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# A Discussion With **FRIEDRICH VON HAYEK**



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# **A Discussion With FRIEDRICH A. VON HAYEK**

Held at  
the American Enterprise Institute  
on April 9, 1975

American Enterprise Institute for Public Policy Research  
Washington, D.C.

Friedrich A. von Hayek is professor of economics emeritus at the University of Salzburg, Austria and received the Nobel Memorial Prize in Economic Science in 1974.

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# **CONTENTS**

|                                                  |           |
|--------------------------------------------------|-----------|
| <b>INTRODUCTION BY GOTTFRIED HABERLER</b>        | <b>1</b>  |
| <b>REMARKS BY<br/>FRIEDRICH AUGUST VON HAYEK</b> | <b>2</b>  |
| <b>QUESTIONS AND ANSWERS</b>                     | <b>11</b> |

## A Discussion With FRIEDRICH A. VON HAYEK

### INTRODUCTION BY GOTTFRIED HABERLER

Professor Hayek is best known as the author of *The Road to Serfdom* and *The Constitution of Liberty*. But these two books are only tiny tips of a huge iceberg.

In his evaluation of Hayek's work for the Nobel Prize Committee of the Swedish Academy, Professor Machlup lists fifteen books, ten other books edited and introduced, ten pamphlets, and more than 130 articles, not counting translations and revised editions. Professor Hayek's writings cover not only all the major areas of economics, but also social philosophy, political philosophy, legal philosophy, philosophy of science, psychology, and intellectual history.

I can do no better than to read a few sentences from the official statement of the Swedish Royal Academy of Science, announcing the award of the Nobel Memorial Prize in Economic Science, 1974:

Professor Hayek's contributions in the field of economic theory are both profound and original. . . . He tried to penetrate more deeply into the business-cycle mechanism than was usual at that time. Perhaps partly due to this more profound analysis, he was one of the few economists who warned about the possibility of the major economic crisis before the great crash came in the autumn of 1929. . . . The Academy is of the opinion that von Hayek's analysis of the functional efficiency of different economic systems is one of his most significant contributions to economic research in the broader sense. . . . As in all areas where von Hayek has

carried out research, he gave a profound historical exposé of the history of doctrines and opinions in this field. . . . His guiding principle when comparing various systems was to study how efficiently all the knowledge and information dispersed among individuals and enterprises is utilized. He concluded that only through far-reaching decentralization in a market system with competition and free price-fixing would it be possible to make full use of knowledge and information.

Today Professor Hayek is going to speak about inflation and recession, a subject in which he has been interested for more than fifty years. When he came to the United States for the first time in the early 1920s, it was to study U.S. monetary policies. After he returned to Vienna and became the first director of the Austrian Institute for Business Cycle Research, he kept abreast of developments in the United States. It was in the monthly report of the Business Cycle Institute, I think in February 1929, that he boldly predicted the imminence of a crisis and a business cycle downturn in the United States.

Now let me remind you, the 1920s were years of stable prices. After the sharp depression of 1920–21, the price level in the United States remained fairly stable until 1929. That was generally regarded as a sign that the business cycle had been vanquished. Professor Hayek's prediction thus ran counter to the views prevailing at that time.

The present situation reminds some people ominously of what happened in the 1920s. Especially worrisome are the combination of inflation and unemployment—a new phenomenon—and the sudden intensification of the recession in the last quarter of 1974. The recession had started earlier, but the sudden downturn took most people by surprise. Today many fear, rightly or wrongly, that the recession, the sharpest we've had in the postwar period, may actually turn into a full-blown depression.

We are very anxious to hear what Professor Hayek has to say about that problem.

## REMARKS BY FRIEDRICH AUGUST VON HAYEK

Professor Haberler, ladies and gentlemen: From the scientific point of view, there are many more interesting problems than inflation, and many to which I would rather devote my time.

But at the present moment I regard it as the prime duty of an economist to devote at least a large part of his energies to persuading the public of one essential truth which people will have to understand before we can hope for a more sensible policy. And that basic truth, as I see it, is simply that the present unemployment is a direct and inevitable consequence of the so-called full employment policy we have been pursuing for the last twenty-five or thirty years.

Nothing short of this insight—that, by creating employment through inflation, we lay the foundations for a future period of worse unemployment—will prevent the public from pressing for more inflation as soon as unemployment becomes alarming. Until we succeed in altering public opinion on this point, the outlook for the future seems to me pretty hopeless.

I don't think there is much hope for the near future in any case. The probable course of events is that inflation will be resumed as supposedly necessary to restore employment, that the intolerable effects of such inflation—which will come even if it does not succeed in removing unemployment—will lead to price controls, and that in this way the market system will be destroyed for the time being.

To assert publicly, as I have done, that the cause of the present unemployment is the full employment policy of the past, implies, of course, the assertion that the great majority of our fellow economists, and particularly those to whom governments have, in fact, been listening during this period, have been wrong, have thoroughly discredited themselves and, I would add, have forfeited their right to be heard. It is a regrettable but undeniable fact that the present economic crisis represents also a very serious crisis in the science of economics: it is we, or at least the great majority of our colleagues, who have made a mess of things; and the best an economist can do is to contribute to the long overdue collapse of the Keynesian bubble, of that fashionable body of opinion which has been dominating economics for the last thirty years.

Before we can return to a reasonable stability and perhaps lasting prosperity, I am convinced that we must exorcise this Keynesian devil—by which I mean not so much the teachings of Lord Keynes himself as those of some Keynesians. Lady Robinson, one of the most prominent among them, has quite recently told us that they “sometimes had some trouble in getting Maynard to see what the point of his revolution really was.”<sup>1</sup>

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<sup>1</sup> Joan Robinson, “What Has Become of the Keynesian Revolution?” in *Essays on John Maynard Keynes*, Milo Keynes, ed. (Cambridge 1973), p. 125.

This is, of course, a very big subject, and I must confine myself to two or three main points. I shall begin by stating the essential element of the Keynesian theory which I think to be wrong. I shall then turn to the question of how it came about that a theory which all economists—or almost all reputable economists—would have regarded as highly misleading forty years ago, could become generally accepted and remain so for such a long period. Finally, I shall ask what the abandonment of this particular technique of securing full employment implies.

First, the basic contention, which I believe to be wrong and from which we must free ourselves, is the belief that employment is a direct and simple function of what is called aggregate demand, and that by keeping aggregate demand at a sufficiently high level we can lastingly secure full employment.

This view—which, as Keynes knew very well, had for generations been held by economic cranks but had been rejected by economists—came to be accepted by economists for two reasons. Or perhaps I should have said three reasons, for the silver voice of that genius in persuasion, Lord Keynes, contributed a good deal to the result. Lord Keynes was exceedingly difficult to resist in conversation or discussion. Even if you knew that he was wrong, you sometimes found it extraordinarily hard to maintain your position while you talked to him—although once you turned away, you realized that you had been misled.

But I will not talk about the personal side. Rather I want to examine the basic validity of the central contention.

Let me say, first, that there are two circumstances in which changes in aggregate demand are indeed the dominating factor in determining the level of unemployment; and these two circumstances have governed the development of the theory.

The first one was an accidental historic situation—but an historic situation that determined the climate of opinion in the country which then dominated economic theory. In 1925, Great Britain had made a laudable attempt to return to gold but mistakenly to do so at the former parity. This policy created a situation where real wages were *generally* too high because they had been artificially raised by the revaluation of the pound. In consequence, British industry, largely dependent on exports, had become unable to compete in the world market. In this situation, the restoration of employment required a reduction of real wages which could be achieved by a general rise of prices.

This particular situation, however, while it largely explains the growth of Keynes's own views, would not be sufficient to explain their wide acceptance.

The second situation in which it is true that an increase of employment requires an increase in aggregate demand is found in the later stages of a depression when, in consequence of the appearance of extensive unemployment, the economy frequently is subjected to a cumulative process of contraction. The original substantial unemployment leads to a shrinkage of demand that causes more unemployment, and so on; it releases a deflation due to the "inherent instability of credit" (to use the terminology of a once very influential but now undeservedly almost forgotten economist who died a few days ago, R. G. Hawtrey).

Once you have the kind of situation in which there already exists extensive unemployment, there is thus a tendency to induce a cumulative process of secondary deflation, which may go on for a very long time. I am the last to deny—or rather, I am today the last to deny—that, in these circumstances, monetary counteractions, deliberate attempts to maintain the money stream, are appropriate.

I probably ought to add a word of explanation: I have to admit that I took a different attitude forty years ago, at the beginning of the Great Depression. At that time I believed that a process of deflation of some short duration might break the rigidity of wages which I thought was incompatible with a functioning economy. Perhaps I should even then have understood that this possibility no longer existed. I think it disappeared in 1931 when the British government abandoned its attempt to bring wages down by deflation, just when it seemed about to succeed. After that attempt had been abandoned, there was no hope that it would ever again be possible to break the rigidity of wages in that way.

I still believe that we shall not get a functioning economy until wages again become flexible, but I think that we shall have to find different techniques for that purpose. I would no longer maintain, as I did in the early '30s, that for this reason, and for this reason only, a short period of deflation might be desirable. Today I believe that deflation has no recognizable function whatever, and that there is no justification for supporting or permitting a process of deflation.

So much for the first reason for the widespread acceptance of a direct relation between aggregate demand and unemployment: it is indeed the dominating phenomenon in the later stages of a depression.

But there is another reason which, I believe, contributed a great deal to the acceptance of this, in my opinion, rather crude and over-

simplified belief in a direct functional connection between total or aggregate demand and the volume of employment.

This curious reason is the result of the dominating fashion of scientific method: There is undoubtedly some connection, some correlation, between the two factors. It is a very rough one; but if we look at the statistics we find not only that, very roughly, aggregate demand and employment move in fairly close correspondence, but also that this happens to be the only relevant regularity which can be statistically confirmed.

The alternative explanation of extensive unemployment—which, until the middle of the '30s, was fairly widely accepted and which, I believe, is still the true and correct one—has the unfortunate property of not being verifiable by statistical methods. To an economist today, however, only that is true which can be proved *statistically*, and everything that cannot be demonstrated by statistics can be neglected; hence, the true theory has been disregarded.

You may be puzzled by the assertion that there should be a true theory which cannot be statistically confirmed. The explanation is somewhat complex and I can indicate it only very briefly. I have made the subject the main content of my Nobel Memorial lecture, which I delivered in Stockholm four months ago, but I will try to put it concisely. It is a very good illustration of a more general phenomenon, namely, that with modern scientific prejudices about what is to be accepted a valid argument, it can happen that a false theory is regarded as true because there is some statistical evidence in its favor, and that the true theory is rejected because, by its very nature, it cannot be supported by statistical evidence—which is the only kind of observation which counts for that point of view.

That's a point on which you probably have some doubts. Can there be a theory the conclusion of which cannot, by its nature, be statistically supported? I believe I can give you, in this case, an example which, to me, is fairly convincing.

What was the traditional pre-Keynesian view about the causes of extensive unemployment? Generally speaking, it was the assumption of a discrepancy between the distribution of demand among different industries and the distribution of labor and other resources among these industries. As the result of that discrepancy, there will be a lack of correspondence between demand and supply in many sectors, an insufficient demand in some of them and an excessive demand in others; and, as always happens in the case of a discrepancy between demand and supply, resources of all kinds will be idle.

These discrepancies of demand and supply in different industries, discrepancies between the distribution of demand and the allocation of the factors of production, are in the last analysis due to some distortion in the price system that has directed resources to false uses. It can be corrected only by making sure, first, that prices achieve what, somewhat misleadingly, we call an equilibrium structure, and second, that labor is reallocated according to these new prices.

Lacking such price readjustment and resource reallocation, the original unemployment may then spread by means of the mechanism I have discussed before, the "secondary contraction," as I used to call it. In this way, unemployment may eventually become general.

The primary cause of the appearance of extensive unemployment, however, is a deviation of the actual structure of prices and wages from its equilibrium structure. Remember, please: that is the crucial concept.

The point I want to make is that this equilibrium structure of prices is something which we cannot know beforehand because the only way to discover it is to give the market free play; by definition, therefore, the divergence of actual prices from the equilibrium structure is something that can never be statistically measured.

The theory which asserts that unemployment is an effect of a deviation of the actual price structure from the equilibrium structure is thus a theory that cannot be confirmed by statistics. It's the kind of theory which I believe you find in many other fields of economics. What we can confirm from daily observation are the elements from which a theory is built up, our knowledge of the behavior of individuals in various situations. But we cannot test statistically the resulting conclusions, which are derived from these empirical data about individual behavior.

In contrast, the modern fashion demands that a theoretical assertion which cannot be statistically tested must not be taken seriously and has to be discarded. As a result of this belief, a theory which, in my opinion, is the true explanation has been discarded as not adequately confirmed, and a false theory has been generally accepted merely because it happens to be the only one for which statistical evidence, even though very inadequate evidence, is available.

Viewed as part of our intellectual history, this is a very interesting case; but unfortunately it has had very painful and unpleasant consequences. I like to discuss it, not merely because it's so important in itself, but because it shows more generally how important our views about the appropriate scientific procedure can be for the practical application of economics.

It's quite possible to ruin an economic system because economists have wrong ideas about the proper way of approaching scientific problems: such ideas may induce them, as in this case, to discard as inapplicable an intellectually very satisfying theory and to adopt an inadequate one.

This is really the main point I wanted to make—though it should be merely an introduction to a more detailed discussion. But I must go on to the second subject of my talk, namely, what conclusions should we draw from our insight, what ought to be our policy in the future?

At least in these introductory remarks, I do not wish to discuss the question which Professor Haberler suggested as my main problem, that is, what to do in the *immediate* future. It would be inappropriate for me to discuss that question within a week after my arrival in this country; and in fact, I haven't a great deal to suggest for the immediate future.

For forty years I have preached that the time to prevent a depression is during the preceding boom; and that, once a depression has started, there is little one can do about it. My advice was completely disregarded as long as the boom lasted. Now suddenly, when my prediction has come true and we have reached the stage where, in my opinion, little can be done about the inevitable reaction which has set in, people suddenly turn to me and ask for my opinion. I am very much tempted to answer, "Well, if you had listened to me before, you wouldn't be in that mess." Of course, I do not mean you—I mean the public in general.

What I want to discuss is policy in the long run—by which I mean not only the very long run in the Marshallian sense, but policy over the next few years. What we should absolutely avoid is any attempt to recreate employment, or diminish unemployment, by a further dose of inflation.

I will confess that I do not know whether, at this moment, even a strong additional dose of inflation would still be effective. I expect that it will be attempted, and I rather hope that it will not succeed and that we shall be forced to turn to the fundamental problem of the readjustment of the structure of production.

But the main point is: what can we do to avoid the same sort of mistakes in the future?

The public, having so long been taught false doctrines, is still convinced that the government has it in its power substantially to reduce or perhaps, in the short run, completely to abolish unemployment by such tricks as deficit spending, increasing the quantity of

money, and so on. Is there any possibility of preventing the government, even if it should wish to act more sensibly, from being forced by public opinion into repeating its mistakes and being driven to more and more inflation?

This leads me to a point where I am afraid I have persistently disagreed with many of my closest friends and associates. I believe that if we want to prevent the government from giving in to public pressure for quick and rapidly effective measures, we must put fetters on what the government can do and restore several institutions which were designed to prevent the government from abusing its powers, and particularly its powers to inflate.

In fact, the long period of accelerating inflation we have behind us has been very largely the effect of the removal, one-by-one, of those checks on undue credit expansion which the practical wisdom of the past had erected to prevent governments and monetary authorities from inflating. The abandonment of the gold standard was largely motivated by the wish to give governments greater powers of expansion. The Bretton Woods agreement, with its endeavor to put the burden of adjustment on the creditor countries and to relieve the debtor countries of the need to contract, was another step in that direction. The various decisions that provided for additional international liquidity at a time when it was already clear that the process of inflation had started, were further milestones on that road. And finally, the abandonment of fixed rates of exchange, which were the last obstacle preventing the central banks from expanding without limits, has made it possible to continue inflation indefinitely.

In a way, I regard our experience of the last twenty-five years as a large-scale repetition of the periodically recurring credit expansions which we used to call the business cycle. We have made it possible for that process of credit expansion to go on for twenty-five years by removing those checks which in the past used to terminate these periods of expansion after a few years.

The regularity of such early termination of the process of credit expansion during the hundred years preceding 1931 was of course largely due to the fact that the gold standard, in time, braked the expansion. The gold standard was the first obstacle we removed. Later on, we removed all the others. So that now we have a system under which the politician is powerless to resist the pressure of those who know—and, in most instances, know rightly—that the government has the means in the short run, for a short period, to reduce unemployment or relieve them of their momentary difficulties. A politician can resist such pressure only if he can refer to some unsur-

mountable obstacle which prevents him from giving in. As long as he has the power to satisfy those demands, he will be driven to using that power.

I have always maintained this position against those of my friends who were converted to a system of flexible exchange rates on the ground that it gave more scope to a sensible policy. This reason would be perfectly valid if those who conducted the policy, once they were relieved of the institutional obstacles, were able to act according to rational considerations.

The reason becomes invalid, however, when you realize that politicians are under constant pressure for inflation and are able to resist this pressure only by pointing out the obstacles which prevent them from doing what they are asked to do. All these obstacles have been removed, on the advice of economists, and we have been placed in a position where the politicians—or those responsible for monetary policy—have necessarily been driven into an accelerating inflation. There are no longer any institutional obstacles to which they could point and say, “I cannot do this,” and no political group or party that wants immediate relief will listen to the consideration that any short-run gain will have to be paid for by even greater suffering in a year, or in two, or perhaps in five.

I therefore believe that if we are to hope for a more stable and lasting prosperity, we will have to return to a more automatic system, something rather like the old gold standard, like fixed rates of exchange, and so on.

I wish I could, at this point, go along with my friend, Milton Friedman, who believes he can solve the problem by fixing the annual rate of increase of M1 or M2 or M3 so that the discretion of the monetary authorities would be strictly limited. I believe that, in this case, a very eminent scholar has again been misled by the preoccupation with statistics, that is, by the fact that, for statistical reasons, he has to draw a sharp line of demarcation between what he calls money and what he calls near-money or credit.

If such a clear line could be drawn, something might be said for strictly limiting what would be money under that definition.

It is one of the elementary facts of life, however, a fact which a statistician has to disregard but which the practical banker must heed, that the transition from money proper to all kinds of money substitutes is gradual and continuous without any sharp lines of demarcation, and that, in order to maintain a solvent credit system, we must assure the constant convertibility of all kinds of money substitutes into money proper. Any attempt to limit rigidly some

part of this structure could, sooner or later, lead to a rather catastrophic monetary panic. For that reason—with great regret, since I am much attracted by the sort of scheme Milton Friedman has proposed—I do not believe that his proposal can solve the problem. But I agree with the general aim of trying to go back to a more automatic system and, above all, to a system which imposes definite restrictions upon the monetary authorities and enables them to say to anybody who brings pressure upon them: we can't expand further, we are prevented by our institutions from doing so.

I do not know whether we shall be able, in the foreseeable future, to restore a system, such as the gold standard, which, although not foolproof, is at least in some respect protected against the pressure of fools. But it certainly is necessary to make people understand that the now fashionable method of securing employment by monetary and credit management is fundamentally false, and that so long as we attempt to do so, we just repeat, on a greatly enlarged scale, the kind of fluctuations we used to call business cycles. These fluctuations were unpleasant enough when we allowed the misdirection of resources by excessive credit expansion to go on for two or three years. I am afraid they will prove to be even more unpleasant after we have allowed this misdirection of resources to go on for twenty-five years.

Let me emphasize, in conclusion, that I am not arguing that another Great Depression of the type of the 1930s is inevitable. Even then, such a long-lasting and severe depression was not necessary, but was largely due to the foolishness of the policies pursued. We need not move into an equally serious and general depression if we avoid equally foolish policies—which included, in my opinion, the attempts made after 1929 to maintain demand and maintain wages, policies initiated by President Hoover and then expanded by President Roosevelt. But notice the proviso: if we do not make mistakes as bad as those we made in the early '30s. I shall leave it to you to judge what chances we have of avoiding such mistakes.

Thank you. [Applause.]

## QUESTIONS AND ANSWERS

DR. HABERLER: Thank you very much for your provocative talk. I am very glad to see that your thoughts are still contrary to much of the modern conventional wisdom.

Professor Hayek is ready to answer questions and respond to criticism. To start the discussion, let me ask one question or two.

I was very glad you said that you find some justification in the view that depressions are aggravated by a cumulative spiral and that there is such a thing as a secondary deflation. Don't you think that it is possible to do something about that aggravation without recreating the fundamental maladjustments which, in your opinion, caused the depression?

PROFESSOR HAYEK: I hope I implied this. The moment there is any sign that the total income stream may actually shrink, I should certainly not only try everything in my power to prevent it from dwindling, but I should announce beforehand that I would do so in the event the problem arose.

I am only a little upset by the arguments used now: at a time when the quantity of money is still lustily increasing, people already are talking about combatting deflation although there is nothing like a deflation going on. The time for action will have come once deflation starts.

DR. HABERLER: Yes, the present situation is complicated by the fact that we have both inflation and unemployment. But going back to the 1930s when prices were falling rapidly, would you say now that at that time much could have been done to stop the spiral?

PROFESSOR HAYEK: You see, even at that time, I did say so. I will tell you of an episode that may be significant. In 1929, or perhaps 1930, when the depression was beginning to get quite serious on the European continent, a German political commission—the Braun Committee—proposed to combat it by reflation (though that term had not yet been coined), by rapid credit expansion. One of the members, in fact the main author of the report, was my late friend, Professor Wilhelm Roepke. I thought that in the circumstance the proposal was wrong, and I wrote an article against it. I did not publish the paper, however, but sent it to Roepke with a covering letter in which I made the following point:

Apart from political considerations, I think you should not—not yet at least—start expanding credit. But if the political situation is so serious that continuing unemployment would lead to a political revolution, please, do not publish my article. That is a political consideration, however, which I cannot judge from outside Germany, but which you will be able to judge.

Roepke's reaction was not to publish the article, because he was convinced that at that time the political danger of increasing unemployment was so great that he would rather risk the danger of causing further misdirections by more inflation in the hope of post-

poning the crisis; at that particular moment, such postponement seemed to him politically necessary.

I have never denied that one can, in the short run, reduce unemployment in that fashion. All I am arguing is that in the long run you do more harm than good by inflation, and unless the circumstances of the moment threaten greater dangers, I would not inflate.

I realize this is not exactly the answer to your question. You ask whether I have changed my opinion about combatting secondary deflation. I do not have to change my theoretical views. As I explained before, I have always thought that deflation had no economic function; but I did once believe, and no longer do, that it was desirable because it could break the growing rigidity of wage rates. Even at that time I regarded this view as a political consideration; I did not think that deflation improved the adjustment mechanism of the market.

SAMUEL A. MITCHELL, Research from Washington, Inc.: Once the government nurtures the illusion that the public is entitled to uninterrupted real increases in purchasing power, how can it possibly take the proper measures, or refrain from making mistakes, without running the risk of major social instability? Isn't a more likely alternative an effort to use the state's coercive powers which, perhaps, could be a prelude to a much more authoritarian environment and general loss of liberty?

PROFESSOR HAYEK: I don't think there is any way to avoid the risk of major social instability in the present situation. I don't feel competent to answer the question of what degree of social unrest the available alternative courses will produce. All I can say is that, as I see it, only a course which aims at restoring the market, which involves avoiding further inflation, even at the price of temporary though very substantial unemployment, will make it possible to preserve our fundamental political and cultural institutions, democracy and personal liberty. Any other course, either letting inflation continue or deliberately resuming it as a means of combatting unemployment, or increasing the quantity of money but clamping on price controls, leads inevitably to a planned economy, a centrally directed economy, involving the abolition of free institutions.

I think that there are only three possibilities at this moment. The first would be a return to stable money, which is not possible without passing through a period of very substantial and fairly prolonged unemployment.

The second alternative is to go on expanding rapidly enough to create a tolerable level of employment. That means accelerating the

rate of inflation and—perhaps I ought to have explained this earlier, but I won't stop to consider it now—accelerating inflation cannot go on indefinitely. Sooner or later, it causes such a disorganization of the whole economic system that it ceases to stimulate employment.

The third possibility—unfortunately, the most likely to be chosen—is to go on expanding while trying to combat the effect on prices by controls. That is a direct path to a centrally planned economy, and that is the one we are most likely to go.

DUDLEY DILLARD, University of Maryland: I would like to ask a question about scientific method, about the theory that a theory can be true although its truth can't be tested statistically. I am familiar with your economic work, but not with your methodological and philosophical work. But my question is, why should anyone accept such a theory as true? Perhaps you could explain how you would demonstrate, by other than statistical methods, why such a theory is true.

PROFESSOR HAYEK: I think the answer is fairly simple. If we take as premises some undisputed facts, which everybody accepts as facts of daily observation, we can logically deduce from them certain consequences, which permit only one answer to the problem. In other words, if we deduce certain consequences from admitted facts, by logically correct argument, the truth of our deductions has to be accepted. You might object that I have left out some facts, and that the result would have been different if I had not neglected those other facts. Well, my answer to this objection would be: quote the facts, please, and I shall be quite willing to consider them. We have been talking about this problem for a hundred years, and nobody has yet mentioned any generally admitted facts which would essentially modify my conclusions.

As a result of the peculiar position of the natural sciences, which deal with relatively simple phenomena, the doctrine has arisen that it is always the predicted result which must be directly confirmed by empirical evidence. But we can also arrive at new results from premises which we can confirm empirically; if these premises are accepted and the logic of our deductions is accepted, I think we can be fairly confident that the result will be true of the world in which we live.

Experience may, of course, contradict such a result; that is, such a theory, like all theories, can be disproved, and we will accept it only as long as it is not being disproved. But on the whole I think we have been trying unsuccessfully to disprove the theory for a very long time. And I am inclined to regard the recent experience as an even better confirmation than any past one.

CHIAKI NISHIYAMA, Rikkyo University: I earned my Ph.D. from Professor Hayek, but at the same time I joined in the workshop of Milton Friedman, and I am here really in a difficult position: I have to defend Milton Friedman against my mentor.

In Japan, we have constructed ten different M series, covering the 100 years of Japanese monetary history, and we have found that there is, indeed, a close correlation among all those different series. Admittedly, this is not sufficient evidence for the assertion that M2 or M3 could substitute for some weighted average of all kinds of money. But I'd like to present still another piece of evidence for Milton Friedman's assertion that we can optimize the money supply by controlling M2 or M3. This evidence, too, comes from Japan's experience.

From 1965 on through 1970, the Japanese monetary authority maintained the rise in the money supply every year at 16 percent; in this period we had price stability and yet a very rapid rate of economic growth. Reacting to the "Nixon Shock" of August 1971, however, the monetary authority let the rate of money supply suddenly increase from 16 percent to 30 percent. Then, realizing that it had made a mistake, it began to reduce the rate of money supply growth as early as January 1973, and continued to do so, in a very gradual way, for twenty-four months. The rate of increase was brought down from 30 percent to the original level of 16 percent at the end of 1973, and to 10 percent in September 1974.

Our rate of inflation was tremendous in the first and second quarter of last year: the CPI went up 24.5 percent, the WPI 36 percent. But those price indices have now come down: the rise of the WPI to 4.9 percent a year, and that of the CPI to 13 percent. In contrast, when the U.S. authorities tried to change the rate of money supply, what they did was reduce it from 6 percent to zero for three months, then shoot it up to 10 percent, and then reduce it once again to zero six months later.

I wonder if this is what we monetarists are arguing for. When we say that the rate of money supply is important, we're not saying that we should optimize it all at once, at all costs; rather I would say that, if we can regard the case of Japan as a success, the reason is that we have done it gradually, over a period of twenty-four months.

But I have to admit that while we succeeded in controlling inflation, we created a large amount of unemployment. And that is a problem which cannot be solved solely by sticking to monetarism.

Professor Hayek is absolutely right when he says that we have to do something else. What we did in Japan was to try to convince

labor that it was in its own interest not to raise nominal wage rates but rather that reducing nominal wage rates would optimize their real income. Apparently we have succeeded in this effort. Of course, we should also, as Professor Hayek has emphasized, try to optimize the price structure, perhaps by trying to reduce the rate of monopoly or oligopoly or other organizational rigidities. Therefore, my final conclusion is more in line with your position, and in this way I try to synthesize your position with Professor Friedman's.

PROFESSOR HAYEK: I really do not disagree with anything you said, and I hardly disagree from the *theoretical* argument of Milton Friedman—there are only minor differences. But I do disagree on a practical point.

I am all in favor of the monetary authorities' aiming at keeping the increase in the quantity of money at a constant rate, as Friedman proposes. What I oppose is that this aim should be legally imposed upon them. I have to say this because I explicitly favor imposing some external restraints upon the monetary authorities.

I would not use the legal restraint suggested by Friedman because it would have certain dangerous consequences. In 1844, the British attempted to place a fixed limit on the circulation of bank notes. They were forced to circumvent this restriction by dispensing the Bank of England in the three succeeding crises from the obligation to keep the circulation within that limit. Nevertheless, the expectation that the Bank of England would not be able to provide the liquidity which was needed in such a crisis was one of the causes of the panics. I believe the Friedman proposal would lead to the same situation. If the public felt that the demand for liquidity exceeded the amount the law permitted the central bank to provide, we would again get panics of this sort. And for this reason, regretfully, I conclude that we must leave a certain amount of elasticity and discretion to the monetary authorities.

PROFESSOR JÜRGEN NIEHANS, Johns Hopkins University: I have a question on exactly this point. I am not a defender of the 4 percent rule. Still your main point of criticism was somewhat puzzling to me, and maybe you could explain it.

You said it is one of the major duties of the monetary authorities to see to it that the various types of money and near monies are perfect substitutes, and you criticized the 4 percent rule because you believe that, under such a rule, this obligation could not be discharged and that this situation would lead to disturbances, crises, and so on. On the other hand, you are for a gold standard. Now, in what respect

would the gold standard, where gold takes the place of the monetary base in some sense, be more elastic, and how could the monetary authorities discharge their obligations better under a gold standard than under the 4 percent rule? If I had to make a choice, I would rather have a rigid, predictable 4 percent rule than a quantity of gold which could not be exactly predicted, and the rigidity would not be worse in the first case than in the second.

PROFESSOR HAYEK: First, let me consider a terminological point which I must clear up to make sure that we do not misunderstand each other. You used the expression "perfect substitutes" with regard to different kinds of money. They cannot be perfect substitutes, and I am not suggesting that they should be made perfect substitutes. Just because they are not perfect substitutes, it's necessary to make the various kinds of money freely exchangeable against each other; at certain times people are very anxious to exchange one kind for another.

Now, it is quite true that, under a gold standard, the most universal liquid element is gold and that you cannot, in the short run, increase the quantity of gold. But you can increase the quantity of gold in the hands of the public. That is the purpose for which the central bank holds reserves, and the answer is that if it has enough reserves and is able to give the public gold when it wants, it will avert that sort of panic.

PROFESSOR EGON SOHMEN, University of Heidelberg: I think, Professor Hayek, that in your major diagnosis of the reasons for recessions, you may perhaps not be so far off from the emerging professional opinion in this field, the modern conventional wisdom. I was very pleased to see recently a book on the microeconomic foundations of macroeconomic policy,<sup>2</sup> and I think this is precisely what you are driving at. You stress the microeconomic reasons for maladjustment and recession. I think almost any economist agrees that rigid wages can be a cause of unemployment. The only trouble is, of course, that it is politically very difficult to do something about it. I would, therefore, like to hear what you would propose to do about that problem.

But I would not stop there. There are occasions when a major industry utilizes only about two-thirds of its capacity, but at the same time its prices are held rigidly constant or even increasing. Now I was a little bit surprised that in *The Constitution of Liberty*, and

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<sup>2</sup> E. Phelps, ed., *Micro-Economic Foundations of Employment and Inflation Theory* (New York, 1970).

elsewhere, you have downgraded the importance of antitrust policy. Such policy would seem to me to be one of the possible devices for moving against price stickiness in the field of commodity markets—and I think commodity markets are just as important as labor markets.

Finally, if central banks exercise price controls in dealing with foreign exchange, major disequilibria can develop. People may not want to hold certain currencies, and governments are then obliged to clamp on controls in order to prevent people from freely exchanging different currencies against each other. If this goes on for several years, I think it would be a clear indication that there is a disequilibrium, that a certain price is out of equilibrium and ought to be changed.

It would seem to me that in line with your general position you should be in favor of very strong antitrust policies to correct the second type of disequilibrium, and you should also be in favor of flexible exchange rates in order to correct the third type of disequilibrium.

PROFESSOR HAYEK: I should be very sorry if I gave the impression in *The Constitution of Liberty* that I was against a policy which aims at restoring competition. But that doesn't mean that I must approve of antitrust policy as it is actually pursued.

I don't believe that either the attempt to fight bigness as such, nor the various efforts to aim at a competitive price as if competition existed in fields where it cannot exist, can have the desired results. I think there are other possible ways which avoid in particular the very harmful discretionary powers actually conferred on the enforcing authorities.

What I would do is to expand greatly an until recently little used clause of the antitrust laws about triple damages to people whose freedom of action has been infringed, and to leave it to the potential competitors to act against attempts at restricting competition. I would also favor very extensive alterations in private law to make the prospects of competition more effective. But I have very grave doubts about what is actually being done, and generally about what can be done by an authority that has been given discretionary powers.

ILSE MINTZ, National Bureau of Economic Research: If I understood you correctly, your theory is that unemployment arises because of structural disequilibria, in contrast to the view that it is the consequence of insufficient overall demand.

Now, if this theory is correct, would it not follow that job vacancies in some industries should be roughly equal to the size of

unemployment in other industries? In other words, should we not find some industries that have a shortage of labor and other resources, and other industries that have a surplus, that is, unemployment? If this conclusion were correct, then we would also have an empirical test for that theory because we could look at the number of vacancies, compare it with the number of unemployed, and find out how this ratio varies over time.

PROFESSOR HAYEK: Unfortunately, surplus and shortage of labor are not symmetrical, because, if there exists an excess of labor, wages do not fall, and if there is a shortage of labor, wages rise. In consequence, where there exists a tendency to shortage, equality between demand and supply is at once established by the rise of wages; but where there is an excess of labor, wages do not fall to establish a similar equilibrium. The two situations are not "on all fours" because, as we all know, wages are only rigid downwards, but by no means rigid upwards.

DR. HABERLER: May I pursue for a moment that question, which I thought was a very good one. I agree with you that there is an asymmetry: there may be no vacancies although there is unemployment. But you went further. You said that it is empirically impossible to measure this discrepancy between the structure of demand and the structure of the supply.

Now, if you say it is very hard to foresee it "ex ante," I can see that. But once the situation has arisen, isn't it possible, then, to distinguish between structural unemployment—let me say, in the automobile industry, supposing it is overexpanded and has to contract—and general unemployment which is the consequence of the secondary deflation? The next question would be, is it not possible that the secondary monetary repercussions, the so-called secondary deflation, is often much more important than the primary discrepancy that brought it about?

PROFESSOR HAYEK: I have never seen that question empirically investigated and should very much wish to see it attempted. I hesitate to encourage anybody because I don't expect he would get very far; but if anybody were to succeed, I should, of course, be greatly interested in the results.

DR. HABERLER: There was a lot of general unemployment in the 1930s in practically all industries; we could therefore say, without detailed statistical study, that there was a lot of secondary deflation and general unemployment created by that deflation, even if we

cannot say precisely how much unemployment was of one kind or the other.

PROFESSOR HAYEK: This reminds me of a point which I perhaps should have mentioned earlier. There is a special difficulty about the present situation. In the misdirection of labor and the distortion of the structure during the past business cycles, it was fairly easy to point to the excessive expansion because it was, on the whole, confined to the capital-goods industries. The whole thing was due to an overexpansion of credit for investment purposes, so you could point to the industries producing capital equipment as those which had been overexpanded.

In contrast, the present expansion of money, which has been brought about partly by means of bank credit expansion and partly through budget deficits, has been the result of a deliberate policy, and has gone to entirely different channels. The additional expenditure has been much more widely dispersed. In the earlier cases, I had no difficulty in pointing to particular illustrations of overexpansion; now I am somewhat embarrassed when I am asked the question, because I would have to know the particular situation in a particular country, where these additional money flows went in the first place, et cetera. I would also have to trace the successive movement of prices which indicate these flows. In consequence, I have no general answer to this question.

I do not doubt that in a sense we have today the same kind of phenomenon, but the overexpansion, the undue increase of labor employed in particular occupations, is not confined to a single, clearly delimited block such as the capital-goods industries. It is now spread much more widely, and the distribution is much more difficult to describe. This is again a field some statistically minded economist should investigate in order to show how the process operates in a particular country. I should be most grateful for such an investigation.

DR. HABERLER: I think it is time to adjourn. Thank you very, very much, Professor Hayek, for being with us today and for your interesting talk. [Applause.]

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