

## The Present and Future Reliability of the LaRouche-Riemann Model's U.S. Forecasts

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As the accompanying table illustrates, the quarterly LaRouche-Riemann forecasts for the U.S. economy have achieved the highest degree of accuracy ever attained in economic forecasting. During the same period, beginning the last quarter of 1979, that these forecasts have been published, all competing forecasts, those of the U.S. government included, have been consistently wrong to a degree of being outrightly absurd.

Per Annum Rate of Change of Industrial Production*	Date of Forecasts	1980		1981		1982	
		1st Half 3/80	2nd Half 7/80	1st Half 1/81	2nd Half 7/81	1st Half 1/82	2nd Half 7/82
<b>EIR Forecasts</b>		- 7.5	+ 1.1	+1.1	- 5.2	-6.0	-6.9
<b>Index of Actual Industrial Production</b>		-14.0	+13.2	+2.6	-12.4	-6.6	-5.4
Data Resources Inc.		- 1.2	-12.1	+2.0	- 0.1	-7.7	+0.3
Wharton Economic Forecast		+ 0.5	-11.4	+9.8	+ 1.8	-9.1	+1.6
Conference Board		+ 2.0	- 9.6	+0.9	- 1.8	-5.1	+1.4
Commerce Department		- 1.6	- 8.6	+6.0	+ 1.4	-4.0	+4.2
Morgan Guaranty		- 1.4	-10.7	+5.9	+ 2.6	-7.5	-2.7
Evans Econometrics		+ 0.3	-11.2	+4.6	+ 4.1	-0.8	+0.9

Nonetheless, although the LaRouche-Riemann forecasting method is perhaps the only competent approach available anywhere in the world today, we are still far short of the standards specified in our original design-specifications. Since all responsible private and governmental agencies will be turning increasingly to use of our forecasting methods, it is our responsibility to forewarn policy-influencers of the limitations of accuracy of the LaRouche-Riemann forecasts. It was wrong for the policy-influencers to disregard the most accurate forecasting service in existence during the crucial decision-making of the recent three years. It would be only less an error if those policy-influencers now went to the opposite extreme, and wishfully overestimated the accuracy of this forecasting method.

Therefore, this report outlines points on which every government, corporate, and trade-union official should be broadly informed, and on which economists and related professional policy-influencers must develop rather precise knowledge. With only a necessary degree of brief reference to quality of the LaRouche-Riemann forecasts during preceding years, we outline where the matter stands at present, and what improvements may be expected during the near future.

### ***The Principled Limits of Forecasting Accuracy***

No man can predict the future, whether in economic forecasting or any other feature of life of nations. The best we can do is to forecast the logical consequences of either continuing existing policies or replacing those with rather well-defined changes in policies.

For example, assuming no monetary collapse during early 1983 (a very large assumption today), and assuming that the deficits of governments do not gobble up all increases in money-supply of the United States and other nations, the most optimistic forecast for the first half of 1983 under present policy-matrices, would be a continued collapse at a rate of between 4 percent and 7 percent per annum. In fact, government deficits (and financing requirements) are much greater than is presently admitted publicly. In fact, although a worldwide chain-reaction financial collapse could occur almost any day during the first half of 1983, the probable period for such a general financial collapse is presently during the March–April period, probably a few weeks prior to the May Williamsburg monetary conference.

The forecast of a probable (optimistic) 4 percent to 7 percent rate of continued collapse is based on “objective” forecasting considerations alone. However, there are conditions under which a slight recovery might be construed to be in progress during some part of the first 45 days of 1983. That latter case illustrates one of the general problems of interpretation of “objective” forecasts.

The forecast of the probable collapse of the world's financial system is a trickier proposition. That forecast is objective in the respect that it will be impossible to refinance the portion of the world debt threatened with collapse during 1983, unless that refinancing is accomplished by actions presently strongly rejected by the governments and financial institutions of North America and Western Europe. The present babbling about the possible success of “case by case” management of Third World debt-problems is an instance of outright political lying by the leading governmental and financial institutions issuing such reports. We have been on the verge of a general collapse of the world's financial structures since summer 1982, and the problem has been growing progressively worse since August 1982.

However, since short-term bookkeeping tricks can keep defaulted debts legally assets for a limited period of time, the dominant financial institutions of the world do have a limited power to regulate the timing of a new financial collapse. They have limited power either to postpone an inevitable financial collapse by aid of such bookkeeping tricks, or to trigger a potential chain-reaction collapse on any morning they choose to prick the financial bubble. Our forecast of the March-April period—preceding the May monetary conference—is therefore based in large part on rather intimate knowledge of the present policies of dominant financial agencies.

Let us examine both instances, to illustrate the two general ways actual developments may deviate for short periods from the most scientifically accurate forecast which might be offered.

It is possible, for example, for the major automotive firms to choose to build inventories from a present 60-plus days' supply to a 75-, or even a 100-days' supply. This can be done on condition that the financial community decides to fund such inventory-building. If such production of inventory surpluses were extended beyond the automotive industry to several other industries, it would be possible to create a deceptive appearance of a slight U.S. recovery in progress over a period of 30 days or so, until the point was reached at which the excess inventory caused the economy to plunge downward much more sharply than if no such short-time inventory-building binge had been undertaken. At the end of 90 to 180 days, firms would be bankrupted which would have survived the period had no misdirected inventory-building binge occurred.

In such a case, the forecaster has committed no error. Any significant deviation from the policy-guidelines indicated by his forecast leads the economy to a relatively increased degree of disaster. It is not the function of forecasting to predict; the function of good forecasting is to assist the policy-shaping processes of government, financial institutions, and private institutions generally.

In the second illustrative case, the forecast is premised on the following set of facts.

The best-informed circles of international finance, in Venice, Switzerland, London, and so forth, have decided that a general financial collapse, wiping out between \$1 and \$2 trillion of paper values, is inevitable for 1983. We have discussed this in detail and at length repeatedly with the most powerful circles of policymakers associated with the direction of such institutions. Their opinion is correct, assuming that they continue to reject the only existing alternatives to such a collapse—as they do. Moreover, under present and projected policies of the leading bankers and governments of Western Europe and the United States, the collapse will occur by the end of the second quarter of 1983 or immediately afterward.

In that aspect of our forecasting, our judgment is based on both objective considerations independent of the wills of policymakers, and upon knowledge that those policymakers more or less correctly estimate the situation on that account. One therefore assumes that those policy-makers will act to minimize injury to their own special interests under these conditions.

Our knowledge extends further in the matter. We have discovered and cross-checked the particular game-plan for first-half 1983 actually adopted by the most powerful circles of such policy-makers. Moreover, that discovered plan is currently in operation, through aid of such complicit agencies as Secretary of State George Shultz, *Time* magazine's January 10, 1983 issue, and Sen. Charles Mathias (R-Md.) in rigging the climate of panic around the U.S. Congress and administration.

The plan of the Anglo-Swiss bankers is to shift the relatively greater burden of the collapse to the United States. This requires a bamboozling of the Reagan administration and the Congress. The intent is to break the will of the Reagan administration by January 28, 1983, and to use administration support for the "bail-out" scheme to ensure total capitulation by the U.S. Congress during the latter part of February 1983.

The nominal policy-objective of this short-term operation is to terrorize the United States and other nations into submitting to the plans projected to be adopted by the international monetary conference now scheduled to occur at Williamsburg, Virginia during May 1983. That plan is currently described most frequently as an intent to establish the International Monetary Fund (IMF) as a World Central Bank at that conference. This plan is sometimes described as a revival of the proposals made by Britain's John Maynard Keynes at the close of the last World War. In fact, it is an intent to use the IMF as a political front for a worldwide financial dictatorship by the Basel, Switzerland Bank for International Settlements (BIS), *a private bank*. Under the proposed arrangement, the BIS would exert greater dictatorship over world trade and the internal economic policies of nations than the Federal Reserve System presently exerts over the U.S. internal economy. *It is the greatest swindle in modern history, transforming the U.S. constitution to a mere dead-letter, and eliminating the last vestige of actual sovereignty of the United States, among other victimized nations.*

If the United States can be induced—during January and February 1983—to underwrite the largest portion of a general collapse of debt-value, the Anglo-Swiss financial interests will escape the worst effects of a general financial collapse. It then becomes convenient for the Anglo-Swiss interests to proceed with their stated plan, to collapse the international financial system during March 1983. Then, after several weeks of the terror such a collapse represents, the nations will "sign anything" the Anglo-Swiss demand at the May Williamsburg meeting.

Overall, this forecast of the most probable timing for a financial collapse is necessary, to forewarn policy-influencers of the most probable decisions to be made. Yet, even our widespread documentation of the plans indicated does tend to cause alteration of those plans. Again, *we are not predicting; we are forecasting*. The difference in character between the first illustration of problems of forecasting and this second condition ought to be more or less obvious.

The most important feature of such forecasting is not the specific sets of figures projected. Such figures do have practical importance, of course. However, the real importance of the forecast is to assist policymakers and policy-influencers in understanding the most characteristic features of those economic and monetary problems to which their policymaking must be addressed. A forecast is functionally a forewarning of the most probable direction of developments which will ensue unless we act quickly and efficiently to change the policy-framework determining such probable direction of developments. We are not “predicting the winners of the race;” we are “handicapping” the policies which determine the “probable winners and losers.”

The practical significance of these cautionary words of advice on forecasting loses all vagueness once we have shifted our attention from such generalizations, to the internal features of the methods employed for the LaRouche-Riemann forecasting practices in particular.

### ***The LaRouche-Riemann Method, Past and Present***

Most broadly, the LaRouche-Riemann method of analytical forecasting is based upon what Treasury Secretary Alexander Hamilton was the first to name officially the “American System of political-economy.” This American System, established as the direct adversary to the British political-economic dogmas of Adam Smith, Thomas Malthus, and David Ricardo, was the policy upon which the successful development of the United States was premised, as well as the economic policy adopted by the Meiji Restoration in Japan. The superior performance of Japan relative to the United States today is predominantly a result of the fact that Japan’s practice is strongly influenced by the American System, whereas the U.S. economy has been ruined by overdoses of the British system.

The American System was brought into the young United States by the circles around Benjamin Franklin. The principal influence upon the Americans was French mercantilism, but a French mercantilism incorporating the initial discovery and development of economic science (beginning 1671) by Gottfried Leibniz. The American System was therefore congruent in essential features with the political-economy of Lazare Carnot’s Ecole Polytechnique, and the economic thinking of Claude Chaptal and Charles A. Dupin. After the disastrous effects of the administrations of Jefferson and Madison, who ruined the U.S.

economy (and national defense) with doses of Adam Smith, the American System was revived by Presidents Monroe and John Quincy Adams, under the influence of a close collaborator of Benjamin Franklin, Philadelphia's Mathew Carey. Through channels of the Society of Cincinnatus, especially the Marquis de Lafayette, French and German developments in economic science and technology were fused with home-grown U.S. American System political-economy, including the important work of Commandant Sylvanus Thayer at West Point. The German-American Friedrich List, the architect of the German economic miracle of the 19th century, was a close collaborator of Lafayette and Carey, as well as a specialist in the work of Chaptal and Dupin. Later, Henry C. Carey, Lincoln's chief economic adviser and son of Mathew Carey, joined the ranks of Hamilton, Mathew Carey, and Friedrich List as the most famous exponents of the American System. It was chiefly through Carey collaborator E. Peshine Smith, that the economic miracle of the American System was introduced to Japan.

Provided we include Leibniz among the founders of the American System, this writer added nothing to economic science which was not established in principle by the indicated authorities. The LaRouche-Riemann method represents, predominantly, the writer's successful solution of the chief internal limitation of the earlier design of the American System. He discovered, beginning 1952, that an approach based on reference to the mathematical physics of the great Bernhard Riemann permits us to master the previously unsolved problem of exposing the explicit connection between advances in technology and increases in the potential rate of economic growth.

Aided by continued progress in perfecting that method of analytical forecasting, successful forecasts were made during late 1956 into January 1957 (of the arrival, and character of the 1957–59 recession), a long-range forecast of 1958–59 (forecasting the eruption of a post-1964 series of monetary crises leading toward a new depression worse than that of the 1930s), and important supplements to that long-range forecast made during 1971 and 1974–75.

The present LaRouche-Riemann quarterly forecasts for the U.S. economy are the outgrowth of a design developed during December 1978, leading to the publication of the first regular, quarterly forecast for the U.S. economy eleven months later. The circumstances of that December 1978 design are relevant in several ways to appreciation of the principal internal features of the LaRouche-Riemann forecasting method today.

The decision to proceed with development of a computer-based quarterly forecasting system was made during early December 1978, during the course of two seminars held in New York City. The subject of those seminars was an assessment of the comparative progress of U.S.

and Soviet work in relativistic physics research. The proposal to develop the regular economic forecast occurred as a by-product of those seminars' discussions.

The general focus of the seminars was the apparent failure of both leading U.S. and Soviet circles to appreciate the significance of the connection between Riemann's 1854 habilitation dissertation<sup>1</sup> and Riemann's famous 1859 paper on shock-waves,<sup>2</sup> despite the importance of the later paper in the development of the H-bomb.<sup>3</sup> It was in this context that the LaRouche-Riemann forecasting was proposed.

The LaRouche-Riemann method of analytical forecasting involves the assembly of a special kind of potential function, in which all crucial transformations within economic processes are comprehended mathematically as the generation of "shock-waves," a generation analogous to the generation of "sonic booms" as predicted by Riemann's 1859 paper. The significance of this latter method is properly understood only if the 1859 paper is understood as a product of the program summarized in Riemann's 1854 habilitation dissertation.

In December 1978, it was proposed that applying the LaRouche-Riemann method to computer forecasting would not only provide the most accurate forecasting for the U.S. economy available, but that the success of such forecasting would help to persuade U.S. laboratories of the importance and efficacy of our approach to the connection between the two cited Riemann papers.

The problem within the U.S. science community (and also within the Soviet community, at least to a large degree), is that during the 1860s and later a massive, escalating attack was deployed to eradicate Riemann's influence from the practice and teaching of science. Those attacks came from the followers of Laplace and Cauchy in France, from Vienna-influenced circles such as Kronecker, Dedekind, and Helmholtz in Germany, and massive attacks from the Apostles' group at Britain's Cambridge University.<sup>4</sup> From the beginning of his career in

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<sup>1</sup> "On the Hypotheses Which Underlie Geometry." A passable translation by Clifford has been available, reprinted by Dover publications in Smith, ed., *A Source Book in Mathematics*, 1959. A corrected translation has been made by the writer's associates for a book on leading features of Riemann's work compiled and edited, with commentaries, by Uwe von Parpart, planned for publication during the months ahead. The relationship of Riemann's original work to the work of Louis Legendre, the significance of what Riemann named "Dirichlet's Principle" for the general notion of the Riemann surface, and the position of the 1859 paper on shock-waves in this setting are among the leading topics documented and discussed by Parpart in that book in preparation.

<sup>2</sup> "On The Propagation of Plane Air Waves of Finite Amplitude," U. Parpart and S. Bardwell, trans., *International Journal of Fusion Energy*, Vol. 2, No. 3, 1980. This translation was commissioned during the December 1978 seminars.

<sup>3</sup> This debt to Riemann is openly discussed in the published Soviet literature, but, until recently, Riemann's 1859 paper was treated as virtually a military secret by the United States.

<sup>4</sup> Russell's *Lectures on Geometry*, his first book, was devoted to scurrilous attacks on Riemann and Cantor. Later, according to Göttingen archives, Russell travelled to Germany to continue his campaign of vilification against Riemann, Cantor, and Felix Klein. Lord Rayleigh, during the 1890s, certified himself an eternal ass by

science, until he dropped out of science during the middle 1920s, Bertrand Russell's work within the nominal bounds of mathematical science was devoted entirely to attempting to destroy the influence of Riemann, Georg Cantor, and Göttingen University's Felix Klein. So, although Minkowski, Einstein and others adopted a somewhat corrupted version of Riemann for relativistic physics, and though the problems of supersonic and space flight, as well as the H-bomb, forced Riemann's 1859 paper back into attention, Riemann's work is tolerated only by exception in university teaching-programs and related areas today. The Newton-Cauchy-Maxwell faction of Riemann's embittered adversaries is relatively hegemonic in teaching and prevailing professional opinion today. This has proven a potentially disastrous impediment to progress in certain branches of work in the United States today, a disaster incorporated in the fanatically anti-Riemannian opinions expressed by Presidential science adviser George Keyworth presently, a product of the "Oppenheimer Faction" at the Los Alamos Laboratory.<sup>5</sup>

Since the connection between technological advancement and economic growth is central to "impact studies" for scientific research-and-development work today, it was hoped that the demonstrated superiority of the LaRouche-Riemann method of forecasting for such purposes would tend to catalyze fruitful rethinking about the importance of Riemann's work among leading laboratories, thus provoking fresh, fruitful approaches to designing a new set of experiments employing Riemannian hypotheses and breaking out of the bounds of the Newton-Cauchy-Maxwell box impeding U.S. science presently.

To set the development of the LaRouche-Riemann forecasting into motion, during December 1978, this writer produced a design for the "model," guiding the work of two coordinated teams. The one team was a data-assembly staff under the direction of team-leader David P. Goldman. The other was a scientific and computer-programming team under the direction of Uwe von Parpart and Dr. Steven Bardwell. Parpart did the overall coordination of the development of the "model," and the writer limited himself chiefly to supervision of fulfillment of design-specifications and auditing of quality of forecasting work in progress.

The December 1978 design had two interdependent features-in-chief. The first of these features was the translation of the writer's analytical method into the form of a set of constraints suited for computer operations. The second feature was the specification that all "non-linear transformations" were to be mathematically comprehended from the standpoint of the cited 1859 Riemann paper on "shock-waves."<sup>6</sup>

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denouncing the mathematics of Riemann's 1859 paper.

<sup>5</sup> Interview with Paul Gallagher, 1981.

<sup>6</sup> See, Parpart and Bardwell, "Economics Becomes a Science," *Fusion*, July 1979.



From the beginning, it was policy that the actual operations would be a reasonable approximation of those design-specifications. The conditions requiring resort to approximations were chiefly these. 1) We were limited by the financial means available, and limitations of staff and computer facilities so imposed. 2) We were limited by the shortcomings of existing masses of data. Important categories of data simply do not exist, except as reasonable approximation may estimate them. Available U.S. government and Federal Reserve data are a mixture of sampling, guesstimates, and dubious outright concoctions. The “garbage” of available official data placed limitations on the possible accuracy of detail in the forecasts produced. 3) There are significant, intrinsic limitations in attempting to use even the best available existing computer systems for processing mathematical functions of the type implicit in the specifications. Economical computer operations required resort to reasonable short-cuts, and ongoing work in developing an improved choice of mathematical procedures. 4) There are significant, obvious improvements in mathematical physics needed to permit the more advanced modes of forecasting the design implies. Several years of research on two continents, including correlation of little-known primary material buried in archives, has been conducted to the purpose of approaching the point at which such more sophisticated considerations could be resolved.<sup>7</sup>

During October 1979, immediately following the introduction of the Volcker policies by President Jimmy Carter and Paul A. Volcker, this writer requested the staff to accelerate the schedule, to test the rate at which continuation of Volcker's policies would lead into a general economic depression. So, the first quarterly forecast for the U.S. economy was issued in November 1979, rather than the previously intended issuance during 1980.

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<sup>7</sup> Lyndon LaRouche, “What Is an Economic Shock Wave?” *EIR*, Dec. 7 (Part One) and Dec. 14 (Part Two), 1982, and Jonathan Tennenbaum, “A Topological, Shock-Wave Model of the Generation of Elementary Particles,” *EIR*, Feb. 1, 1983. The direct bearing of Tennenbaum's paper on the shock-wave issue is underscored most obviously in the introductory section of that published working-paper. Although the writer demands a conical generation of world-line space, as opposed to the cylindrical version employed by Tennenbaum in that paper, two significances of that treatment of Minkowski's special relativity by Tennenbaum are to be emphasized. First, it is a pedagogical exercise, which makes its point adequately by employing the simpler cylindrical illustration. Secondly, more important, it corrects Minkowski, by eliminating the field-particle paradox from his schema, and relocating Minkowski's conceptions within Riemannian space. The objective of this mathematical work is to arrive at the proper quantum-theory for economic shock-waves, treating shock-wave leaps as leaps from one “Keplerian” harmonic set of values to another harmonic set, as the work of E. Schrödinger and A. Sommerfeld implies for quantum relations. This is consistent with M. Planck's account of his work, a work which has general implications for relativistic physics, by no means limited to microphysics. The question for us is: What are the characteristics of allocation of limited investment-resources to advanced technologies-development, such that we can anticipate with required efficiency how much such concentration of investment is required to generate a desired economic shock-wave effect? This requires, of course, empirical studies of such processes in actual economies; however, we need the general theory the analysis of such empirical data requires.

Despite the limitations of approximation incorporated into the computer application, the November 1979 forecast soon proved itself the most accurate short-term forecast which had ever been made in economic forecasting to that date. The medium-term forecast produced together with the forecast issued during the first quarter of 1980 accurately forecast the sequence and general timing of developments leading from then into the last quarter of 1981.

Important improvements, including improvements in inter-sectoral analysis, were accomplished during 1981. This led into the December 1981 general forecast for 1982, issued together with the quarterly forecast for that time. This forecast that a continuation of administration-Federal Reserve policies during 1982 meant approximately a 7 percent rate of decline in rate of goods-output during the first nine months of 1982, and an accelerated rate of decline, to the 12–15 percent annual rate, during approximately October 1982. That forecast has been accurate to within the range of the margin of error of existing governmental and Federal Reserve reporting-data.

Such accuracy as that December 1982 forecast had never been approached in modern history before then. Certain conclusions must be advanced concerning the competence of judgment of those who continue to rely on discredited, “Brand X” varieties of forecasting.

During 1982, by approximately September 1982, a major further improvement in the design of the computer programs was accomplished. Aided indirectly by important work accomplished in the Federal Republic of Germany in collaboration with this writer, an important advance in the mathematical physics of the “model” was accomplished. This advancement will be reflected in projected forecasts for 1983 quarters, as soon as a rather massive amount of kitchen-work in improving the database is accomplished.

Still to be done, the next further step will be to recast the analysis entirely so as to reflect the demographic features of society, together with improved treatments of energy-flux-density. The assembly of that data base and its incorporation will significantly improve the refinement of U.S. forecasting, and is indispensable for both global forecasting and analytical studies of developing-nations economies. Work toward building up to a global forecasting model is making progress, aided by a process of bringing a number of particular countries' economies into regular forecasting practice. In the case of developing nations, much of the data published by agencies of such nations and by agencies of the United Nations Organization, are chiefly rather arbitrary concoctions with little relationship to actuality. The population-model is indispensable for any early competence in forecasting for such cases. The advancement of quality of the database in these ways is indispensable for the more advanced quality of analytical studies projected for the period ahead.

So far, the LaRouche-Riemann forecasting method is adequate for policy-shaping of general economic and monetary policies for entire economies and coordination of major sectors of those entire economies. Presently, it permits only broad, if useful indications concerning other important matters, such as technology impact-studies. Additionally, as we have indicated, even in the best form it could acquire, it would always suffer the limitations intrinsic to forecasting. It can merely predict the impact of policies; it cannot predict exactly what governments and firms will do.