then the impact (or lack thereof) of these verbal communications requires analysis if the study of interstate relations is to advance. The linkage strategy is a paragon of a formal diplomatic effort to exercise influence through a series of pronouncements that attempted to extract political deference through economic threats. Hence the case is relevant to and holds implications for a wide spectrum of theoretical questions concerning foreign policy compliance, sanctions, economic statecraft, and bargaining. For in this case we have an unusual example of government officials who explicitly enunciated a problem, identified the solution they sought to remedy this problem, postulated the results that initiative was expected to produce, and identified the criteria by which the success of the initiative was to be assessed (as calculated by the government's own measures). What followed as a result of the effort tells us much about the practice of statecraft in the contemporary system and the limits of the system's wealthiest power.

The results clearly illustrate that the solution to the problem was inadequate to reverse the unfavorable conditions in the United Nations that so concerned the Reagan administration. It is therefore pertinent to ask why.

First, our findings add evidence to the discourse on economic sanctions, which posits that such pressures, whether threatened or imposed, are likely to confront stiff resistance from target states. The resilience of aid recipients clearly demonstrates that their policies were driven more powerfully by interests other than by the economic threat of a hegemon. Even though the bilateral relationship between donor and recipients was highly asymmetrical, the limits to the exercise of influence by a dominant state over weak states were revealed. This conforms to other evidence (for example, see Singer, 1972, and Menkhaus and Kegley, 1988) that subordinate states often do not act compliantly toward core states on which they are economically dependent.

The pattern illustrated—that poorer states respond with indifference toward economic threats—is also consistent with most previous research on sanctions. As Hufbauer and Schott with Elliott (1983:76) found from their 70-year survey of sanction "episodes," "At most, there is a weak correlation between economic deprivation and political willingness to change . . . The *economic* impact of sanctions may be pronounced . . . but other factors in the situational context almost always overshadow the impact of sanctions in determining the *political* outcome."

The history of international economic sanctions and reprisals attests to the unusual conditions in which they have proven effective (Wallensteen, 1968; Daoudi and Dajani, 1988). Such measures have rarely succeeded, even under favorable circumstances; under conditions of aid and trade dependence, political compliance rarely emerges—these ties fail to bind (see Roeder, 1985). Auspicious conditions clearly were not present in 1986 when the U.S. undertook its effort to extract compliance with its economic leverage. And the setting—the United Nations—was an inhospitable

environment of cross-cutting alignments in which to overcome the inertia of voting practices.[16]

The inability of the policy to achieve its intended goals may also be attributable in part to the assumptions the Reagan administration embraced about the motives of aid recipients and the incentives to which they would respond. American officials appeared to operate from the assumption that countries in need of foreign assistance would sacrifice their interests and freedom of choice to avert economic sanctions. That belief may have been unwarranted (see Richardson and Kegley, 1980) in that it overlooked the probability that Third World countries would not interpret aid as requiring political concessions. This conclusion is reinforced by the evidence showing that the countries responding most deferentially after 1987 were the states least in need of assistance; this suggests that recipients acted primarily in terms of their strategic interests rather than their economic needs.

Moreover, many recipients not only look askance at the equation of gifts for deference but regard the linkage as exploitative (Krasner, 1985). Instead of viewing foreign aid as a benevolent form of global welfare, some observers assail it as a seductive means for the powerful to coopt the powerless. Past studies are rife with denunciations of *Aid as Imperialism* (Hayter, 1971) that is *Zapping the Third World* (Linear, 1985). Initiatives such as America's aid-for-support linkage strategy are often cited as evidence that extraordinary costs are attached to foreign aid transfers. Overlooked, perhaps, were realistic assessments of *How Foreign Policy Decisions are Made in the Third World* (Korany, 1986)—a subject that this study recommends exploring.

As Harsanyi (1962) has shown, to predict the successful exercise of influence, it is important to consider both the costs to a dominant country of extracting foreign policy compliance *and* the costs to the dependent country of defying the other's attempt at coercion. For many recipients of U.S. assistance, the costs of deference to a powerful donor presumably exceeded the benefits.

The U.S. government was equally constrained by its own economic limitations. Because net levels of U.S. foreign aid declined even before the threats and the linkage strategy were enunciated (as large budget and trade deficits mounted), the "carrot" of foreign aid that U.S. leaders dangled before aid recipients was insufficient to be taken seriously; it is unlikely that many recipients perceived meaningful rewards to be available. A "reverse political effect" (Renwick, 1981:86) could have been operative that undermined U.S. credibility while emboldening recalcitrant aid recipients. In this political climate, the U.S. aid program was increasingly dominated by major recipients representing its established security interests.[17] This restricted the flexibility of the entire program and reduced the salience of nonmilitary criteria in funding decisions which, while providing further ammunition to critics of foreign aid, illustrated the subservience of foreign assistance to geostrategic considerations.

It is also evident that a cohesive plan of action was not implemented by the United States to pressure recipient states for the cooperation that was sought. Such an effort requires not only heated rhetoric on the floor of the General Assembly but concerted interagency coordination extending to the embassy and bureau levels. That institutional coordination was not forthcoming, for reasons well explained by the "bureau-

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[16] Many states' votes on a given issue arguably have more to do with their desire to please a group of allies than their normative stand on the issue. In this sense, the type of "horse trading" prevalent in the U.S. Congress is commonplace on the floor of the General Assembly (Alker, 1969:142).

[17] Security interests dominated deliberations over aid packages to the top five recipients—Israel, Egypt, Pakistan, Turkey, and El Salvador—which received more than half the total amount of U.S. aid during the years under review. Since their annual aid disbursements were less susceptible to cutbacks, their share of the foreign aid pie grew during the period of observation.

cratic politics" paradigm as it applies to the making of American foreign policy (see Hilsman, 1990). Instead, largely due to their role and mission, U.S. diplomats at the United Nations quickly shifted to other aspects of behavior within that multilateral forum.[18] They did not receive support from other agencies responsible for the implementation of American foreign policy, including the White House itself. In his legislatively mandated report on the 1988 session (which featured an all-time low aggregate coincidence rate of 15.4 percent), U.N. Ambassador Vernon Walters sought to defuse criticism with his praise of what he termed "a productive session" by arguing, "The statistics do not tell the complete story. We need to look beyond them" (U.S. Department of State, 1989:I–4).

Instead of the aggregate record, Ambassador Walters explained that increased improvement in "key votes" was more salient to the United States,[19] as was an effort to resolve issues by consensus and to silence anti-American "name-calling" on the floor of the General Assembly. At issue was whether this was a diplomatic way of departing from an ineffective strategy without taking the embarrassing step of acknowledging that the original linkage strategy had been jettisoned.

In the apparent absence of ongoing U.S. efforts toward pursuing an aid-for-agreement linkage, and given the limited economic resources available to back the strategy, the aid program was consistently driven more by bureaucratic momentum and security considerations than by case-by-case evaluations of recipients' diplomatic conduct. This clear pattern should caution current and future policymakers against presuming that the entrenched political dynamics governing the distribution of American foreign aid can be easily modified through an effort to make such ancillary concerns as recipients' voting behavior in the United Nations a primary consideration.

For subsequent research, this pattern also adds empirical evidence to support the thesis that bureaucratic momentum powerfully drives U.S. budget policy and that other factors are often more potent than economic threats in influencing the degree to which U.S. policy positions receive support from the recipients of American foreign assistance. More generally, the inability of the Reagan administration to sustain interest in and effectively carry out its strategy suggests the need for more penetrating empirical and theoretical analysis of the obstacles to a democracy's conduct of foreign affairs. No account of the allocation of American foreign aid programs and its consequences can be complete without attention to the domestic and institutional influences on changes in that relationship.

James Barber (1979:379) underscored this when he advised that "sanctions cannot be isolated from other international and domestic issues. They may clash with other interests, or be given a lower priority than other goals of the imposing states." The subsequent attempt by the U.S. government to link U.N. support and foreign aid apparently overlooked this principle; as a consequence, the threats intrinsic to the bargaining strategy lacked credibility.

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[18] The United States also was preoccupied at the time with its efforts to reform the U.N.'s budgetary process, and with its highly publicized withdrawal from the United Nations Economic, Social, and Cultural Organization (UNESCO). It is notable that the U.S. government persisted in these efforts and ultimately achieved a measure of success in realizing these stated goals.

[19] Whether other states shared the Americans' concern for the "key" votes is an open question, but the overall record clearly revealed the extent to which the United States' isolation from the majority of states in the U.N. General Assembly had continued to grow in the mid-1980s. Even by Walters' own criteria, American foreign policy failed to receive strong support on issues defined as "key" by the U.S. In 1989, the first year for which an account of aggregate figures on "key votes" was published by the State Department, the overall level of support on key issues (23.3 percent) was only slightly higher than the average on all votes (U.S. Department of State, 1990:214). These findings are especially noteworthy given the intense efforts U.S. diplomats made to win support among other U.N. members on these issues.

The Reagan administration's effort failed ultimately because it applied the American foreign aid program to uses for which it was never designed. To exercise influence, aid allocations must consider the interests, values, and perceptions of those whom the United States wishes to influence. It is neither realistic nor consistent with *realpolitik* logic to expect others to conform to pressures by adopting policies that run counter to their national interests.

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