

**A FREE-MARKET
MONETARY SYSTEM**

A FREE-MARKET MONETARY SYSTEM

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*A claim for equality of
material position can be
met only be a government
with totalitarian powers.*

F.A. Hayek

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When a little over two years ago, at the second Lausanne Conference of this group, I threw out, almost as a sort of bitter joke, that there was no hope of ever again having decent money, unless we took from government the monopoly of issuing money and handed it over to private industry, I took it only half seriously. But the suggestion proved extraordinarily fertile. Following

A lecture delivered at the Gold and Monetary Conference, New Orleans, November 10, 1977. It made its first appearance in print in the *Journal of Libertarian Studies* 3, no 1 (Fall 1979).

it up I discovered that I had opened a possibility which in two thousand years no single economist had ever studied. There were quite a number of people who have since taken it up and we have devoted a great deal of study and analysis to this possibility.

As a result I am more convinced than ever that if we ever again are going to have a decent money, it will not come from government: it will be issued by private enterprise, because providing the public with good money which it can trust and use can not only be an extremely profitable business; it imposes on the issuer a discipline to which the government has never been and cannot be subject. It is a business which competing enterprise can maintain only if it gives the public as good a money as anybody else.

Now, fully to understand this, we must free ourselves from what is a widespread but basically wrong belief. Under the Gold Standard, or any other metallic standard, the value of money is not really derived from gold. The fact is, that the necessity of redeeming the money they issue in gold, places upon the issuers a discipline which forces them to control the quantity of money

in an appropriate manner; I think it is quite as legitimate to say that under a gold standard it is the demand of gold for monetary purposes which determines that value of gold, as the common belief that the value which gold has in other uses determines the value of money. The gold standard is the only method we have yet found to place a discipline on government, and government will behave reasonably only if it is forced to do so.

I am afraid I am convinced that the hope of ever again placing on government this discipline is gone. The public at large have learned to understand, and I am afraid a whole generation of economists have been teaching, that government has the power in the short run by increasing the quantity of money rapidly to relieve all kinds of economic evils, especially to reduce unemployment. Unfortunately this is true so far as the short run is concerned. The fact is, that such expansions of the quantity of money which seems to have a short run beneficial effect, become in the long run the cause of a much greater unemployment. But what politician can possibly care about long run effects if in the short run he buys support?

My conviction is that the hope of returning to the kind of gold standard system which has worked fairly well over a long period is absolutely vain. Even if, by some international treaty, the gold standard were reintroduced, there is not the slightest hope that governments will play the game according to the rules. And the gold standard is not a thing which you can restore by an act of legislation. The gold standard requires a constant observation by government of certain rules which include an occasional restriction of the total circulation which will cause local or national recession, and no government can nowadays do it when both the public and, I am afraid, all those Keynesian economists who have been trained in the last thirty years, will argue that it is more important to increase the quantity of money than to maintain the gold standard.

I have said that it is an erroneous belief that the value of gold or any metallic basis determines directly the value of the money. The gold standard is a mechanism which was intended and for a long time did successfully force governments to control the quantity of the money in an appropriate manner so as to keep its value equal with

that of gold. But there are many historical instances which prove that it is certainly possible, if it is in the self-interest of the issuer, to control the quantity even of a token money in such a manner as to keep its value constant.

There are three such interesting historical instances which illustrate this and which in fact were very largely responsible for teaching the economists that the essential point was ultimately the appropriate control of the quantity of money and not its redeemability into something else, which was necessary only to force governments to control the quantity of money appropriately. This I think will be done more effectively not if some legal rule forces government, but if it is the self-interest of the issuer which makes him do it, because he can keep his business only if he gives the people a stable money.

Let me tell you in a very few words of these important historical instances. The first two I shall mention do not refer directly to the gold standard as we know it. They occurred when large parts of the world were still on a silver standard and when in the second half of the last century silver suddenly

began to lose its value. The fall in the value of silver brought about a fall in various national currencies and on two occasions an interesting step was taken. The first, which produced the experience which I believe inspired the Austrian monetary theory, happened in my native country in 1879. The government happened to have a really good adviser on monetary policy, Carl Menger, and he told them,

Well, if you want to escape the effect of the depreciation of silver on your currency, stop the free coinage of silver, stop increasing the quantity of silver coin, and you will find that the silver coin will begin to rise above the value of their content in silver.

And this the Austrian government did and the result was exactly what Menger had predicted. One began to speak about the Austrian "Gulden," which was then the unit in circulation, as banknotes printed on silver, because the actual coins in circulation had become a token money containing much less value than corresponded to its value. As silver declined, the value of the silver

Gulden was controlled entirely by the limitation of the quantity of the coin.

Exactly the same was done fourteen years later by British India. It also had had a silver standard and the depreciation of silver brought the rupee down lower and lower till the Indian government decided to stop the free coinage; and again the silver coins began to float higher and higher above their silver value. Now, there was at that time neither in Austria nor in India any expectation that ultimately these coins would be redeemed at a particular rate in either silver or gold. The decision about this was made much later, but the development was the perfect demonstration that even a circulating metallic money may derive its value from an effective control of its quantity and not directly from its metallic content.

My third illustration is even more interesting, although the event was more short lived, because it refers directly to gold. During World War I the great paper money inflation in all the belligerent countries brought down not only the value of paper money but also the value of gold, because paper money was in the large measure substituted for gold, and the demand for gold fell. In

consequence, the value of gold fell and prices in gold rose all over the world. That affected even the neutral countries. Particularly Sweden was greatly worried: because it had stuck to the gold standard, it was flooded by gold from all the rest of the world that moved to Sweden which had retained its gold standard; and Swedish prices rose quite as much as prices in the rest of the world. Now, Sweden also happened to have one or two very good economists at the time, and they repeated the advice which the Austrian economists had given concerning the silver in the 1870s, "Stop the free coinage of gold and the value of your existing gold coins will rise above the value of the gold which it contains." The Swedish government did so in 1916 and what happened was again exactly what the economists had predicted: the value of the gold coins began to float above the value of its gold content and Sweden, for the rest of the war, escaped the effects of the gold inflation.

I quote this only as illustration of what among the economists who understand their subject is now an undoubted fact, namely that the gold standard is a partly effective

mechanism to make governments do what they ought to do in their control of money, and the only mechanism which has been tolerably effective in the case of a monopolist who can do with the money whatever he likes. Otherwise gold is not really necessary to secure a good currency. I think it is entirely possible for private enterprise to issue a token money which the public will learn to expect to preserve its value, provided both the issuer and the public understand that the demand for this money will depend on the issuer being forced to keep its value constant; because if he did not do so, the people would at once cease to use his money and shift to some other kind.

I have as a result of throwing out this suggestion at the Lausanne Conference worked out the idea in fairly great detail in a little book which came out a year ago, called *Denationalization of Money*. My thought has developed a great deal since. I rather hoped to be able to have at this conference a much enlarged second edition available which may already have been brought out in London by the Institute of Economic Affairs, but which unfortunately

has not yet reached this country. All I have is the proofs of the additions.

In this second edition I have arrived at one or two rather interesting new conclusions which I did not see at first. In the first exposition in the speech two years ago, I was merely thinking of the effect of the selection of the issuer: that only those financial institutions which so controlled the distinctly named money which they issued, and which provided the public with a money, which was a stable standard of value, an effective unit for calculation in keeping books, would be preserved. I have now come to see that there is a much more complex situation, that there will in fact be two kinds of competition, one leading to the choice of standard which may come to be generally accepted, and one to the selection of the particular institutions which can be trusted in issuing money of that standard.

I do believe that if today all the legal obstacles were removed which prevent such an issue of private money under distinct names, in the first instance indeed, as all of you would expect, people would from their own experience be led to rush for the only thing they know and understand, and start

using gold. But this very fact would after a while make it very doubtful whether gold was for the purpose of money really a good standard. It would turn out to be a very good investment, for the reason that because of the increased demand for gold the value of gold would go up; but that very fact would make it very unsuitable as money. You do not want to incur debts in terms of a unit which constantly goes up in value as it would in this case, so people would begin to look for another kind of money: if they were free to choose the money, in terms of which they kept their books, made their calculations, incurred debts or lent money, they would prefer a standard which remains stable in purchasing power.

I have not got time here to describe in detail what I mean by being stable in purchasing power, but briefly, I mean a kind of money in terms which it is equally likely that the price of any commodity picked out at random will rise as that it will fall. Such a stable standard reduces the risk of unforeseen changes in the prices of particular commodities to a minimum, because with such a standard it is just as likely that any one commodity will rise in price or will fall in price

and the mistakes which people at large will make in their anticipations of future prices will just cancel each other because there will be as many mistakes in overestimating as in underestimating. If such a money were issued by some reputable institution, the public would probably first choose different definitions of the standard to be adopted, different kinds of index numbers of price in terms of which it is measured; but the process of competition would gradually teach both the issuing banks and the public which kind of money would be the most advantageous.

The interesting fact is that what I have called the monopoly of government of issuing money has not only deprived us of good money but has also deprived us of the only process by which we can find out what would be good money. We do not even quite know what exact qualities we want because in the two thousand years in which we have used coins and other money, we have never been allowed to experiment with it, we have never been given a chance to find out what the best kind of money would be.

Let me here just insert briefly one observation: in my publications and in my lectures including today's I am speaking constantly about the government monopoly of issuing money. Now, this is legally true in most countries only to a very limited extent. We have indeed given the government, and for fairly good reasons, the exclusive right to issue gold coins. And after we had given the government that right, I think it was equally understandable that we also gave the government the control over any money or any claims, paper claims, for coins or money of that definition. That people other than the government are not allowed to issue dollars if the government issues dollars is a perfectly reasonable arrangement, even if it has not turned out to be completely beneficial. And I am not suggesting that other people should be entitled to issue dollars. All the discussion in the past about free banking was really about this idea that not only the government or government institutions but others should also be able to issue dollar notes. That, of course, would not work.

But if private institutions began to issue notes under some other names without any fixed rate of exchange with the official

money or each other, so far as I know this is in no major country actually prohibited by law. I think the reason why it has not actually been tried is that of course we know that if anybody attempted it, the government would find so many ways to put obstacles in the way of the use of such money that it could make it impracticable. So long, for instance, as debts in terms of anything but the official dollar cannot be enforced in legal process, it is clearly impracticable. Of course it would have been ridiculous to try to issue any other money if people could not make contracts in terms of it. But this particular obstacle has fortunately been removed now in most countries, so the way ought to be free for the issuing of private money.

If I were responsible for the policy of any one of the great banks in this country, I would begin to offer to the public both loans and current accounts in a unit which I undertook to keep stable in value in terms of a defined index number. I have no doubt, and I believe that most economists agree with me on that particular point, that it is technically possible so to control the value of any token money which is used in competition with other token monies as to fulfill

the promise to keep its value stable. The essential point which I can not emphasize strongly enough is that we would get for the first time a money where the whole business of issuing money could be effected only by the issuer issuing good money. He would know that he would at once lose his extremely profitable business if it became known that his money was threatening to depreciate. He would lose it to a competitor who offered better money.

As I said before, I believe this is our only hope at the present time. I do not see the slightest prospect that with the present type of, I emphasize, the present type of democratic government under which every little group can force the government to serve its particular needs, government, even if it were restricted by strict law, can ever again give us good money. At present the prospects are really only a choice between two alternatives: either continuing an accelerating open inflation, which is, as you all know, absolutely destructive of an economic system or a market order; but I think much more likely is an even worse alternative: government will not cease inflating, but will, as it has been doing, try to suppress the

open effects of this inflation; it will be driven by continual inflation into price controls, into increasing direction of the whole economic system. It is therefore now not merely a question of giving us better money, under which the market system will function infinitely better than it has ever done before, but of warding off the gradual decline into a totalitarian, planned system, which will, at least in this country, not come because anybody wants to introduce it, but will come step by step in an effort to suppress the effects of the inflation which is going on.

I wish I could say that what I propose is a plan for the distant future, that we can wait. There was one very intelligent reviewer of my first booklet who said,

Well, three hundred years ago nobody would have believed that government would ever give up its control over religion, so perhaps in three hundred years we can see that government will be prepared to give up its control over money.

We have not got that much time. We are now facing the likelihood of the most unpleasant political development, largely as

a result of an economic policy with which we have already gone very far.

My proposal is not, as I would wish, merely a sort of standby arrangement of which I could say we must work it out intellectually to have it ready when the present system completely collapses. It is not merely an emergency plan. I think it is very urgent that it become rapidly understood that there is no justification in history for the existing position of a government monopoly of issuing money. It has never been proposed on the ground that government will give us better money than anybody else could. It has always, since the privilege of issuing money was first explicitly represented as a Royal prerogative, been advocated because the power to issue money was essential for the finance of the government—not in order to give us good money, but in order to give to government access to the tap where it can draw the money it needs by manufacturing it. That, ladies and gentlemen, is not a method by which we can hope ever to get good money. To put it into the hands of an institution which is protected against competition, which can force us to accept the money, which is subject to

incessant political pressure, such an authority will not ever again give us good money.

I think we ought to start fairly soon, and I think we must hope that some of the more enterprising and intelligent financiers will soon begin to experiment with such a thing. The great obstacle is that it involves such great changes in the whole financial structure that, and I am saying this from the experience of many discussions, no senior banker, who understands only the present banking system, can really conceive how such a new system would work, and he would not dare to risk and experiment with it. I think we will have to count on a few younger and more flexible brains to begin and show that such a thing can be done.

In fact, it is already being tried in a limited form. As a result of my publication I have received from all kinds of surprising quarters letters from small banking houses, telling me that they are trying to issue gold accounts or silver accounts, and that there is a considerable interest for these. I am afraid they will have to go further, for the reasons I have sketched in the beginning. In the course of such a revolution of our monetary system, the values of the precious

metals, including the value of gold, are going to fluctuate a great deal, mostly upwards, and therefore those of you who are interested in it from an investor's point of view need not fear. But those of you who are mainly interested in a good monetary system must hope that in the not too distant future we shall find generally applied another system of control over the monetary circulation, other than the redeemability in gold. The public will have to learn to select among a variety of monies, and to choose those which are good.

If we start on this soon we may indeed achieve a position in which at last capitalism is in a position to provide itself with the money it needs in order to function properly, a thing which it has always been denied. Ever since the development of capitalism it has never been allowed to produce for itself the money it needs; and if I had more time I could show you how the whole crazy structure we have as a result, this monopoly originally only of issuing gold money, is very largely the cause of the great fluctuations in credit, of the great fluctuations in economic activity, and ultimately of the recurring depressions. I think if the capitalists had been

allowed to provide themselves with the money which they need, the competitive system would have long overcome the major fluctuations in economic activity and the prolonged periods of depression. At the present moment we have of course been led by official monetary policy into a situation where it has produced so much misdirection of resources that you must not hope for a quick escape from our present difficulties, even if we adopted a new monetary system. ❖

The Pretense of Knowledge

The particular occasion of this lecture, combined with the chief practical problem which economists have to face today, have made the choice of its topic almost inevitable. On the one hand the still recent establishment of the Nobel Memorial Prize in Economic Science marks a significant step in the process by which, in the opinion of the general public,

“The Pretense of Knowledge” is Friedrich A. Hayek’s Nobel Prize Lecture delivered at the ceremony awarding him the Nobel Prize in economics in 1974 in Stockholm, Sweden. It is included here with permission of the Nobel Foundation.

economics has been conceded some of the dignity and prestige of the physical sciences. On the other hand, the economists are at this moment called upon to say how to extricate the free world from the serious threat of accelerating inflation which, it must be admitted, has been brought about by policies which the majority of economists recommended and even urged governments to pursue. We have indeed at the moment little cause for pride: as a profession we have made a mess of things.

It seems to me that this failure of the economists to guide policy more successfully is closely connected with their propensity to imitate as closely as possible the procedures of the brilliantly successful physical sciences—an attempt which in our field may lead to outright error. It is an approach which has come to be described as the “scientific” attitude—an attitude which, as I defined it some thirty years ago,

is decidedly unscientific in the true sense of the word, since it involves a mechanical and uncritical application of habits of thought to fields

different from those in which they have been formed.¹

I want today to begin by explaining how some of the gravest errors of recent economic policy are a direct consequence of this scientific error.

The theory which has been guiding monetary and financial policy during the last thirty years, and which I contend is largely the product of such a mistaken conception of the proper scientific procedure, consists in the assertion that there exists a simple positive correlation between total employment and the size of the aggregate demand for goods and services; it leads to the belief that we can permanently assure full employment by maintaining total money expenditure at an appropriate level. Among the various theories advanced to account for extensive unemployment, this is probably the only one in support of which

¹“Scientism and the Study of Society,” *Economica*, IX, no. 35 (August 1942), reprinted in *The Counter-Revolution of Science* (Glencoe, Ill.: The Free Press, 1952), p. 15.

strong quantitative evidence can be adduced. I nevertheless regard it as fundamentally false, and to act upon it, as we now experience, as very harmful.

This brings me to the crucial issue. Unlike the position that exists in the physical sciences, in economics and other disciplines that deal with essentially complex phenomena, the aspects of the events to be accounted for about which we can get quantitative data are necessarily limited and may not include the important ones. While in the physical sciences it is generally assumed, probably with good reason, that any important factor which determines the observed events will itself be directly observable and measurable, in the study of such complex phenomena as the market, which depend on the actions of many individuals, all the circumstances which will determine the outcome of a process, for reasons which I shall explain later, will hardly ever be fully known or measurable. And while in the physical sciences the investigator will be able to measure what, on the basis of a *prima facie* theory, he thinks important, in the social sciences often that is treated as important which

happens to be accessible to measurement. This is sometimes carried to the point where it is demanded that our theories must be formulated in such terms that they refer only to measurable magnitudes.

It can hardly be denied that such a demand quite arbitrarily limits the facts which are to be admitted as possible causes of the events which occur in the real world. This view, which is often quite naively accepted as required by scientific procedure, has some rather paradoxical consequences. We know: of course, with regard to the market and similar social structures, a great many facts which we cannot measure and on which indeed we have only some very imprecise and general information. And because the effects of these facts in any particular instance cannot be confirmed by quantitative evidence, they are simply disregarded by those sworn to admit only what they regard as scientific evidence: they thereupon happily proceed on the fiction that the factors which they can measure are the only ones that are relevant.

The correlation between aggregate demand and total employment, for instance,

may *only* be approximate, but as it is the only one on which we have quantitative data, it is accepted as the only causal connection that counts. On this standard there may thus well exist better “scientific” evidence for a false theory, which will be accepted because it is more “scientific,” than for a valid explanation, which is rejected because there is no sufficient quantitative evidence for it.

Let me illustrate this by a brief sketch of what I regard as the chief actual cause of extensive unemployment—an account which will also explain why such unemployment cannot be lastingly cured by the inflationary policies recommended by the now fashionable theory. This correct explanation appears to me to be the existence of discrepancies between the distribution of demand among the different goods and services and the allocation of labor and other resources among the production of those outputs. We possess a fairly good “qualitative” knowledge of the forces by which a correspondence between demand and supply in the different sectors of the economic system is brought about, of the conditions under which it will be achieved,

and of the factors likely to prevent such an adjustment. The separate steps in the account of this process rely on facts of everyday experience, and few who take the trouble to follow the argument will question the validity of the factual assumptions, or the logical correctness of the conclusions drawn from them. We have indeed good reason to believe that unemployment indicates that the structure of relative prices and wages has been distorted (usually by monopolistic or governmental price fixing), and that to restore equality between the demand and the supply of labor in all sectors changes of relative prices and some transfers of labor will be necessary.

But when we are asked for quantitative evidence for the particular structure of prices and wages that would be required in order to assure a smooth continuous sale of the products and services offered, we must admit that we have no such information. We know, in other words, the general conditions in which what we call, somewhat misleadingly, an equilibrium will establish itself: but we never know what the particular prices or wages are which would exist if the market were to bring about such an

equilibrium. We can merely say what the conditions are in which we can expect the market to establish prices and wages at which demand will equal supply. But we can never produce statistical information which would show how much the prevailing prices and wages *deviate* from those which would secure a continuous sale of the current supply of labor. Though this account of the causes of unemployment is an empirical theory, in the sense that it might be proved false, e.g., if with a constant money supply, a general increase of wages did not lead to unemployment, it is certainly not the kind of theory which we could use to obtain specific numerical predictions concerning the rates of wages, or the distribution of labor, to be expected.

Why should we, however, in economics, have to plead ignorance of the sort of facts on which, in the case of a physical theory, a scientist would certainly be expected to give precise information? It is probably not surprising that those impressed by the example of the physical sciences should find this position very unsatisfactory and should insist on the standards of proof which they find there. The reason for this state of affairs is

the fact, to which I have already briefly referred, that the social sciences, like much of biology but unlike most fields of the physical sciences, have to deal with structures of *essential* complexity, i.e., with structures whose characteristic properties can be exhibited only by models made up of relatively large numbers of variables. Competition, for instance, is a process which will produce certain results only if it proceeds among a fairly large number of acting persons.

In some fields, particularly where problems of a similar kind arise in the physical sciences, the difficulties can be overcome by using, instead of specific information about the individual elements, data about the relative frequency, or the probability, of the occurrence of the various distinctive properties of the elements. But this is true only where we have to deal with what has been called by Dr. Warren Weaver (formerly of the Rockefeller Foundation), with a distinction which ought to be much more widely understood, “phenomena of unorganized complexity,” in contrast to those “phenomena of organized complexity” with which we have to deal in the social

sciences.² Organized complexity here means that the character of the structures showing it depends not only on the properties of the individual elements of which they are composed, and the relative frequency with which they occur, but also on the manner in which the individual elements are connected with each other. In the explanation of the working of such structures we can for this reason not replace the information about the individual elements by statistical information, but require full information about each element if from our theory we are to derive specific predictions about individual events. Without such specific information about the individual elements we shall be confined to what on another occasion I have called mere pattern predictions—predictions of some of the general attributes of the structures that will form themselves, but not containing specific

²Warren Weaver, "A Quarter Century in the Natural Sciences," *The Rockefeller Foundation Annual Report 1958*, chapter I, "Science and Complexity."

statements about the individual elements of which the structures will be made up.³

This is particularly true of our theories accounting for the determination of the systems of relative prices and wages that will form themselves on a wellfunctioning market. Into the determination of these prices and wages there will enter the effects of particular information possessed by every one of the participants in the market process—a sum of facts which in their totality cannot be known to the scientific observer, or to any other single brain. It is indeed the source of the superiority of the market order, and the reason why, when it is not suppressed by the powers of government, it regularly displaces other types of order, that in the resulting allocation of resources more of the knowledge of particular facts will be utilized which exists only dispersed among

³See my essay “The Theory of Complex Phenomena” in *The Critical Approach to Science and Philosophy. Essays in Honor of K.R. Popper*, ed. M. Bunge (New York, 1964), and reprinted (with additions) in my *Studies in Philosophy, Politics and Economics* (London and Chicago, 1967).

uncounted persons, than any one person can possess. But because we, the observing scientists, can thus never know all the determinants of such an order, and in consequence also cannot know at which particular structure of prices and wages demand would everywhere equal supply, we also cannot measure the deviations from that order; nor can we statistically test our theory that it is the deviations from that "equilibrium" system of prices and wages which make it impossible to sell some of the products and services at the prices at which they are offered.

Before I continue with my immediate concern, the effects of all this on the employment policies currently pursued, allow me to define more specifically the inherent limitations of our numerical knowledge which are so often overlooked. I want to do this to avoid giving the impression that I generally reject the mathematical method in economics. I regard it in fact as the great advantage of the mathematical technique that it allows us to describe, by means of algebraic equations, the general character of a pattern even where we are ignorant of the numerical values which will determine its particular manifestation. We could scarcely

have achieved that comprehensive picture of the mutual interdependencies of the different events in a market without this algebraic technique. It has led to the illusion, however, that we can use this technique for the determination and prediction of the numerical values of those magnitudes; and this has led to a vain search for quantitative or numerical constants. This happened in spite of the fact that the modern founders of mathematical economics had no such illusions. It is true that their systems of equations describing the pattern of a market equilibrium are so framed that if we were able to fill in all the blanks of the abstract formulae, i.e., if we knew all the parameters of these equations, we could calculate the prices and quantities of all commodities and services sold. But, as Vilfredo Pareto, one of the founders of this theory, clearly stated, its purpose cannot be “to arrive at a numerical calculation of prices,” because, as he said, it would be “absurd” to assume that we could ascertain all the data.⁴ Indeed, the chief

⁴V. Pareto, *Manuel d'économie politique*, 2nd. ed. (Paris, 1927), pp. 223–24.

point was already seen by those remarkable anticipators of modern economics, the Spanish schoolmen of the sixteenth century, who emphasized that what they called *pretium mathematicum*, the mathematical price, depended on so many particular circumstances that it could never be known to man but was known only to God.⁵ I sometimes wish that our mathematical economists would take this to heart. I must confess that I still doubt whether their search for measurable magnitudes has made significant contributions to our theoretical understanding of economic phenomena—as distinct from their value as a description of particular situations. Nor am I prepared to accept the excuse that this branch of research is still very young: Sir William Petty, the founder of econometrics, was after all a somewhat senior colleague of Sir Isaac Newton in the Royal Society!

⁵See, e.g., Luis Molina, *De iustitia et iure*, Cologne 1596–1600, tom. II, disp. 347, no. 3, and particularly Johannes de Lugo, *Disputationum de iustitia et iure tomus secundus* (Lyon, 1642), disp. 26, sect. 4, no. 40.

There may be few instances in which the superstition that only measurable magnitudes can be important has done positive harm in the economic field: but the present inflation and employment problems are a very serious one. Its effect has been that what is probably the true cause of extensive unemployment has been disregarded by the scientistically minded majority of economists, because its operation could not be confirmed by directly observable relations between measurable magnitudes, and that an almost exclusive concentration on quantitatively measurable surface phenomena has produced a policy which has made matters worse.

It has, of course, to be readily admitted that the kind of theory which I regard as the true explanation of unemployment is a theory of somewhat limited content because it allows us to make only very general predictions of the kind of events which we must expect in a given situation. But the effects on policy of the more ambitious constructions have not been very fortunate and I confess that I prefer true but imperfect knowledge, even if it leaves much indetermined and unpredictable, to a pretense of

exact knowledge that is likely to be false. The credit which the apparent conformity with recognized scientific standards can gain for seemingly simple but false theories may, as the present instance shows, have grave consequences.

In fact, in the case discussed, the very measures which the dominant "macro-economic" theory has recommended as a remedy for unemployment, namely the increase of aggregate demand, have become a cause of a very extensive misallocation of resources which is likely to make later large-scale unemployment inevitable. The continuous injection of additional amounts of money at points of the economic system where it creates a temporary demand which must cease when the increase of the quantity of money stops or slows down, together with the expectation of a continuing rise of prices, draws labor and other resources into employments which can last only so long as the increase of the quantity of money continues at the same rate—or perhaps even only so long as it continues to accelerate at a given rate. What this policy has produced is not so much a level of employment that could not have been

brought about in other ways, as a distribution of employment which cannot be indefinitely maintained and which after some time can be maintained only by a rate of inflation which would rapidly lead to a disorganization of all economic activity. The fact is that by a mistaken theoretical view we have been led into a precarious position in which we cannot prevent substantial unemployment from re-appearing; not because, as this view is sometimes misrepresented, this unemployment is deliberately brought about as a means to combat inflation, but because it is now bound to occur as a deeply regrettable but inescapable consequence of the mistaken policies of the past as soon as inflation ceases to accelerate.

I must, however, now leave these problems of immediate practical importance which I have introduced chiefly as an illustration of the momentous consequences that may follow from errors concerning abstract problems of the philosophy of science. There is as much reason to be apprehensive about the long run dangers created in a much wider field by the uncritical acceptance of assertions which have the appearance of being scientific

as there is with regard to the problems I have just discussed. What I mainly wanted to bring out by the topical illustration is that certainly in my field, but I believe also generally in the sciences of man, what looks superficially like the most scientific procedure is often the most unscientific, and, beyond this, that in these fields there are definite limits to what we can expect science to achieve. This means that to entrust to science—or to deliberate control according to scientific principles—more than scientific method can achieve may have deplorable effects. The progress of the natural sciences in modern times has of course so much exceeded all expectations that any suggestion that there may be some limits to it is bound to arouse suspicion. Especially all those will resist such an insight who have hoped that our increasing power of prediction and control, generally regarded as the characteristic result of scientific advance, applied to the processes of society, would soon enable us to mould society entirely to our liking. It is indeed true that, in contrast to the exhilaration which the discoveries of the physical sciences tend to produce, the insights which we gain from the study of society more

often have a dampening effect on our aspirations; and it is perhaps not surprising that the more impetuous younger members of our profession are not always prepared to accept this. Yet the confidence in the unlimited power of science is only too often based on a false belief that the scientific method consists in the application of a ready-made technique, or in imitating the form rather than the substance of scientific procedure, as if one needed only to follow some cooking recipes to solve all social problems. It sometimes almost seems as if the techniques of science were more easily learnt than the thinking that shows us what the problems are and how to approach them.

The conflict between what in its present mood the public expects science to achieve in satisfaction of popular hopes and what is really in its power is a serious matter because, even if the true scientists should all recognize the limitations of what they can do in the field of human affairs, so long as the public expects more there will always be some who will pretend, and perhaps honestly believe, that they can do more to meet popular demands than is really in their

power. It is often difficult enough for the expert, and certainly in many instances impossible for the layman, to distinguish between legitimate and illegitimate claims advanced in the name of science. The enormous publicity recently given by the media to a report pronouncing in the name of science on *The Limits to Growth*, and the silence of the same media about the devastating criticism this report has received from the competent experts,⁶ must make one feel somewhat apprehensive about the use to which the prestige of science can be put. But it is by no means only in the field of economics that far-reaching claims are made on behalf of a more scientific direction of all human activities and the desirability of

⁶See *The Limits to Growth: A Report of the Club of Rome's Project on the Predicament of Mankind* (New York, 1972); for a systematic examination of this by a competent economist cf. Wilfred Beckerman, *In Defence of Economic Growth* (London, 1974), and, for a list of earlier criticisms by experts, Gottfried Haberler, *Economic Growth and Stability* (Los Angeles, 1974), who rightly calls their effect "devastating."

replacing spontaneous processes by “conscious human control.” If I am not mistaken, psychology, psychiatry, and some branches of sociology, not to speak about the so-called philosophy of history, are even more affected by what I have called the scientific prejudice, and by specious claims of what science can achieve.⁷

If we are to safeguard the reputation of science, and to prevent the arrogation of knowledge based on a superficial similarity of procedure with that of the physical sciences, much effort will have to be directed toward debunking such arrogations, some of which have by now become the vested interests of established university departments. We cannot be grateful enough to such modern philosophers of science as Sir Karl Popper for giving us a test by which we

⁷I have given some illustrations of these tendencies in other fields in my inaugural lecture as Visiting Professor at the University of Salzburg, *Die Irrtümer des Konstruktivismus und die Grundlagen legitimer Kritik gesellschaftlicher Gebilde* (Munich, 1970), now reissued for the Walter Eucken Institute, at Freiburg i.Br. by J.C.B. Mohr (Tübingen, 1975).

can distinguish between what we may accept as scientific and what not—a test which I am sure some doctrines now widely accepted as scientific would not pass. There are some special problems, however, in connection with those essentially complex phenomena of which social structures are so important an instance, which make me wish to restate in conclusion in more general terms the reasons why in these fields not only are there only absolute obstacles to the prediction of specific events, but why to act as if we possessed scientific knowledge enabling us to transcend them may itself become a serious obstacle to the advance of the human intellect.

The chief point we must remember is that the great and rapid advance of the physical sciences took place in fields where it proved that explanation and prediction could be based on laws which accounted for the observed phenomena as functions of comparatively few variables—either particular facts or relative frequencies of events. This may even be the ultimate reason why we single out these realms as “physical” in contrast to those more highly organized structures which I have here

called essentially complex phenomena. There is no reason why the position must be the same in the latter as in the former fields. The difficulties which we encounter in the latter are not, as one might at first suspect, difficulties about formulating theories for the explanation of the observed events—although they cause also special difficulties about testing proposed explanations and therefore about eliminating bad theories. They are due to the chief problem which arises when we apply our theories to any particular situation in the real world. A theory of essentially complex phenomena must refer to a large number of particular facts; and to derive a prediction from it, or to test it, we have to ascertain all these particular facts. Once we succeeded in this there should be no particular difficulty about deriving testable predictions—with the help of modern computers it should be easy enough to insert these data into the appropriate blanks of the theoretical formulae and to derive a prediction. The real difficulty, to the solution of which science has little to contribute, and which is sometimes indeed insoluble, consists in the ascertainment of the particular facts.

A simple example will show the nature of this difficulty. Consider some ball game played by a few people of approximately equal skill. If we knew a few particular facts in addition to our general knowledge of the ability of the individual players, such as their state of attention, their perceptions and the state of their hearts, lungs, muscles etc., at each moment of the game, we could probably predict the outcome. Indeed, if we were familiar both with the game and the teams we should probably have a fairly shrewd idea on what the outcome will depend. But we shall of course not be able to ascertain those facts and in consequence the result of the game will be outside the range of the scientifically predictable, however well we may know what effects particular events would have on the result of the game. This does not mean that we can make no predictions at all about the course of such a game. If we know the rules of the different games we shall, in watching one, very soon know which game is being played and what kinds of actions we can expect and what kind not. But our capacity to predict will be confined to such general characteristics of the events to be expected and not include

the capacity of predicting particular individual events.

This corresponds to what I have called earlier the mere pattern predictions to which we are increasingly confined as we penetrate from the realm in which relatively simple laws prevail into the range of phenomena where organized complexity rules. As we advance we find more and more frequently that we can in fact ascertain only some but not all the particular circumstances which determine the outcome of a given process; and in consequence we are able to predict only some but not all the properties of the result we have to expect. Often all that we shall be able to predict will be some abstract characteristic of the pattern that will appear—relations between kinds of elements about which individually we know very little. Yet, as I am anxious to repeat, we will still achieve predictions which can be falsified and which therefore are of empirical significance.

Of course, compared with the precise predictions we have learnt to expect in the physical sciences, this sort of mere pattern predictions is a second best with which one does not like to have to be content. Yet the

danger of which I want to warn is precisely the belief that in order to have a claim to be accepted as scientific it is necessary to achieve more. This way lies charlatanism and worse. To act on the belief that we possess the knowledge and the power which enable us to shape the processes of society entirely to our liking, knowledge which in fact we do not possess, is likely to make us do much harm. In the physical sciences there may be little objection to trying to do the impossible; one might even feel that one ought not to discourage the over-confident because their experiments may after all produce some new insights. But in the social field the erroneous belief that the exercise of some power would have beneficial consequences is likely to lead to a new power to coerce other men being conferred on some authority. Even if such power is not in itself bad, its exercise is likely to impede the functioning of those spontaneous ordering forces by which, without understanding them, man is in fact so largely assisted in the pursuit of his aims. We are only beginning to understand on how subtle a communication system the functioning of an advanced industrial society is based—a communications

system which we call the market and which turns out to be a more efficient mechanism for digesting dispersed information than any that man has deliberately designed.

If man is not to do more harm than good in his efforts to improve the social order, he will have to learn that in this, as in all other fields where essential complexity of an organized kind prevails, he cannot acquire the full knowledge which would make mastery of the events possible. He will therefore have to use what knowledge he can achieve, not to shape the results as the craftsman shapes his handiwork, but rather to cultivate a growth by providing the appropriate environment, in the manner in which the gardener does this for his plants. There is danger in the exuberant feeling of ever growing power which the advance of the physical sciences has engendered and which tempts man to try, "dizzy with success," to use a characteristic phrase of early communism, to subject not only our natural but also our human environment to the control of a human will. The recognition of the insuperable limits to his knowledge ought indeed to teach the student of society a lesson of humility which should guard him against

becoming an accomplice in men's fatal striving to control society—a striving which makes him not only a tyrant over his fellows, but which may well make him the destroyer of a civilization which no brain has designed but which has grown from the free efforts of millions of individuals. ❖