Hayek's Nobel
50 Years On

EDITED BY KRISTIAN NIEMIETZ

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Founded in 1955, the Institute of Economic Affairs is Britain's oldest free market think tank. Its mission is to improve understanding of the fundamental institutions of a free society by analysing and expounding the role of markets in solving economic and social problems.

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Friedrich Hayek was an economist born in Vienna in 1899. He served in the Austro-Hungarian Army in the First World War before moving to the UK where he spent much of his career teaching at the London School of Economics. In 1944 he published *The Road to Serfdom* and in 1974 he was awarded the Nobel Prize in Economics. He wrote multiple publications for the Institute of Economic Affairs including *Confusion of Language in Political Thought (1968), Economic Freedom and Representative Government (1973)* and *The Denationalisation of Money (1976).*

Foreword

Friedrich August von Hayek never formally worked for the Institute of Economic Affairs, but he was nonetheless one of the most significant figures in the institute's history. If we had to name ourselves after a person, in the way our friends from the Adam Smith Institute do, we would undoubtedly be the Friedrich August von Hayek Institute, or, more realistically for the sake of media-friendliness, the F. A. Hayek Institute.

For a start, it was Hayek's book *The Road to Serfdom* (1944), which won over – and alarmed – Antony Fisher, the future founder of the IEA. A little later, it was Hayek who personally dissuaded Fisher from his initial plan of becoming a politician and who persuaded him to become an 'ideas entrepreneur' instead. It was Hayek's model of how tectonic changes in the climate of opinion happen – outlined in *The Intellectuals and Socialism* (1949) – which became the closest thing to a blueprint for the future IEA, albeit on a very abstract level. Hayekian themes, such as competition as a trial-and-error process, the dispersed and tacit nature of economically relevant knowledge, the role of market prices in collating and transmitting economic information and the relationship between personal and economic freedom, appear in almost every major IEA publication.

Hayek was, of course, also an IEA author in his own right. His IEA publications *Confusion of Language in Political Thought* (1968), *A Tiger by the Tail: The Keynesian Legacy of Inflation* (1972) and *Economic Freedom and Representative Government* (1973), but especially his book *Denationalisation of Money* (1976), are frequently downloaded to this day (although the word 'download' would probably have meant nothing to Hayek). Last but not least, you can spot him in old photos taken at or around the institute, and you can find some of his letter correspondence (no e-mails yet) with our predecessors in our archives.

The anniversary of Hayek receiving the Nobel Prize is therefore an important anniversary for us at the IEA as well.

The Nobel Prize was, of course, not a wholesale endorsement of 'Hayekianism'. Rather, the Royal Swedish Academy of Sciences specifically singled out his 'theory of business cycles and his conception of the effects of monetary and credit policies', and 'new ideas with regard to basic difficulties in "socialistic calculating"'. Those are important parts of Hayek's work, but they are not the sum total.

But, for better or worse, the prestige of a Nobel Prize rarely remains confined to a silo in such a way. For better or worse, the opinions of a Nobel Prize-winning economist will inevitably be taken more seriously, even when they comment on issues that have nothing to do with the subject they won the Nobel Prize for. (The clearest contemporary examples of this have to be Paul Krugman

and Joseph Stiglitz.) It undoubtedly helped the cause of classical liberalism that a series of economists in that tradition won Nobel Prizes in that period, for example, Milton Friedman in 1976, George Stigler in 1982, or James Buchanan in 1986.

We decided to mark the occasion of this anniversary by republishing Hayek's Nobel Prize lecture, *The Pretence of Knowledge*, with introductions from three contemporary scholars steeped in Hayekian thought.

Bruce Caldwell, author of an acclaimed Hayek biography, provides some context about what the Nobel Prize meant for Hayek's career and professional recognition at the time. Although it did lead to a renewed interest in the (by then largely forgotten) Austrian School of Economics, it certainly did not mean that Hayek was now winning the argument. The *zeitgeist* was still very much against him, and he continued to face a lot of hostility.

Until not so long ago, it looked as though Hayek's contribution to the Socialist Calculation Debate had been rendered redundant by events. We seemed to have reached Fukuyama's 'end of history', characterised by a broad acceptance of the market economy across most of the political spectrum. But the rise of 'Millennial Socialism' in the 2010s brought these old arguments back to life. It is simply not true anymore that acceptance of the market economy can be taken for granted. Peter Boettke shows that the 'new' socialism is just as flawed as the old one, and no more robust in the face of the Hayekian critique.

The history of classical liberalism is inseparable from the history of the Enlightenment, so unsurprisingly, classical liberals have a huge respect for science, especially the natural sciences. To this day, we classical liberals often find ourselves arguing against various forms of mysticism and irrationalism, for example, radical environmentalist groups such as Extinction Rebellion and Just Stop Oil, or 'woke' progressives who see science as Western-centric and 'colonialist'. But everything in its proper place, as was the theme of *The Pretence of Knowledge*:

> 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 [...] 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.

Donald Boudreaux shows us some contemporary examples of this misuse of science, in the form of a naïve 'scientism' that Hayek warned us about half a century ago.

While we see this anniversary as a cause for celebration, we nonetheless do not necessarily intend this publication to be just an exercise in backslapping for self-proclaimed Hayekians. Rather, we hope that it will be just as valuable to non-Hayekians or even anti-Hayekians who, even if they end up disagreeing, nonetheless wish to inform

themselves about the ideas of a man who, for better or worse, was clearly a towering figure in the history of economic thought.

The views expressed in this monograph are, as in all IEA publications, those of the authors alone and not those of the Institute (which has no corporate view), its managing trustees, Academic Advisory Council members or senior staff. IEA monographs are blind peer-reviewed by at least two academics or researchers who are experts in the field, a practice which we have waived in this exceptional case, since it is a republication of a historic text.

KRISTIAN NIEMIETZ

Editorial Director, Institute of Economic Affairs

London, October 2024

Hayek's Nobel*

Bruce Caldwell

Introduction

On 9 October 1974 the Royal Swedish Academy of Sciences sent Friedrich A. Hayek a cable to inform him that he had been awarded the 1974 Sveriges Riksbank Prize in Economic Sciences in Honour of Alfred Nobel jointly with the Swedish economist Gunnar Myrdal. On the same day the Secretary General of the Academy, Carl Gustaf Bernhard, sent Hayek a letter that spelled out the details. The prize ceremony would take place on 10 December 1974. Myrdal and Hayek would each get a medal and a certificate, and they would split the monetary award, 550,000 Swedish crowns, equally, with each person's share coming to about US $62,570 at the time. The citation for the prize would read, 'For their pioneering work in the theory of money and economic fluctuations and for their

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penetrating analysis of the interdependence of economic, social and institutional phenomena.'[1]

He originally planned to title his lecture 'The Dangers of the Scientistic Error', but ultimately decided on 'The Pretence of Knowledge', a phrase that he had used before: a variant of it appeared in lectures he gave at the University of Virginia in spring 1961, and it was the title attached to a book project that he began that same year but never finished[2].

Hayek's toast at the dinner stressed, as his Nobel lecture would the following day, the limits of economics as a science. It is worth quoting at length:

> ... if I had been consulted whether to establish a Nobel Prize in Economics, I should have decidedly advised against it ... the Nobel Prize confers on an individual an authority which in economics no man ought to possess. ... the influence of the economist that mainly matters is an influence over laymen: politicians, journalists, civil servants and the public generally. There is no reason why a

1 Carl G. Bernhard to F. A. Hayek, 9 October 1974, the F. A. Hayek Collection, the Hoover Institution, Stanford, CA, box 47, folder 10. A second copy of the letter was also sent to Tokyo, in care of Professor Chiaki Nishiyama of Rikkyo University. Apparently, someone at the Academy knew that Hayek was planning to visit Japan at the end of the month.

2 The notebook for the project ran to 85 numbered pages; it may be found in the Hayek Collection, box 139, folder 9. The Virginia Lectures are now available in Hayek (2014 [1961]).

> man who has made a distinctive contribution to economic science should be omnicompetent on all problems of society - as the press tends to treat him till in the end he may himself be persuaded to believe. One is even made to feel it a public duty to pronounce on problems to which one may not have devoted special attention. (Hayek 1975: 38-39)

Daughter Christine had some vivid memories of the trip, which she described as 'a cracking time'. Christine also had strong memories of her father's co-recipient, Gunnar Myrdal, whom she described as 'a very gloomy chap', who 'didn't smile and made no attempt to be friendly'[3]. She said that they concluded that he was probably miffed about having to share the prize. They did not know that the enmity between Myrdal and Hayek was long-standing, and would soon grow worse, more on which soon.

Hayek's Prize lecture

At 11 a.m. the next day Hayek gave his Prize Lecture at the Stockholm School of Economics. The central message of the talk was a methodological one. Economic ideas are invoked mainly by way of example.

Hayek begins by noting that the dominant problem of the day - accelerating inflation - made the choice of topic almost inevitable. He immediately blames the problem on

3 Christine Hayek, interview with the author, 15 October 2012.

'policies which the majority of economists recommended and even urged governments to pursue', concluding that 'as a profession we have made a mess of things' (Hayek 2014 [1975]: 362)[4]. He then locates the source of the problem: the profession's scientistic attitude. One sees this on display in the profession's acceptance of a theory that posits a simple positive relationship between total employment and aggregate demand. This (Keynesian - though he does not use the phrase) theory is popular, he asserts, because it is one for which strong quantitative evidence can be adduced. (By this he presumably means the statistics that governments collect, such as employment statistics and the national income accounts.) He contrasts this with his own preferred theory, one that locates the cause of the cycle in distortions in the structure of relative prices. Unfortunately, because the structure of relative prices is the result of decisions made every day by millions of market participants, we can never quantitatively estimate what the 'right' structure might be. The conclusion is evident:

> ... 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. (Hayek 2014 [1975]: 363-364)

4 Hayek's politeness is evident in his use of the word 'we' when he clearly meant 'you' economists!

This procedure is dangerous because it can lead to incorrect policy conclusions, for example, that unemployment can be 'lastingly cured by the inflationary policies recommended by the now fashionable theory' (Hayek 2014 [1975]: p. 364).

Hayek moves from his specific example to make a more general point, that when dealing with phenomena of 'organized complexity,' often the best we can do is to make pattern predictions, that is, make predictions about some of the general attributes of the structures that will form themselves. He is eager to emphasise that he is not anti-mathematics; within his own field, for example, the equations of general equilibrium theory provide a picture of the structure of prices. But one cannot meaningfully fill in the equations with data. He recognises that his own preferred theory is a limited one, but he concludes by confessing that 'I prefer true but imperfect knowledge, even if it leaves much indetermined and unpredictable, to a pretence of exact knowledge that is likely to be false' (Hayek 2014 [1975]: 367).

Hayek's concerns go beyond economics. The danger he sees is that the public will come to expect science to be able to do more than it can in the field of human affairs. He specifically mentions the 'enormous publicity' given to the Club of Rome's report on 'the predicament of mankind' in the 1972 book *The Limits to Growth*, and the lack of media attention to the devastating criticisms

that had been given to this report by 'competent experts'[5]. Hayek's fear was that a false image of the overwhelming power of science, joined with progressive views about how to reshape the social world, all reinforced by uncritical media attention, would bring about policies that would end up doing great harm. His fears would only be reinforced by the very public debates among professional economists over such issues that would soon take place.

Battles of the Nobels

Hayek and Gunnar Myrdal's relationship went way back. There was little reason for either recipient to be pleased with the joint award. As was evident in the 10 October 1974 *New York Times* piece that announced the Prize, their economic beliefs could not have been more different. In response to the inflation that was then building in the United States, Myrdal said that wage and price controls and gasoline rationing were necessary to stabilise the economy. Hayek argued that a temporary increase in unemployment was necessary to reduce it.

The reception of the two men's Lectures was also quite different. Hayek had gotten permission from the Nobel Committee to submit his address for simultaneous

5 The *Limits to Growth* (Meadows, Meadows, Randers, & Behrens, 1972) used a computer simulation to provide dire prognostications concerning the fate of the planet in terms of population growth, resource depletion and environmental degradation. This work is a precursor to more recent climate change modelling. For more on this period, see Sabin (2013).

publication in a professional journal. The journal he chose was *Economica*, the LSE house journal for which he had served as editor during World War II. The response from *Economica* editor Ray Richardson was less than heartening. He said that they would accept his paper but preferred a *revised* version, noting that 'it was the general opinion of the editors and the Board that the Nobel Prize ceremony probably inhibits laureates from giving as scholarly and well-ordered account of their views as they would wish.[6]' He then provided five points for Hayek to consider if he chose to revise the paper. Hayek withdrew it from consideration.

Myrdal gave his Prize Lecture not at the December award ceremony but on 17 March 1975. Titled 'The Equality Issue in World Development,' it could be taken as containing just the sort of approach that Hayek had warned about in his own Lecture. The world, and especially the 'Third World' (as the less-developed portion of it was then called) was facing enormous challenges: the immediate oil crisis, but also a food crisis, a population explosion and the depletion of non-renewable resources. The amount of humanitarian aid (as opposed to military aid) given by the United States and many other developed countries to the Third World was miniscule and was so small because it was typically justified with national self-interest arguments. (This he contrasted with the highly moral arguments proffered by his own Swedish government when they gave aid.) If catastrophe was to be avoided,

6 Letter, Ray Richardson to F. A. Hayek, 26 February 1975, the Hayek Collection, box 126, folder 20.

the 'lavish' level of consumption in the developed world would need to be reduced, and aid to the developing world correspondingly increased. Myrdal concludes by offering what he terms 'moral and rational reasons for a new world order', and some steps that would need to be taken:

> ... the cutting down of consumption, and of production for home consumption, of many other items besides food, and in all the developed countries, is rational and in our own interest. This is what the discussion of the 'quality of life' is all about. Our economic growth in a true sense could certainly be continued, but it should be directed differently, and in a planned way, to serve our real interest in a better life ....
>
> I am in deep sympathy with the urgings of medical men, environmentalists and other colleagues in the natural sciences, when they speak for the rationality in our own interest, individually and still more collectively, of a much more frugal life style so far as growth in consumption, and production for home consumption, of many material products is concerned. This is what I sincerely mean is in line with our own welfare as well as our proclaimed ideals. Real economic planning should be done in these rational terms (Myrdal 1975).

The moral argument is accompanied by criticisms of certain groups and practices. He mentions the fact that 'in our competitive society all groups are ... always brought to press for more' and that 'commercial marketing does

certainly not work for a more rational discussion of our consumption demands' (Myrdal 1975). Still, Myrdal is able to end on a positive note because of the promise of science:

> Even though my world view must be gloomy, I am hopeful about the development of our science. We can by immanent criticism in logical terms challenge our own thinking and cleanse it from opportunistic conformism. And we can widen our perspective. Everything can be studied. We are free to expand and perfect our knowledge about the world, only restricted by the number of scientists working and, of course, the degree of their diligence, brightness and their openness to fresh approaches (Myrdal 1975).

Unlike Hayek's Lecture, Myrdal's was very well received. A couple of years after it was delivered, Alfred Kastler, a Nobel laureate in Physics (1966), sent out a circular letter to his fellow laureates, asking them 'to read and to meditate upon' Myrdal's Lecture, and reminded them of their 'responsibility to use their public prestige to help make things go in the right direction and to prevent the misuse of science.'

Another laureate to cause Hayek concern was Wassily Leontief, the father of input-output analysis, who won the prize in 1973. Soon after he gained his Nobel, he joined a chorus of voices that already were calling for economic planning to deal with the economic problems facing the United States. In March 1974 he penned an article for the

New York Times calling for a National Economic Planning Board, and in February of the next year offered up an editorial that began with the sentence, 'Why is planning considered a good thing for individuals and business but a bad thing for the national economy?' By July he was testifying before the Joint Economic Committee of the US Congress. Hayek and Ludwig von Mises had, of course, offered compelling arguments in the 1920s and 1930s against the sort of planning that Leontief was advocating in the 1970s, and Hayek must have been genuinely amazed to see the case being revived in the public arena by a well-respected economist. Hayek ultimately responded with a piece titled, 'The New Confusion about "Planning"' that appeared in January 1976 in *The Morgan Guaranty Survey*, a business periodical[7]. His criticisms of Leontief were substantive and certainly demanded a response, but the one that he got was not altogether satisfying. Instead of responding publicly, Leontief sent a letter to the publisher of *The Morgan Guaranty Survey* in which he called into question Hayek's *scientific* credentials, noting that he could find no reference to Hayek in the *International Encyclopedia of the Social Sciences.* He concluded that 'Professor Hayek can claim the unique distinction of being the only Nobel Laureate whose name and contributions are not mentioned even once

7 Hayek (1976/1978). Hayek's article includes references to the *New York Times* articles and to Leontief's appearance before the Joint Committee.

in the authoritative international reference work on the discipline that he represents.'[8]

The next episode involved Hayek only indirectly[9]. In 1976 Milton Friedman won the Prize. The year before Friedman visited Chile and, during the visit, had an audience with General Augusto Pinochet, who had become President after a military coup overthrew the democratically elected government of the Marxist politician Salvador Allende. Friedman had offered the Pinochet government advice on how to reduce inflation. Within weeks of the announcement, the *New York Times* published two letters, each signed by two Nobel laureates[10]. One letter spoke of the Nobel selection committee's 'deplorable exhibition of insensitivity' in giving him the prize, the other called

8 Letter, Wassily Leontief to Milton W. Hudson, 23 February 1976, the Hayek Collection, box 33, folder 8. Hayek found out about the letter because the publisher of the Survey sent him a copy. Some recent research suggests a different view of Hayek's influence: Skarbek (2009) examined the Nobel laureates that the other laureates in economics had cited the most in their Prize Lectures, and the top two on the list were Kenneth Arrow and Hayek. Hayek got 13 citations; Leontief and Myrdal each got 3. Along similar lines, a recent Google Scholar search indicated higher citation counts for Hayek than for either Leontief or Myrdal. Hayek's top piece ('The Use of Knowledge in Society') was cited just under 11,000 times, whereas the top pieces of Myrdal and Leontief received 5,674 and 2,585 citations, respectively. Retrieved 3 November 2014.

9 The next few paragraphs draw on Caldwell and Montes (2015), where Hayek's reaction to the public criticism of Friedman is noted in the course of explaining why Hayek decided to accept an invitation to visit Chile in November 1977.

10 One was signed by George Wald (medicine) and Linus Pauling (chemistry and peace), the other by David Baltimore and Salvador E. Luria (both medicine).

the committee's decision 'disturbing' and 'an insult to the people of Chile' who were 'burdened by the reactionary economic measures sponsored by Professor Friedman' (Friedman & Friedman 1998: 596–597). When Friedman went to Sweden to receive the prize in December there were multiple demonstrations, and during the ceremony itself an individual protester shouted 'Down with capitalism, freedom for Chile' as Friedman was receiving his medal.

On 14 December 1976, four days after the awards ceremony, Hayek's Nobel co-recipient published a piece in the Swedish newspaper *Dagens Nyheter*, an English translation of which soon appeared in the American popular economics magazine *Challenge*. Noting Friedman's recent receipt of the prize, Myrdal criticised the Swedish Academy of Science for its secretive practices in choosing recipients, a process that made it difficult for any opposition to form prior to their recommendation[11]. He also argued that, because economics is at best a 'soft' science, the awarding of a Nobel in it had become a political act that should be discontinued. Myrdal then segued into a discussion of the prize he had shared with Hayek (another political act, in his view), noting the

11 Lindbeck (1985) offers a description of the process by which the Nobel Prize winners in economics are selected. The prize in economics dates only to 1969. Given Myrdal's later ambivalence about such an award, it is interesting to note that before the decision had been made to grant one in economics, Myrdal was a leader of those who 'energetically pushed the idea of a prize in economic science' (Lindbeck 1985: 38). His actions in this regard led biographer Barber (2008: 164) to remark, 'The thought that he might have a personal stake in this was rather ill disguised.'

'thousands of cablegrams I received from colleagues all over the world, mostly informing me that they were deeply critical of the Nobel Prize being given to Hayek' (Myrdal 1977: 52). Thousands! He ended his piece expressing regret that he had accepted the award. His excuse was that

> I should have declined to receive it, particularly as I did not need the money but gave it away … But I had not then thought the problem through. I was merely disgusted. Also, the message reached me very early one morning in New York, when I was totally off my guard (Myrdal 1977: 52)[12].

Hayek had throughout his career been known for keeping his disagreements with opponents on a professional level[13]. By the 1970s he was doubtless beginning to wonder if this had been a good strategy. The treatment Friedman was receiving would have angered him. So would Myrdal's

12 Myrdal's piece did not go unnoticed. On 31 May 1977 the *New York Times* published an article by Leonard Silk entitled 'Nobel Award in Economics: Should Prize Be Abolished?' Silk begins by summarising the controversy: 'The award of the Nobel Memorial Prize in Economic Science to Prof. Milton Friedman of the University of Chicago last October provoked a storm of criticism over Professor Friedman's right-wing politics, focused particularly on his willingness to give advice to the central bank and the post-Allende government in Chile. This storm has been followed by a blast from an earlier Nobel Laureate, Prof. Gunnar Myrdal.' The disagreement among economists had become a very public event.

13 In his review of Hayek's *The Road to Serfdom*, Schumpeter (1946: 269) characterised both Hayek and the book as (perhaps overly) polite: '… it is also a polite book that hardly ever attributes to opponents anything beyond intellectual error. In fact, the author is polite to a fault …'

incredibly intemperate public remarks: 'disgust' is a strong word. (At least this explains why Myrdal appeared gloomy at the awards ceremony!)

Throughout this period other laureates took public policy positions on various issues of the day, usually on the opposite side from Hayek. In October 1976 the Club of Rome published its third report, titled *Reshaping the International Order*. The book was produced by a group of about 20 experts led by Nobelist Jan Tinbergen, and had many 'proposals for action' to speed economic development and reduce income inequality. Another episode involving laureates culminated in Hayek once more standing virtually alone. A 'Manifesto on World Hunger' was circulated among Nobel laureates by a group called Food and Disarmament International and whose programme drew directly on Myrdal's ideas that spending on armaments should be greatly reduced and redirected towards providing food and other assistance to developing countries. Fifty-four Nobelists signed by the time the group went public in June 1981. The economist signatories included Myrdal, Tinbergen, Lawrence Klein and Kenneth Arrow. Hayek's reaction was given at a Kuratorium meeting of Nobel laureates in the Bavarian town of Lindau that he happened to be attending at around the same time. On 28 June 1981 (only a few days after the group's announcement) he read a four-page speech there protesting the Manifesto, contesting its factual claim that the Third World was facing mass starvation. He also pointed out that the majority of signatories were natural scientists who had no expertise in such matters, yet were using their scientific prestige

to attempt to sway public opinion. This seemed a clear violation of scientific principle: 'we have no right to vouch for the scientific validity of views which [we] are not professionally competent to judge'[14].

Consequences, intended and unintended

As it did for many others before and since, the awarding of the Nobel changed Hayek's life opportunities. Along with finishing up his *Law, Legislation and Liberty* trilogy, he also found time to return in his research to the two topics - monetary theory and the critique of socialism - which had occupied him when his career first began more than 40 years earlier. The result of the first effort was 'The Denationalization of Money' (Hayek 1999 [1978]), a pamphlet that argued for benefits of the competitive issue of money and which helped to spawn a renewed interest in the study of alternative monetary regimes that continues today.

The second topic was a direct response to the evident gap between his own views and those of the majority of other Nobel laureates in economics in the 1970s that was just discussed. We can trace Hayek's reaction in the notecards that he inscribed during this period. He was clearly increasingly agitated about claims that were being made by other prominent economists, and resolved to

14 The relevant papers may be found in the Hayek Collection, box 30, folder 4; box 32, folder 1; and box 110, folder 3.

write a book that would be titled *What Is Wrong with Economics*. His targets would include Paul Samuelson on unemployment, Leontief on planning, Tinbergen on social justice, and Myrdal on development, with appendices on (John Stuart) 'Mill's Muddle' and 'The Neglect of Ludwig von Mises.' Though he never wrote the book, the behaviour of certain of his fellow laureates clearly disturbed him.

Hayek decided instead to organise 'the Paris Challenge,' a grand debate between the proponents of socialism and capitalism. The proposition to be debated was, 'Was Socialism a Mistake?' He envisaged a five- or six-day affair, with 12 speakers on each side. Hayek worked on the project for much of 1978 but could not get the line-up of personnel that he hoped for, nor, perhaps more importantly, sufficient funding. He turned again to the writing project but this time it was not to be directed against the economics profession, but against socialism itself. The book that was finally produced was his last major publication, *The Fatal Conceit: The Errors of Socialism* (1988)[15].

For the second time in his long career, Hayek had become a recognised public intellectual[16]. Especially in the United

15 See Cubitt (2006: 24-26) for more on the Paris Challenge. The materials relating to the Paris Challenge episode may be found in the Hayek Collection, box 125, folders 7-10.

16 The first time was the publication of *The Road to Serfdom* in 1944 and, perhaps more important, the subsequent condensation of that book in *The Reader's Digest*. For more on this, see the editor's introduction to F. A. Hayek (2007 [1944]).

Kingdom as the Thatcher government began to enact policies that challenged the growth of the welfare state, Hayek became for many among the British chattering classes a figure of scorn, the 'mad professor' or 'Mrs. Thatcher's pin-up boy' (in Labour MP Michael Foot's colourful prose) who had somehow taken control of Thatcher's mind (Cubitt 2006: 28). Hayek had not, of course, contributed directly to the rise of political figures like Thatcher and Ronald Reagan, whose successes were due more to public disenchantment with the manifold policy failures of their predecessors, and he had very little influence on the policies that were actually enacted. But at least certain politicians mentioned his name positively and paid lip service to his ideas, which was a welcome change from his treatment in other quarters during the 1970s.

Acknowledgments

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Hayek and Technosocialism

Peter J. Boettke

In 1989, after a long and nightmarish experience, the socialist regimes in East and Central Europe all collapsed, epitomised by the fall of the Berlin Wall in November of that year. In 1991, the Soviet Union ceased to exist. Statues of Marx and Lenin were toppled in cities across the former socialist world, and a new era of freedom was ushered in. The death toll of this experience was estimated to be upwards to 100 million souls. Economic deprivation and political tyranny were defining characteristics of these regimes.

The transition from socialism, however, did not always go smoothly, as the political economy questions of dealing with the vested interests of the old regimes were more difficult than anticipated, and the establishment of the required institutional framework confronted both political and cultural resistance. But in those countries that successfully transitioned, undeniable benefits to people's lives were realised. They lived longer and more prosperous lives. In Poland, for example, the average income in 1990 was around $8,493 when reforms were introduced, and $31,985 by 2017. Had they stayed on the

pre-1990 growth path, the average income would only have increased to $14,177 in 2017. That is an amazing difference in the standard of living of the average Pole. But their life expectancy increased just as dramatically. Prior to transition, Polish life expectancy was around 70 years; after the transition it rose to 77 years by 2017. That is more than 6 years greater than what the socialist trend line would have predicted. Market reforms delivered to the average Pole a longer and more prosperous life. This pattern is similar across all the former socialist countries that succeeded in liberalising their economies and their polities, despite hardships along the way and missteps by political leaders. But as the horrors of the socialist experience fade into the past, a new generation of intellectuals are once more attracted to socialism. Globalisation has produced an economic miracle – in 2015, for the first time in human history, less than 10% of the world's population were living in extreme poverty. Back in 1980, that figure was closer to 40% of the global population. But globalisation has also highlighted growing inequality both within and between countries, especially the global south. The reason for this is often glossed over by critics, who want to blame it on neoliberalism and market fundamentalism, but the best data we have suggests that the divide is between those who have integrated into the global economy and those who have for whatever reason failed to do so. Globalisation is not the cause of the inequality, but the solution to it.

Nevertheless, socialism in the 21st century has had a revival of sorts. We can criticise this as part of an era of

the 'Great Forget', when the horrific lessons of the 20th century are lost because of the bias of presentism and a misunderstanding of the hard-fought arguments in the social sciences that were played out over that century of war, depression and cold war. We need to remember what lesson 1989 should have taught us about the socialist system. We need to revisit the thinkers who clarified the reasons for the failure of socialism and challenge its scientific and moral claims.

F. A. Hayek, along with his mentor Ludwig von Mises, was the primary critic of the economic aspirations of the socialist project in the 20th century. They would be joined by other economists, social scientists and philosophers – and by historical experience – in this critique of the socialist enterprise, but they are the authors of the *economic critique* of socialism. Their work is not a normative critique of socialism – though they were both liberals and had a strong affinity for normative individualism. But the critique of socialism they offered was an immanent critique and, strictly speaking, a means/ends analysis of this social system of production and distribution. Socialist ends were examined in light of socialist means chosen for their effectiveness in achieving the desired outcomes.

The original socialist economic system promise was for a *rationalisation* of production that would result in a burst of productivity that would deliver mankind from the 'Kingdom of Necessity' to the 'Kingdom of Freedom'. This would deliver humanity from the vagaries of economic inefficiency and injustice that capitalism wrought.

Rationalisation of production was a prerequisite for the achievement of a classless society. The institution of private property and the capitalist process of exchange and production led to both alienation and exploitation. The radical promise of socialism of the eradication of the injustice of exploitation was premised on the transcendence of alienation. The revolution called for the abolition of private property and commodity production for profit. Collective ownership would follow, and production for direct use, rather than exchange, would substitute. The 'invisible hand' of the market, which takes place behind the backs of citizens, would be abolished in favour of the visible hand of collectivist planning. Rationalisation would bring deliverance.

But what if the chosen means could not achieve the desired ends? What if the abolition of private property and market exchange led not to rationalisation but to economic chaos? All social systems of production must by necessity have some sorting process to move from the abstract notion of 'the desirable' to the more concrete notion of 'the feasible' and finally to the very practical notion of 'the viable'. Thus, any social system of exchange and production must perform this function of *rational economic calculation*. And, to put a finer point on this, in a world of scarcity – which is our human existence – there must be some mechanism in place that determines how we produce more with less and avoid producing less with more. This is the very definition of economic rationality at the system level, and it does not matter what ends one seeks to satisfy normatively, the pursuit must be done in

the most efficacious manner possible to achieve (or come close to) 'the good society' sought.

This is why the Mises–Hayek critique cuts to the heart of the issue. They argued that without the price system, this sorting in the economic realm between the desirable, feasible and viable would be rendered impossible. Not impractical, but literally impossible. Why? Because without private property in the means of production, there would be no market in the means of production, and without a market, there would be no relative prices established indicating the relative scarcity of various goods and services, and thus, there could be no rational economic calculation of the best uses of scarce resources. Rather than more with less, such a voyage without an economic compass would result in producing less with more – the very definition of economic irrationality. Socialist goals were unachievable via socialist means.

This argument originally offered by Mises in 1920 and then in 1922 sparked a debate in the German language, and after the subsequent translation of his essay in 1935, led to an English-language debate in which Hayek and his colleague Lionel Robbins took up the side of the opposition to socialism. On the other side, for our purposes here, the main advocates of the economics of socialism were Oskar Lange and Abba Lerner. They basically accepted the basic neoclassical argument (a variant of which has been stated above) that *any* social system of exchange and production faces the same fundamental problem of allocating scarce resources among competing ends, and as such there is a *formal similarity* that they face. The

optimality conditions of a socialist system are precisely the same optimality conditions that capitalism must satisfy. The formal relationship between averages and marginals is the same across time and place, as well as system. In short, to achieve optimality, the 'price' must be equated with marginal cost, and production must be at that level, which is equated with average cost. This would ensure that the full opportunity costs of production are taken into account and all least-cost technologies are employed. The formal similarity proposition stated by early neoclassical economists such as Friedrich von Wieser and Vilfredo Pareto was just a logical statement about the necessity of any system to meet these optimality conditions if it were to achieve economic efficiency in production and distribution.

It was not, in their hands, a statement about how alternative systems could or could not mimic this formal result. What Lange and Lerner did was establish a model of 'market socialism' that simply applied the logic of the Walrasian system to the implementation of socialist planning. At its most basic, they simply postulated that the central planning board dictated to socialist firms to set price equal to marginal cost and produce at average cost. Once this is done, they argued, socialism could operate just as efficiently as capitalism in theory. They furthermore argued that since modern capitalism suffered from serious practical problems in operation caused by private property and the inherent dynamics of capitalism – e.g., separation of ownership from control, monopoly and imperfect competition, macroeconomic volatility and unemployment – their market socialist

model could *outperform* capitalism in practice. In stating their case, especially Lange, they thought that they had thoroughly refuted Mises, Hayek and Robbins.

Hayek spent the next decades of his career articulating his response to these arguments and developing his unique insights into the nature of the price system and the market economy. Rather than go through that evolution step by step, in this essay I will just focus on Hayek's 1982 article 'Two Pages of Fiction' published in *Economic Affairs*, where he tries to set the record straight. He starts this article by stating clearly that the 'endless repetition' of the claim that Oskar Lange had refuted Mises's argument is in fact false. Hayek goes step by step through the assertions made in the analysis and the justification of the claims made. His first point in the argument relates to Lange's misappropriation of Philip Wicksteed's discussion of a wider notion of price in his analysis. As Hayek says, Wicksteed nowhere made the argument that these 'quasi-prices' could serve as substitutes for market prices. The individual will, of course, Hayek insists, have to weigh alternatives in the act of choice, 'but the problem is precisely how he can do so where he does not know the particular concrete facts determining this necessity' (Hayek 1982). Mises's argument is that the alternatives offered to us become known to us in economic life through money prices established on the market. Lange's effort to turn basic economic theory against Mises is to Hayek 'inexcusable' and can only be explained by the politically predetermined prejudices on the part of the thinker (Hayek 1982: 135).

Hayek digs deeper to expose Lange's errors. Lange throughout his argument against Mises refers to 'given data', and as Hayek points out, this assumption possesses an 'irresistible attraction' to the mathematical economist because it provides tractability. Issues such as deep uncertainty, changing circumstances, ignorance and the question of whom this data is given to are analytical nuisances, as are the generative nature of the market price system and the role that the entrepreneur plays in the discovery, utilisation and communication of the unique knowledge of time and place. Treating all the relevant knowledge as data given in total to the socialist planners is tantamount to solving the problem by assumption. It no way explains how this knowledge is discovered and becomes known to planners. Instead, this approach to modelling the planner's problem 'asserts a sheer impossibility which only a miracle could realise' (Hayek 1982: 137). Once we recognise that the knowledge of the market would not be available to anyone where prices are not provided by the generative process of the competitive market process, then we can start to make progress in economic science. The knowledge of the market is contextual in nature and does not exist outside of that context. One of Lange's problems, Hayek contends, is that he gets confused between the knowledge that economic participants utilise in their day-to-day affairs as they must adapt to a multitude of circumstances and adjust to constantly changing conditions, and the knowledge the economic theorist pretends to possess in order to write down an equilibrium solution to a set of simultaneous equations.

Hayek tells his readers that 'I have never conceded' the Misesian argument about impossibility (Hayek 1982). Hayek, following Mises, argued that rational economic calculation was impossible in the socialist commonwealth. Without private property in the means of production, there would be no prices to guide economic actors, and without these prices, the economic planners could not engage in the rational calculation of alternative investment projects. The system would be unable to sort from the technologically feasible to the economically viable in the assessment of productive activity. It would instead be planned chaos, characterised by systemic waste and the inability to correct, so less will be produced with more rather than more being produced with less (the very definition of rational production). Lange simply erred in his caricature of the positions held respectively by Mises and Hayek. Treating Hayek's statements that logically, if all the knowledge required to plan was given; if the actors were omniscient, benevolent and omnipotent, then an equilibrium solution follows, is *not* a retreat to the practical difficulties. Rather, it was to highlight the 'factually false hypothesis' that all the necessary information was at the disposal of the planning authorities. In fact, Hayek refers to this effort to claim he retreated from the Misesian position as a 'scandalous misrepresentation.'

I would add that Mises, in his treatise *Human Action* (1949), also pointed out that once the assumption of omniscience was joined with the assumption of benevolence, then the argument for socialist planning was inevitable. But such assumptions do not address the fundamental problem

of the rationalisation of production. Mises wanted to highlight the unique role that *economic knowledge* played in the coordination of economic activities through time. As a result, he, for the sake of argument, assumed planners were rightly motivated and that they possessed all the relevant technological knowledge of their day. The question was: How do economic planners sort from the array of technologically feasible projects to those that are economically viable in the absence of the price system? The answer in 1920 and the answer to this day is: they cannot!

Lange's confusion in this debate was highlighted in an article he wrote many years after the debate, entitled 'The Computer and the Market' (1967). In this article he says that his original piece refuted Mises and Hayek and Robbins. But if he were to write today in the 1960s rather than the 1930s, he would simply say, What is the problem? We simply put the simultaneous equations into an electronic computer, and the solutions would be provided in less than a second. The market is an antiquarian computational tool. Once we recognise that the market is little more than an effort to simulate what an electronic analogue computer can do, we realise its outdatedness. Mathematical modelling is an essential instrument in optimal planning, and it can fulfil a function that the market economy and its clumsy and awkward price system were never able to perform (Lange 1967).

Lange's argument foreshadowed modern arguments for technosocialism. But his argument, and those of today, are based on the fundamental confusion that

the problem that must be solved is a *computational* problem. There was a misleading use of both Pareto and Enrico Barone by Lange and those influenced by him ever since. Before detailing that argument, however, let me simply state that every generation of enthusiasts for socialist planning claimed to utilise the most advanced techniques of their age to achieve their goals. Only the romantic, unscientific socialists refused to address the necessity of solving the problem of the rationalisation of production. The scientific socialist offered answers from labour coupons to War Planning, to Taylorism and piece rate production, to Five-Year plans, to planometrics to Input-Output tables, to linear programming and to cybernetics. These tools, thinkers argued through the last century, would provide the missing lynchpin that would make the socialist system workable and usher in the new age of the rationalisation of production. But they only work if the problem to be solved is a computational one. The other aspect that I want to clarify is that of incentives and management of economic affairs. Early socialists dismissed these concerns because, with the transformation of the material forces of production, the actors in the system would also be transformed and no longer possess the characteristics of homo-economicus. Modern socialists like Lange dismissed these concerns as psychological and not economic. Mises and Hayek, on the other hand, understood full well the practical importance of the disincentive effects of collective ownership and bureaucratic management, but for sake of argument they granted the assumptions of both early and modern socialists to demonstrate that even if the socialist planners were rightly motivated, they would not

have access to the knowledge that would enable them to achieve their stated end of rationalisation of production. This was part of their argumentative strategy to steelman their intellectual opponents' argument. In making this move they focused attention on how the market price system brings about the necessary adaptations and adjustments to constantly changing circumstances, and it is the main task of economic science to explain this process. The upshot of this debate was for Mises and Hayek to articulate as clearly as possible how property rights incentivise actors, how relative prices guide actors and how profits lure actors, and losses discipline actors so that productive specialisation is pursued and peaceful social cooperation under the division of labour is realised. In explicating this process, they highlighted that a corollary to the division of labour in society was the division of knowledge, and that it is through the market process this knowledge is discovered, utilised and communicated. The knowledge is contextual and generative; outside the process itself it is not just difficult to obtain – *it does not exist*.

When Hayek published *Collectivist Economic Planning* in 1935, he included an appendix that translated Enrico Barone's 1908 paper 'The Ministry of Production in the Collectivist State.' Hayek, also, often pointed to Pareto's argument about the computational complexity of the mathematical solution, whereas the market 'solves' the problem without any central direction every day. However, it is important to look at these two pages from Barone and read them carefully:

> It is conceivable, in fact, that with a vast organization for this work it would be possible to collect the individual schedules for every given series of the various equivalents, including the premium for deferred consumption. Hence it is not inconceivable that with these schedules collected – *always supposing the technical coefficients known and invariable* – it would be possible by a paper calculation to find a series of equivalents, which would satisfy the equations expressing the physical necessities of production and the equalisation of costs of production and the equivalents, which become prices. There is no analytical difficulty about it: it is a problem of very simple linear equations (Hayek 1935: 287, emphasis added).

But Barone quickly clarifies his position, which is forgotten by Lange and other readers, when he states, 'it is frankly inconceivable that the economic determination of the technical coefficients can be made a priori in such a way as to satisfy the conditions of the minimum cost of production which is an essential condition for obtaining that maximum to which we have referred' (Barone 1908) Barone continues and argues that what is needed to gather the knowledge of the coefficients is brought about only experimentally in the process of the competitive entrepreneurial market economy. Experiments can be successful, or they can be unsuccessful, and what we learn from market experimentation is how to adjust and adapt to the myriads of circumstances and opportunities.

> The Ministry of Production could not do without these experiments for the determination of the economically most advantageous technical coefficients if it would realize the conditions of the minimum cost of production which is essential for the attainment of the maximum collective welfare (Barone: 288).

These two pages offset the claim that Barone proved Mises wrong in advance – Mises publishing his original article on social calculation in 1920 and Barone publishing this piece in 1908. Barone is not responsible for the 'fiction' but those reading him were, such as Lange. As Hayek wrote in 'The Competitive Solution' (1940), 'The fact that is has never been denied by anybody, except socialists, that these formal principles ought to apply to a socialist society, and the question raised by Mises and others was not whether they ought to apply but whether they could in practice be applied in the absence of a market' (Hayek 1948 (1940]: 183).

The modern literature has still not come to grips with the Mises critique and the nature of the market process, let alone Hayek's elaboration of the discovery function of the market process and the role of prices in the economic system. Prices guide production, and calculation enables coordination. In the modern proposals for technosocialism, like Lange did before them, they say, What is the problem? Artificial Intelligence (AI) can now solve this, and that the market economy is an obsolete

computational machine. Jack Ma Yun[1], founder and chairman of Alibaba Group, for example, declared in his speech at the World Zhejiang Entrepreneurs Convention in November 2016:

> Over the past 100 years, we have come to believe that the market economy is the best system, but in my opinion, there will be a significant change in the next three decades, and the planned economy will become increasingly big. Why? Because with access to all kinds of data, we may be able to find the invisible hand of the market.
>
> The planned economy I am talking about is not the same as the one used by the Soviet Union or at the beginning of the founding of the People's Republic of China. The biggest difference between the market economy and planned economy is that the former has the invisible hand of market forces. In the era of big data, the abilities of human beings in obtaining and processing data are greater than you can imagine.
>
> *With the help of artificial intelligence or multiple intelligence, our perception of the world will be elevated to a new level. As such, big data will make the market smarter and make it possible to plan and predict market forces so as to allow us to finally achieve a planned economy.* (emphasis added)

1 'Can Big Data Help Resurrect the Planned Economy?' *Global Times,* June 14, 2017. (https://www.globaltimes.cn/page/201706/1051715.shtml).

Such grand promises are repeated in other popular treatments of 'technosocialism' such as Brett King and Richard Petty's *The Rise of Technosocialism* (2021) or Leigh Philips and Michael Rozworski's *The People's Republic of Walmart: How the World's Biggest Corporations are Laying the Foundations for Socialism* (2019). The means to achieve the socialist goals are finally provided for us today by advanced computing. No longer will the planned economy face the problems that plagued efforts in the past, nor will it suffer the dysfunctions that resulted because of the failure to adequately address those problems. Furthermore, if we want proof of concept, all we have to do is point to the success of multi-national and large-scale enterprises. The modern large corporations rely on these big data tools and, in so doing, prove that central management of an economy is indeed possible. But in making these arguments, these writers once more confuse the calculation issue with a computational issue, and they get confused over the nature of the social epistemic problem that must be confronted in commercial life. For example, Philips and Rozworki are simply making a category error, as Hayek would stress, between organisations and orders. Organisations such as firms (even large firms such as Walmart), in short, have a teleology and rely on identified residual claimants with decision authority to coordinate their enterprise activity, but orders – the economic system as a whole – have no such teleology, and no one actor or group of actors has decision authority over the entire system. There is no single scale of values for society as a whole. The social order is not so arranged to achieve a single end but instead a multiplicity of ends determined by the participants in

the 'Great Society'. The economic order, as opposed to the economic organisation, is means-related, not ends-related. But there are other foundational issues besides this category error associated with the long history of the socialist calculation debate that are being glossed over in the current conversation.

First, as we have argued, the calculation problem was never a computational complexity problem; it was a problem of the contextual nature of knowledge. The knowledge utilised in the market is knowledge of time and place. Outside of that context of market competition, the knowledge does not exist. It is not that it is difficult to access; it is that it is nowhere to be found as it was never generated. Second, much of this knowledge is not only contextual and emergent but tacit in nature. It is the type that cannot be gathered as a statistic.

The market process should be characterised as one of adaptation and re-adaptation to constantly changing circumstances and presents to economic actors what is called a 'wicked learning environment' – one where the parameters are relatively free. What computers can do is process most efficiently information in 'kind learning environments' – one where the parameters are fixed. In such a world, the algorithms that are finite and known (even if absurdly numerous) just need to be sorted with speed. Computers can do that – e.g., chess. But in those 'wicked environments' the adaptations and adjustments require a skilful adjudication between a variety of past experiences and the exercise of judgement over imagined responses, and through some combinatorial thinking

creative and novel adaptations emerge to tackle the problem at hand – e.g., playing soccer. Computers can expertly play chess, but they only badly and without much agility play soccer. Is that a technological question or an essential element associated with comparing action within a world of fixed parameters with action within a world of free parameters? The Hayekian argument was that this was not just a technological issue but an essential one related to the nature of the knowledge to be utilised by the actors to effectively execute their plans. A world of creativity and novelty or, in other words, a world of entrepreneurship, cannot be reduced to algorithms. Modern technology can be an extremely useful tool for individuals in aiding their competitive activity on the market (such as Walmart), but these tools are not substitutes for the market process and the role that prices play in guiding us, the role that profits play in luring us, the role that losses play in disciplining and the role that the pattern of resource ownership plays in incentivising us. The market process is a continual process of adaptation and adjustment guided by price movements, and this process is made possible by the liberal institutions of property, contract and consent.

Hayek's core arguments about the price system and the market economy, as well as the liberal principles of political economy and justice, were among the most creative advances in economic science in the 20th century, and their scientific insight and social philosophical wisdom remain among the most enduring contributions in the history of political economy.

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Hayek and the Dangers of the Misuse of Science

Donald J. Boudreaux

At the height of the COVID pandemic, proclamations that 'Science is Real' were seen everywhere and were used to justify lockdowns, vaccine mandates and other COVID interventions. I encountered this proclamation on various websites as well as on physical posters plastered on walls in some buildings on the campus of George Mason University, where I teach. After in-person teaching had resumed, I noticed that one of my students had a 'Science is Real' sticker on her computer[1].

My reaction each time I encountered this proclamation was, 'Who, exactly, says that science *isn't* real?' Oh, I know that back in 2020 and 2021 people could be found on social media making absurdly unscientific claims about COVID – just as they could be found then and can still be found now, making absurdly unscientific claims about the environment, election conspiracies, penis enhancement and countless other topics. With a substantial portion of the world's eight billion people

1 Such as this sticker: https://dissentpins.com/products/science-is-real-sticker?gad_source=1&gclid=EAIaIQobChMIi7mVsc3dhgMV60lHAR-0P3Qe2EAAYASAAEgKPL_D_BwE

connected to smartphones or laptops, the probability of some randomly selected science-denying notion being publicly propounded by at least one of these individuals is nearly 100 percent.

And while many of even the most rational persons in our midst each sports a small handful of quirky, irrational beliefs, I am confident that the number of modernity's denizens who truly believe that science is fake or useless is too small to matter.

There is, however, one particular irrational belief that is widespread and poses a serious threat to liberal civilisation. Ironically, that is the belief that society is, or ought to be, a science project. Our civilisation is not threatened by the puny puddle of people who deny that science is real; instead, it is threatened by the deep ocean of people who mistakenly think that science is godlike and that scientists are the deity's angels who are here on Earth to engineer us to bliss points if only we will let them.

It is this faulty notion of science that F.A. Hayek warned against so often and in varied ways, including in his Nobel lecture[2].

Hayek said in the opening moments of that lecture that, as a profession, economics has 'made a mess of things'. Hayek's observation here refers to three levels of 'mess'.

2 In addition to Hayek's Nobel lecture, see Hayek 2018.

The most obvious mess that he had in mind is inflation, which he noted 'has been brought about by policies which the majority of economists recommended and even urged governments to pursue'. Accelerating then in the U.S., the U.K. and the continent, inflation was indeed a serious problem for which economists deserved much of the blame.

A deeper mess, therefore, was a particular style of economic theorising that set governments loose to create inflation. That theorising – the purest form of which is Keynesianism – is of macroeconomics that portrays the economy as a clean and mechanical function of 'aggregate demand' interacting with 'aggregate supply'. Unemployment is simply the result of the former being too low, and in the Keynesian view there is no reason to suppose that market forces alone will raise aggregate demand to its full-employment level. In order to ensure full employment, therefore, aggregate demand must be managed by the government. As long as there are unemployed resources, increased spending will increase output without raising prices. Inflation was believed to occur only if and when government 'stimulus' continued after full employment had been reached, for only then would there be no more idle resources available for use in creating the additional outputs that prevent the rise in aggregate demand from causing a rise in prices.

By the mid-1970s, however, both inflation and unemployment were rising. Keynesians were surprised, but not Hayek. Hayek understood several realities that the Keynesians did not, not the least of these being that the

economy is too complex to model with aggregates such as 'aggregate demand.' These aggregates mask an essential aspect of a market economy, namely, the countless and ceaseless adjustments that individual entrepreneurs, investors, firms, workers and consumers make – each in light of his or her own unique bits of knowledge – to the many individual prevailing wages and prices. Ultimately, what drives economic activity is the relationship of particular wages and prices relative to each other. Taking proper account of the complex phenomena that swirl beneath the aggregates reveals that the seemingly straightforward connection between total spending and unemployment is not straightforward at all.

'Labour' isn't a homogenous blob, nor are 'capital' and 'investment' or 'savings' and 'consumption' or 'stimulus spending'. The last causes some prices and wages to rise earlier than it causes others to rise. The resulting distortion in the pattern of relative prices pollutes the information on which savings and investment decisions are made. Distortions in real economic activity appear and mount, as do expectations of inflation. Economic activity becomes less efficient and vigorous. Unemployment rises along with inflation – an occurrence that is effectively ruled out by assumption by the construction of simple macroeconomic aggregates.

The third and deepest way in which economists have made a mess of things is rooted in the intellectual impetus to mimic the method of physics. Infatuation with positive prediction drew economists' attention away from the messy, individualised and practically impossible-

to-measure changes in relative prices. The courses of all these individual, particular prices and wages are practically impossible to observe and – if for no reason other than that they depend in part on subjective tastes and expectations – theoretically impossible to predict. (Anyone who could correctly do so would quickly become a trillionaire.) Yet entrepreneurs, investors, workers and consumers respond only to these individual, particular economic phenomena. Economic actors do not respond to aggregate demand, aggregate supply or the price level.

Nevertheless, measurable correlations of aggregate economic phenomena to each other seemingly indicate the same sort of relationships that physical scientists detect among physical phenomena and then use to successfully predict future states of these phenomena. So, to be truly 'scientific', it was thought, economists had to measure measurable things and report any discovered correlations. Economic aggregates are measurable in ways that individual, particular relative prices are not. Macroeconomic science, therefore, had measurable aggregates as its elemental units.

The 'stagflation' that was accelerating just as Hayek was delivering his Nobel lecture did much to discredit Keynesianism. But there remained the belief that the economy is an object that can and should be manipulated scientifically. From imposing taxes in order to 'internalize' the 'social costs' of carbon emissions, to implementing tariffs and subsidies in order to encourage the domestic development of 'the industries of the future,' to deploying antitrust regulators in order to ensure 'optimal' industrial

structure and competitive conditions, the economy is portrayed as a machine to be engineered.

For at least two reasons, this portrayal is deeply flawed.

The first reason is that, while writing or enunciating words such as 'social costs' or 'industries of the future' often suggests that data on such phenomena is easily enough gotten, such data is typically unavailable. We can all agree that there are negative consequences of carbon emissions imposed on third parties, but this reality does not imply that we have any means of measuring these consequences. How large are these negative consequences compared to the (usually ignored) positive consequences of carbon emissions, such as lowering the risks of dying from cold weather?[3] What would be the costs, in terms of reduced economic growth, of government-engineered efforts to reduce carbon emissions? And given that many carbon-emitting activities are already taxed – for example, retail taxes paid on petrol sales – might it be possible that we are already taxing carbon emissions optimally, or perhaps even excessively? (Boudreaux 2014) As for the industries of the future, what are they? And what, exactly, *is* the optimal number of firms in the retail industry or in the pharmaceutical sector?

No one knows the answers to questions such as these. And despite the pretences of many econometricians,

3 Matt Ridley, 'The BBC has co-opted bad weather to its alarmist climate crusade,' Blog post, July 19, 2023 (https://www.mattridley.co.uk/blog/the-bbc-has-co-opted-bad-weather-to-its-alarmist-climate-crusade/)

government officials and thinktank pundits, no one can possibly know the answers – at least not with enough confidence to justify overturning the presumption of liberty that ought to restrain government interventions into the organic processes of free societies.

The ability in textbooks and in academic seminars to describe theoretically what market imperfections look like and to explain how a godlike state would 'correct' these imperfections is, frankly, child's play – or, at least, sophomores' play. But it is pretentious to suppose that flesh-and-blood human beings' ability to offer such theoretical descriptions thereby supplies these mortal creatures with the ability to gather all the detailed knowledge that is necessary to render these descriptions as practical guides to public policy.

Therefore, to oppose state actions that are meant to 'internalize externalities,' or to object to industrial policy that promises to create 'the industries of the future,' is not to oppose science. Quite the contrary. It is to rationally recognise the limits of human knowledge and to establish *as a matter of science* that the likely best means of ordering humanity's material affairs is through the decentralised market economy rather than through government commands and controls. Do not forget that Hayek, 29 years before he won the Nobel Prize, offered the still-definitive explanation of how a decentralised market price system makes remarkably effective use of the immense amount of knowledge that is dispersed among countless different human minds (Hayek 1945).

The second flaw in the portrayal of the economy as an object to be engineered is revealed by a paper Hayek published ten years before he was awarded the Nobel Prize. That paper is his 1964 *New Individualist Review* article 'Kinds of Order in Society' (Hayek 1964).

Hayek there distinguished 'organisations' from 'orders'. Organisations are consciously designed arrangements by, and of, human beings, with each person having an assigned task or set of tasks *all meant to achieve a particular outcome*.

Among the most common organisations in modern commercial society are business firms. Consider a restaurant. An entrepreneur purchases space, kitchen equipment and other inputs, as well as hires workers, all for the purpose of earning as much profit as possible through the sales of meals to customers. The owner consciously chooses how the restaurant will operate – for example, what sorts of food to offer on the menu, the restaurant's décor, opening and closing hours, and the number of workers to employ. The owner also institutes various rules to govern the restaurant's operation, among the most important of which specify the tasks that each employee is expected to perform and the amount of discretion each employee will be given to respond to whatever particular detailed circumstances each of the employees will encounter while on the job.

Because the restaurant's owner has a particular, identifiable goal, the performance of all of the restaurant's inputs – including those of its employees – can be

evaluated 'scientifically'. To what extent does the current performance of each input contribute to, or detract from, the pursuit of the restaurant owner's goal? The end is given (maximum possible profit), and so the means can be objectively assessed.

Modern society and the modern global economy, in contrast, are *orders*. Orders differ categorically from organisations. While orders, like organisations, possess observable orderly patterns of behaviour, actors within orders pursue ends of their own choosing that are in no way meant to promote any higher end. Well-functioning orders improve the prospects of people to achieve each of their various individually chosen ends, but, crucially, there is no one end – no particular overarching goal – towards which each of the individuals acting within orders can be said to be working to promote.

Because a modern economy, being an order rather than an organisation, has no goal, the performance of an economy cannot be evaluated scientifically in the same way as can an organisation. The actions of consumers, workers, investors and entrepreneurs cannot be judged to be correct or incorrect, or better or worse, with respect to the economy's goal, as the economy has no goal.

No one designed today's division of labour, with some of us working as plumbers, others as web designers, yet others as butchers, brewers, bakers and basketball players. And no one designed the indescribably complex pattern of exchange relationships that enable each of us to enjoy the fabulous prosperity that we all enjoy. These

phenomena, nevertheless, are real. They are – to use a phrase much loved by Hayek – the results of human action but not of human design. The market, like language, provides enormous assistance to each of us as we each pursue our own individual goals. *But the market, also like language, has no overarching goal toward which it aims.*

From this fact it follows that now-popular phrases such as 'common-good capitalism' are either vacuous or dangerously illiberal[4]. If all that is meant by 'common-good capitalism' is a market order that enables people to achieve as many as possible of their individual and diverse ends, the phrase means nothing more than is meant by Hayek and other champions of the market order when they endorse 'capitalism' unprefixed. But if instead 'common-good capitalism' refers to a particular set of *specific* outcomes – for example, high and rising employment in the manufacturing sector – then this 'common good' is treated as an objective goal of the economy as a whole. The economy is conceived of as an organisation, with each component part being judged by how well or poorly it contributes to the achievement of the overall goal.

It then becomes a question of science whether any specific resource is being used correctly or incorrectly. Particular uses that do not promote the economy's putative goal are *objectively* wrong. And by diminishing the economy's

4 See, for example, Michael Posner, 'Marco Rubio's "Common Good Capitalism" Is Just What We Need,' *Forbes*, 12 December 2019 (https://www.forbes.com/sites/michaelposner/2019/12/12/marco-rubios-common-good-capitalism-is-just-what-we-need/)

capacity to fulfil its objective function, these objectively wrong uses of resources are credibly condemned – by *science* – as anti-social. From this condemnation, it is a short leap to the conclusion that objective science not only condones, but requires the forcible redirection of that resource into a use that makes a greater contribution to the economy's goal.

Starting with his participation in the 1930s' socialist calculation debate, a major theme of Hayek's life work was to warn against the danger of misconceiving society and the economy as organisations that have specific goals towards which all individuals can and should strive. This misconception makes society and economy falsely appear to be science projects with objectively correct, scientifically discoverable 'solutions'. Individuals become means – things to be directed by state officials who, guided by science, are engineering all of our actions for the greater good. And critics of such direction are easily silenced, or at least discredited, with accusations of rejecting science.

In this age of ours, in which respect for genuine science is welcome and widespread, anyone who persuades large numbers of people that his or her preferred economic arrangement is not a personal preference but a scientifically objective 'truth' can dispense with democratic politics and compromise. Just as we do not hold popular elections to determine the temperature at which water at sea level boils, we should not hold popular elections to determine which goods and services should be produced and how these outputs are to be produced.

Nor should we leave these outcomes to the free market, in which each person pursues his or her own goals heedless of the common good. These are scientific questions that must be left to the experts.

And because 'Science is Real,' persons who question the wisdom of turning resource-allocation decisions over to the experts are knuckle-dragging science deniers who are justly silenced lest they spread their mysticism in ways that prevent the use of science to engineer humanity to its economic bliss point.

Science is indeed real, admirable and immensely useful. But it cannot tell us what our preferences should be and how we should make the trade-offs that are unavoidable in our world of inescapable scarcity. Science cannot tell us whether or not, and how, to lockdown during pandemics or how much carbon in the atmosphere is 'optimal'. Nor can science tell us what to produce or how to produce it.

Today's globe-spanning economy is a forum in which individuals make their own trade-offs as they judge best and strike compromises through the price system, with billions of strangers about just how to use the countless economic outputs and inputs. Economic science, properly done – as Hayek did it – discovers and describes the logic of the market. But the operation of the market itself is not a science project, and tyranny lurks in attempts to falsely portray it as such.

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The Pretence of Knowledge*

F. A. Hayek

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, 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

**Hayek's Nobel Prize Lecture given in Stockholm on 11 December 1974 is republished here with the kind permission of the Nobel Foundation.*

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 'scientistic' 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 scientistic 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 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.

1 'Scientism and the Study of Society', *Economica*, vol. IX, no. 35, August 1942, reprinted in *The Counter-Revolution of Science*, Glencoe, Ill., 1952, p. 15 of this reprint. [Note: all footnotes are Hayek's.]

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 labour 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 labour in all sectors changes of relative prices and some transfers of labour 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 labour. 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 labour, 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 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

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

3 See 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.

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 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 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

4 V. Pareto, *Manuel d'économie politique*, 2nd. ed., Paris 1927, pp. 223-4.

5 See, 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.

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!

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 pretence 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 labour 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 disorganisation 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 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 scientistic 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

6 See *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'.

7 I 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.Brg. by J.C.B. Mohr, Tübingen 1975.

for giving us a test by which we 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.

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Hayek's Nobel 50 Years On

Hobart Paperback 222

"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." – F. A. Hayek

In 1974, the Royal Swedish Academy of Sciences took the controversial decision to award that year's Nobel Prize in Economic Sciences not just to an establishment figure (the left-leaning, social democratic economist Gunnar Myrdal), but also to a radical outsider: the classical liberal economist Friedrich August von Hayek, a leading proponent of the Austrian School of Economics. It marked a turning point. After years in the wilderness, free-market thinking was finally coming back in from the cold again.

The lecture Hayek presented at the award ceremony, entitled The Pretence of Knowledge, was a warning against what he believed to be the hubris of the economic establishment. Hayek's spirited admonition against those who would reduce human hopes and desires to mathematical models is all too timely today, given the resurgence in various forms of state planning and economic interventionism.

This book contains the full text of Hayek's lecture along with essays by leading scholars of Hayek's work. **Bruce Caldwell**, author of an acclaimed Hayek biography, provides some context about what the Nobel Prize meant for Hayek's career and professional recognition at the time. **Donald Boudreaux** shows us some contemporary examples of this misuse of science, in the form of a naïve 'scientism' that Hayek warned us about half a century ago. **Peter Boettke** shows that the 'new' socialism is just as flawed as the old one, and no more robust in the face of the Hayekian critique.

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