

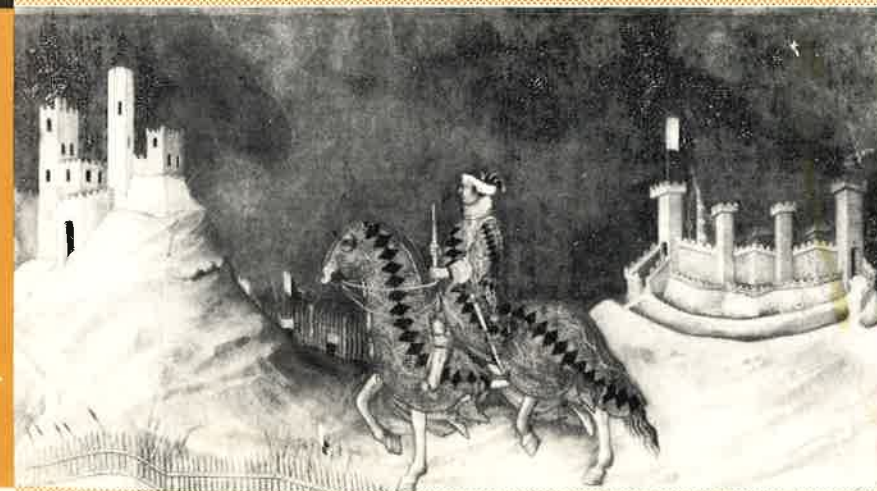
UNIVERSITÀ DEGLI STUDI DI SIENA



QUADERNI DEL DIPARTIMENTO
DI ECONOMIA POLITICA

**CELEBRATING R.M. GOODWIN'S
75th BIRTHDAY**

edited by Massimo Di Matteo



**QUADERNI DEL DIPARTIMENTO
DI ECONOMIA POLITICA**

COMITATO SCIENTIFICO

MARCELLO DE CECCO
MASSIMO DI MATTEO
RICHARD GOODWIN
SANDRO GRONCHI
MICHIO MORISHIMA
MARIO NUTI
LIONELLO PUNZO
SILVANO VICARELLI

Redazione: Dipartimento di Economia Politica, Piazza S. Francesco,
17 - 53100 Siena - tel.0577/298620

La Redazione ottempera agli obblighi previsti dall'Art.1 del D.L.L.
31.8.45 n.660

Le richieste di copie della presente pubblicazione dovranno
essere indirizzate alla Redazione

I Quaderni del Dipartimento di Economia Politica dell'Università di
Siena vengono pubblicati come servizio atto a favorire la tempestiva
divulgazione di ricerche scientifiche originali, siano esse in forma
provvisoria o definitiva. I Quaderni vengono regolarmente inviati a
tutti gli istituti e dipartimenti italiani, a carattere economico, nonché a
numerosi docenti e ricercatori universitari. Vengono altresì inviati ad
enti e personalità italiane ed estere. L'accesso ai Quaderni è approvato
dal Comitato scientifico, sentito il parere di un referee.

(Dal 1979 a tutto il 1987 la denominazione della presente Collana è
stata: "Quaderni dell'Istituto di Economia". La numerazione progressiva
rimane tuttavia la stessa).

CELEBRATING R.M. GOODWIN'S 75th BIRTHDAY

edited by Massimo Di Matteo



Siena, maggio 1990

Massimo Di Matteo is Associate Professor of Economics at the Institute of Economics, Faculty of Economics and Banking, University of Siena.

P R E F A C E

I group here the recollections and the interventions given at the University of Siena on March 25th, 1988. The celebration for Richard Goodwin's 75th birthday was divided into two parts. The first included personal recollections of friends and colleagues of Richard Goodwin. The second was a round table on "World Economic Crisis and Crisis in Economic Science".

The apology I can offer for the long delay in printing this booklet is that I wished to verify the forecast made by Paul Samuelson about the future course of the dollar!

As an Appendix I include a piece of mine (to appear in the Biographical Dictionary of Dissenting Economists, P. Arestis and M. Sawyer, eds., E.Elgar, 1991) where a scientific biography of Richard Goodwin is sketched.

Massimo Di Matteo

THE first of the two volumes of the
series is a history of the
United States from the
beginning of the world to
the present time. It is
written in a simple and
clear style, and is
suitable for the use of
schools and libraries.
The second volume is a
history of the world from
the beginning of the world
to the present time. It is
written in a simple and
clear style, and is
suitable for the use of
schools and libraries.

PART I

RECOLLECTIONS

Prof. L. B E R L I N G U E R

Rettore

University of Siena

E' per me fonte di grande gioia e soddisfazione aprire questa giornata in occasione del 75 anni di Richard Goodwin che, dopo una carriera luminosa e piena di successi negli Stati Uniti e nel Regno Unito, ha scelto di trascorrere il terzo periodo della sua vita di docente e di studioso nella nostra Università. Non sono certo lontano dal vero nell'affermare che tale decisione è stata fortemente influenzata, e direi sollecitata, dai colleghi del Dipartimento di Economia Politica, ai quali va il mio primo ringraziamento per l'onore che la presenza di un grande studioso come Richard Goodwin ha arrecato ed arreca al nostro Ateneo. Egli ha dato un rinnovato impulso all'attività scientifica e didattica, peraltro già valida e qualificata, del Dipartimento contribuendo a farne un centro di eccellenza scientifica in grado di attrarre studiosi da tutte le parti del mondo.

Alcuni colleghi del Dipartimento hanno anche praticamente organizzato, con il contributo della Unioncamere e della Camera di Commercio di Siena, ai quali va il mio ringraziamento, questo pomeriggio. In esso avremo sia testimonianze sull'itinerario scientifico e sulla personalità multiforme di Richard Goodwin che momenti di riflessione sulle prospettive sempre incerte dell'economia mondiale. Sono perciò riconoscente al Ministro dell'Industria e del Commercio, on. Adolfo Battaglia e agli studiosi italiani e stranieri perchè hanno voluto onorare con la loro presenza questa giornata. In maniera particolare

vorrei ricordare il prof. Wassily Leontief e il prof. Paul Anthony Samuelson che hanno entusiasticamente accettato di traversare l'Atlantico per celebrare il loro carissimo amico. Ieri abbiamo appreso che purtroppo il prof. Richard Stone è ammalato a Cambridge e che perciò non può essere qui tra noi come avrebbe desiderato: anche a lui va il nostro grazie.

Non sta a me parlare dell'opera scientifica di Richard Goodwin: altri più attrezzati all'impresa lo faranno tra poco. Vorrei solo ricordare il suo carattere aperto, la sua carica di simpatia umana che conquista tutti, anche coloro che come me non hanno con lui incontri di carattere scientifico. La sua passione per la politica ed il suo anticonformismo intellettuale, che gli hanno procurato, come è noto, qualche difficoltà nella sua carriera, sono certamente tra le sue doti più grandi che rendono la conversazione con lui sempre stimolante e mai banale. Questo suo atteggiamento lo spinge a visitare l'Italia (e la Germania) negli anni trenta per cercare di rendersi personalmente conto del fenomeno fascista e dei pericoli che esso rappresentava. Il suo amore per l'arte nasce anch'esso nello stesso periodo e da allora lo spinge a dipingere per tutta la vita: del resto, con un pizzico di civetteria, egli si presenta agli altri come un pittore prima che come un economista.

Negli spiriti più alti arte e ricerca scientifica sono forse due facce dello stesso atteggiamento intellettuale come dice Einstein in questo passo significativo:

"Se cerchiamo di descrivere la nostra esperienza all'interno degli schemi della logica, entriamo nel mondo della scienza; se, invece, le relazioni che intercorrono tra le forme della nostra rappresentazione sfuggono alla comprensione razionale e pur tuttavia manifestano intuitivamente il loro significato, entriamo nel mondo della creazione artistica. Ciò che accomuna i due mondi è l'ispirazione a qualcosa di non arbitrario, di universale."

Ed allora quale migliore descrizione di Richard Goodwin come di uno scienziato sociale-artista? La vera conoscenza -è stato detto- nasce dallo sforzo di cogliere gli elementi essenziali e di presentarli in maniera chiara e semplice. Un programma al quale credo Richard Goodwin sia stato sempre fedele.

Prof. G. R O L L A

Dean

Faculty of Economics and Banking

University of Siena

In occasioni come questa il rischio di oscillare tra la retorica ed il rimpianto è sempre in agguato. Proprio per evitare questo pericolo si è ritenuto che il modo migliore per celebrare il 75° compleanno del collega Prof. Richard Goodwin consistesse nell'organizzare un incontro di studio, nell'invitare illustri studiosi -italiani e stranieri- a testimoniare il percorso scientifico, le tappe più significative della sua attività di economista.

La presenza di così qualificati studiosi, di tre premi Nobel per l'economia -che ringrazio a nome di tutta la Facoltà per aver aderito all'invito- costituisce la testimonianza migliore non solo della validità dell'iniziativa, ma anche della grande stima e considerazione di cui Goodwin gode.

Toccherà ad altri tratteggiare il suo itinerario scientifico. Da parte mia, vorrei sottolineare soltanto due aspetti di natura generale, che mi paiono significativi.

In primo luogo, è per noi motivo di soddisfazione e di orgoglio la circostanza che la carriera di studioso di Richard Goodwin, snodata attraverso le prestigiose sedi accademiche di Harvard e di Cambridge, si concluda presso la Facoltà di Scienze Economiche e Bancarie; il fatto che si sia creato un collegamento ideale tra queste sedi, che mi auguro non si esaurisca.

La gradita circostanza che a questo incontro abbiano aderito illustri accademici europei e nordamericani testimonia, anche visivamente, come lo sviluppo della ricerca, il progresso degli studi necessitino di una dimensione internazionale, richiedano vasti e saldi collegamenti internazionali.

Chiusure autarchiche non sono possibili. Il cuore pulsante dell'Università non può che essere costituito dalla costruzione di una comunità scientifica senza frontiere. La nostra Facoltà con i suoi dipartimenti ed istituti ha bisogno, e ricerca, autorevoli collegamenti internazionali. Intende favorire tutte le iniziative che si muovono in questa direzione.

Per questi motivi si è impegnata, anche di recente, a realizzare i programmi Erasmus, a far parte del COIMBRA Group, ad intrattenere relazioni scientifiche istituzionalizzate con Università inglesi, canadesi, statunitensi, argentine, spagnole.

In questo spirito, ci auguriamo sinceramente che l'esperienza felice del rapporto con il Prof. Richard Goodwin non rimanga un evento isolato, bensì costituisca un esempio da ripercorrere. Ci auguriamo, cioè, che si creino quanto prima le condizioni perché altri illustri studiosi stranieri possano eleggere la nostra Facoltà quale sede accademica per il loro lavoro e la loro ricerca.

Ma quella di Goodwin non è stata soltanto una presenza emblematica, ma anche una presenza attiva e proficua. Grazie al suo magistero gli studi e la ricerca economica a Siena hanno incontrato nuove occasioni di stimolo; si sono formati giovani studiosi. Inoltre, va ricordato l'apporto prezioso che ha dato all'attività di formazione, dirigendo il Dottorato di ricerca in economia politica.

Per questa ragione ci preoccupiamo che questa esperienza non termini con la conclusione dell'itinerario accademico di Richard Goodwin. Vogliamo testimoniare la persistenza di un rapporto culturale e scientifico con adeguati atti accademici i quali -nel momento in cui Goodwin cesserà dal suo status professorale- evidenzino in modo duraturo, non effimero, la proficuità e l'importanza della sua esperienza senese; che esprimano anche visivamente la permanenza di un legame tra il contributo scientifico di Goodwin e le nostre sedi di ricerca.

Goodwin ha fornito un prezioso apporto alla qualificazione della nostra ricerca economica ed alla proiezione internazionale della nostra

Facoltà; e questo duplice contributo può trovare -nelle nostre intenzioni- un'evidenziazione tangibile, anche fisicamente, nella nostra biblioteca, che potrà contare sul ricco apporto documentale offerto dalle carte e dagli studi (anche inediti) di Richard Goodwin e sulla creazione al suo interno di un apposito fondo: scritti, documenti, materiali di ricerca che non potranno non qualificare e connotare l'intera struttura.

Prof. B. M I C O N I

Director

Department of Political Economy

University of Siena

Let me begin my introduction to this first part of what among us is known as the Goodwin-day, saying that I am very happy to represent the third department of economics, after Harvard and Cambridge (U.K.), to which Richard Goodwin has contributed.

Coming to us after Harvard and Cambridge he has given us some of the prestige of the two universities and helped to give our department a bit of the international flavour that we have always tried to have. Dick has shown here in his productive work and his everyday life, a very clear mind, a remarkable vigour and a very high productivity. Because of this, he has been a source of inspiration to many of us in our scientific work.

But he has achieved even more than this here. He has become friend to many of us. I think that this is due, on top of his abilities of which I have just spoken, to other aspects of his personality.

His political stance, his love for arts, and last, but not least, his expertise on wine all fit together rather well with the Sienese environment, and with the personal tastes of many of the members of the department.

As a last point I want to remark that in occasion of this day, I have realized that I am the person who has led the longest service

period in the department. If on one side this gives me a sad feeling of the quick passage of time, the same fact also gives me some justification in representing today, the department of economics and its continuity.

The continuity of the department of economics is also due to the many people who have been very active part in its building and in its running life and that have left the department. With many of them we have kept very close relations both as friends and colleagues, and we intend to go on on this road, in the future. I see many of them here today, and I give them my warmest welcome and thanks.

On behalf of all these people then I want to present Richard Goodwin our thanks and our best wishes.

Prof. W. L E O N T I E F
New York University

It is for me a great privilege to take part in this particular event, particularly, because possibly of all those present, except Jacky, I possibly knew Goodwin for the longest period of time.

I would like to talk a little about the environment, the time when I learnt to know him.

He spent, after all, a long time in Cambridge, Massachusetts. In a sense it was a formative part of his career, possibly, I would not say of his personality, but certainly his intellectual personality.

I remember very well when we met first. He had just returned from Cambridge, England. Later he returned back to England. It was in 1937, it was 50 years ago. He was still a student, an advanced student because he was at Harvard before I came in Harvard; I began to teach there in 1931, he must have been there about that time. He then already distinguished himself to be elected Rhodes Scholar. Rhodes Scholarship is a very important distinction, not a particular Harvard distinction. Distinguished students, young students are elected and given the possibility to go as Rhodes Scholars to Cambridge, England, to spend some time, studying, acquainting themselves, acquiring a Cambridge accent. These young scholars usually created a bond among themselves. When Dick returned to our Cambridge, I do not know what brought him to me. I considered him, if I may say so, to be one of my students, although he had naturally many other preceptors.

I am speaking now, at the present, not of his scientific formation, but of his formation as a person, the type of a life which he lived in Cambridge and as you know, I, Dick and Jacky had a marvellous ability to create an atmosphere, assemble people. Then, of course, he was still very young, he already created, we already created, a group of people who lived together intellectually, shared our ideas. In this group, we were not a majority of economists. I think that possibly majority were not the economists. Many were literary people interested in literature and art. Many of them later became quite well-known, even famous scholars in their respective field.

I still remember the Goodwins' little house on River Street, the name was River Street, a small old house that the Goodwins transformed in a nest. There was a fire-place. A group of people very often came there for supper. The fire was burning in the winter and the discussions continued hours after hours.

My wife Estella and I often visited the Goodwins in the summer. They hired a summer home, I remember, in Vermont, which is in the north of Cambridge. I and my wife had also a house in Vermont. It was a little house. Of course Goodwin moved there in a little car. It was a small Ford. It has a special name, as you know Dick likes, good cars of good breeding, but I do not remember Goodwin having an entirely new car. Usually they were broken in by somebody else, tamed, but they moved. That little Ford had a special name: Topolino. When the Goodwins went away, the car stayed in a little garage or sometimes in the street through all the frost. When Dick came back, he could work on it for an hour or two, get everything in order and it moved again. Let me say that one was so impressed by these things that even now, when I and my wife travel on the country roads, and see certain types of houses, we say to each other; "Now, this is a Goodwin house". For us, it became a special type of environment, one identifies it as a "Goodwin" house.

Now, of course Dick could describe all this much better, and possibly he will, I do not know, on this occasion. But he will excuse me, if I speak about these things.

There were of course, also, older people there, and I suppose, Prof. Schumpeter was nearly one of us, not quite, because a senior professor cannot quite be, but Schumpeter had also a marvellous

ability to approach people, to take care of them. Politically, he was extremely conservative, but this absolutely did not affect his relationship to social radicals as Dick. It was a kind of a second circle, again long discussions, intellectual discussions. Schumpeter was also very interested in art. When Dick returned from Europe, the first thing discussed were cathedrals. French cathedrals, Italian cathedrals. This gives you the atmosphere of that place.

Speaking now, more of a specific intellectual atmosphere, a little later came Prof. Hansen. This was after the great transformation, then in 1937 or a little later the Keynes's influence came in. I remember one of the youngest men, Bill Brice, who was before in Cambridge, England, returned back to our Cambridge and in an excited way presented the new ideas, the Keynesian ideas. He was a Canadian. Later he became Secretary of State in Canada, and now he is a major older statesman.

There was an incredible excitement in classrooms, in private conversations, in seminars. I think, possibly this then was when Dick got the bug. He got infected. Being a very active person, he became an important part of that movement. This was also the time when theory became more and more methodologically influenced by mathematical methods. Now, Dick, if I am not mistaken, was first an economist and then learnt his mathematics by using it. There is a very great distinction between people who first were mathematicians and then decided that Economics is a good field to show how good mathematicians they are. For him, Mathematics is a tool, is not the main purpose. He is fundamentally an economist who is interested in human affairs, trying to understand them. If I remember correctly again, one of his important contributions was to write an appendix to Hansen's book. Hansen was not a mathematician. He did not know mathematics, I think. Dick formulated his ideas and strengthened them, and of course, in the process developed them further.

The nice thing about mathematics, once you are there you can move faster and with more insurance.

Now these were the times when Dick used his mathematics also in a different way. Of course, whenever you enter a period when technology becomes important, mathematics gains in importance. Then

he even taught mathematics, if I am not mistaken, at Harvard to younger students.

Now, let me just shift to the year 1952, the first meeting on Input-Output Analysis of a small group of people in Driebergen, Holland. It was a small group of people, but there was, beside Dick who was one of the youngest people invited to it, there were Tinbergen, Koopmans and Frisch.

Later two of them were the first Nobel Prize winners in Economics.

There already, Dick presented a paper, which, if I am not mistaken, is included in one of the volumes of his collected papers. I think, there was already something in common between my scientific interests and his. Without going into technicalities, I think, one can say, one characteristic of it, is that one looks at the economy as a whole, one does not study small pieces of the economy, but one considers the economic system as a whole, in the same way as one contemplates a large cathedral.

One does not simply study separate parts, but all parts of it hang together; which of course is somewhat complicated. It is much more difficult to encompass the whole and here mathematics helps you, because mathematics enables you to study the part and see the whole. Very often, it is said, that one either can study separate trees, or one can see the whole forest.

With mathematics, and with Input-Output Analysis, one can really describe the forest in terms of separate trees. This was long before large electronic computers were invented. Of course, finally, the technology caught up and, one can do it very efficiently so that even practically one can use it with understanding, not only as an intellectual structure.

I am talking little too long, so let me go ahead.

I would like now to observe, that it is very natural, very gratifying, nearly inevitable, that Dick came to Italy as an economist. Because there is a great tradition of economists, of economics in Italy.

Prof. P.A. S A M U E L S O N
M.I.T.

Prof. Leontief is a very hard act to follow. I know our time is very limited and our subject is vast, but this day of tribute led me to jot down the names of some very great persons: Goethe, Verdi, Euler, Willard Gibbs, George Birkoff. I hope in the brief time allotted to me not to do full justice to Richard Goodwin, but to touch upon our friendship. I will work in every one of those names. First, let me say that while you here think of him as a person retired for the second time, I think of the young Dick Goodwin.

After all, I was in the Harvard graduate school years before Dick, when he was still sowing wild oats in Oxford and exchanging literary pleasantries with Roy Harrod. I and Lloyd Metzler and Abram Bergson, we were already working in the Leontief workshop. Let me say there are two kinds of reputation in a subject like Economics. There is an outsider reputation - perhaps some of you have heard of J.K. Galbraith, and Prof. Milton Friedman has actually a claim to two kinds of reputation. Dick Goodwin is known among connoisseurs. Many graduate students know and should know John Hicks' notes on the theory of the trade cycle, but it is only connoisseurs who know that, before 1950, it was R. Goodwin, teacher of physics during war time at Harvard, America, who under the tutelage of Philippe LeCorbeiller, also a person known to connoisseurs, mastered the technique and the deep analysis of non-linear auto-relaxation cycles. So, as long as our subject

persists, in the inner circle the name of Richard Goodwin will be known.

I must proceed very quickly. How do I work in Verdi and Goethe into this discussion? They were not young flashes in the pan. They improved with age, like certain good wines and although I think of Goodwin as still very young, his profile through time has been like that of Euler. Newton was different, for it was in the years of the Great Plague that he did most of his great things, whereas Euler all his life returned to basic topics, spiralling upward at a deeper level. If I may say so, part of the rejuvenation of young Dick Goodwin has been the University of Siena. A noticeable pick-up in the pace of his scientific contribution has occurred here. Now, with respect to Willard Gibbs. Gibbs was the first and only great American physicist. But at Yale University, it was his brother-in-law, the librarian, who was considered the great man. Gibbs actually received no salary in the physics department, until John Hopkins offered him \$2000, and which Yale matched. I understand that R. Goodwin has not retired from teaching. He is just like Gibbs, retired from teaching for money at the University of Siena.

Finally, I come to Birkoff who was America's first and at that time only great mathematician on the world level. When Poincaré was dying and made his famous conjecture, he said: "I will announce it and somebody must prove it." It was Birkoff who proved it and the French mathematician Picard would not believe that an American could have done it. G. Birkoff used to sit in the Harvard Faculty Club every afternoon and play bridge. A graduate student came to him and said: "Prof. Birkoff, how come you sit every afternoon and play bridge?". He said: "Young man, if you worked as hard and effectively as I do between 8 and 12 in the morning, you too could play bridge in the afternoon."

Now, I say this because R. Goodwin has not been known as your seven o'clock in the morning until 9.30 at night worker in the office. He was even dismissed as something of a dilettante when he shocked the Harvard Chairman of Economics who was offering him a handsome stipend as an instructor. Goodwin replied: "Sir, how much would I have to work in order to earn \$1,800 a year?". Goodwin is like the astronomer who has learnt that if you want to see best the most

distant star, you do not look right at it, you see it in your peripheral vision. Dick has maximized his originality and creativeness by taking the long and leisurely view. I would never in the world belittle the creativity and activity of someone like Harry Johnson. When Harry Johnson died, he had 18 papers in proof. I suppose, it would take a third of the century for Dick Goodwin too have 18 papers in proof, but the quality as well as the quantity is important.

Now, in concluding, let me tell you a story. It is a true story and Dick probably does not know this because, how would he? Ten days before Joseph Schumpeter died, he and I met, naturally, in the bar of the hotel where the American Economic Association was having its meeting. Schumpeter was 67 years old, in good form, at the top of his form. He had a great book not quite finished. Of course, he did not know that he had only ten days to live nor did I. It is called the gift to the Gods when you have a death like Schumpeter's, in his sleep, with his work well done, with Richard Goodwin ready to complete it. What we discussed in our final moments was this. Schumpeter said to me: "Paul, what are we going to do about Dick Goodwin?". I said: "What do you mean, what are we going to do about Dick Goodwin?". And he said: "Dick Goodwin, quite without regard to merits, has not been offered a tenure position at Harvard". So I went to Dick Goodwin, and said: "It is very unusual at Harvard, but there can sometimes be created a permanent non-tenure post- a fellowship like that of Roy Harrod at Oxford". But Dick Goodwin replied that, if he could not be a first-class member of the club, he would not be a member of the club at all. I did not have the wit to tell Schumpeter then what I can say to him now in whatever Valhalla he may be: "Do not worry about Dick Goodwin, his originality will score and will be recognized, if not on the cover of Newsweek, in a more important place among the economist connoisseurs who treasure the history of original contributions to our subject."

Hail, Caesar!

Prof. R. S T O N E

Cambridge

A LEAF FROM PLUTARCH

When I look back on Richard Goodwin's career and mine I sometimes wonder why, friends as we are, we never collaborated. Our aims as economists are the same, our skills complementary and for over three decades we have lived in the same town. Together we might have produced some masterly works that would have astonished the world. On and off we have talked about it, but when it came to the point we never got down to it. Collaboration, of course, is a tricky business and perhaps some instinct told us that remaining friends was more important than astonishing the world. Still, it is odd that we never even tried.

This point struck me again forcibly when I read Dick's paper "A personal perspective on mathematical economics" which appeared in the Quarterly Review of the Banca Nazionale del Lavoro in 1985. A brief parallel history of our professional lives will show you why.

Dick and I were born in the same year, 1913; he at Newcastle, Indiana, I in London. In 1930 Dick went to Harvard with the intention of studying law, following the wish of his grandfather, a lawyer turned banker. In the following year I was sent by my father, also a lawyer,

to study law at Cambridge. Dick started to read political science as a preparation for the law but soon gave up the idea of becoming a lawyer; he sampled a number of other subjects and eventually narrowed his choice to economics. I, after two years of law, had a nervous breakdown and persuaded my father to let me switch to economics.

Like other people who reached university at the onset of the great depression, Dick and I both believed that economics could provide the key to an understanding of events. He was dissatisfied with orthodox analysis, however, and wrote his first degree dissertation on Marxism. I had my Marxist period too but it did not bite so deep; my crab with orthodox economics was that it was hardly quantitative at all. I longed to fill some of Clapham's "empty economic boxes". No doubt demand depended on prices, but was the effect of a given price change large or small? And if there were several influences pulling in opposite directions what could one say unless one knew the strength of the pulls? But nobody seemed to know and few to care, so I found the subject somewhat mysterious: a splendid bag of tools that could not be used.

After graduation our activities diverged considerably. Dick remained in the academic world and took up economics in earnest. He got a Rhodes scholarship, went to Oxford, read PPE, was supervised by Roy Harrod then working on the trade cycle, and, most exciting, was allowed a preview of the General Theory in proof. Meanwhile he prepared his doctoral dissertation for Harvard by researching into the money supply in England and Wales, a noncontroversial subject which he thought would be more acceptable than Marxism to the people on whom his career depended.

I did not have such clear ideas about my future. My college, Calus, offered me a postgraduate studentship but I did not take it up, partly because my father was anxious to see me settled in a job and partly because I had doubts about my readiness for research. In the end I went into a firm of Lloyds brokers. I learnt a lot from that experience but insurance was not my line. What I liked to do and did in my spare time was in fact research, and what I researched into was of course quantitative economics. I was encouraged in this by Colin Clark, who had been the principal influence on my undergraduate

studies and who in 1937 handed on to me the production of a little monthly called Trends which he had started and which consisted entirely of economic time-series.

So there Dick and I were in the late thirties, two eager young men with a faith in economics, both for different reasons dissatisfied with what we had been taught and both wanting to do something about it. There was this new thing, econometrics, which had recently been put on the map by Frisch and others, and we both saw in it the tool we wanted. Neither of us had had a proper mathematical training, however, and so in order to overcome this handicap we both became, in Dick's phrase, "Sunday mathematicians".

The advent of war gave both our careers an unexpected twist. Dick was back at Harvard, lecturing on economics, writing up his thesis on the money supply and absorbing wisdom from Schumpeter whose pupil and friend he had become, when there was an urgent call for people to teach physics to army and navy officers, most of the university physicists having disappeared into the American military machine. Dick, who knew nothing about physics, gallantly volunteered. In a very short time he learnt the rudiments of the subject and taught it from 1941 to 1945, continuing to learn as he taught. It was hard work but rewarding in that it opened to him the rich field of applied mathematics.

At the outbreak of war I was sucked into the civil service as a statistician. After a year at the Ministry of Economic Warfare I was transferred to the Cabinet Offices to help James Meade fill in with figures some tables he was drawing up for the purpose of financial planning. The result was a mini set of national accounts which Keynes, who was then at the Treasury, got published in a White Paper which accompanied the 1941 budget. I spent the rest of the war in the Central Statistical Office keeping the series of income and expenditure up to date, and doubled this with acting as Keynes' assistant.

By the end of the war Dick and I could say that we had served our apprenticeship. The years that followed were productive ones for both of us. Dick's work centred on the construction of models of interdependence and change and included his famous papers on the flexible accelerator and the matrix multiplier. I was busy on national accounting and the analysis of market demand.

We met for the first time in September 1950 at the International Conference on Input-Output Techniques at Driebergen in Holland and again at the European Meeting of the Econometric Society at Varese in Italy. We hit it off immediately and I suggested that he might come and work at the Department of Applied Economics in Cambridge, of which I was the director. He did come for the year 1951-52 but our plans for working together never materialised, for almost at once he was offered a teaching post by the faculty and then he had other things to do. He became a Fellow of Peterhouse and a Reader in the faculty and remained at Cambridge until we both retired in 1980. To my everlasting chagrin, Cambridge never offered him a chair; to my delight, Siena showed better sense.

Dick and I have always had pretty much the same conception of what economics is all about but our strategies have differed. Dick has approached the subject from above, the bird's eye-view, and concerned himself with the problem of how properly to represent economic systems given the complexities which beset them: economic events are often substantially unique; unlike hydrogen atoms, human beings are all different; the components of an economic system exhibit a great deal of interdependence and so we cannot understand the system by studying them in isolation; tastes and techniques are always changing, and with them the parameters of any model. Such considerations have, I think, led Dick to concentrate on appropriate forms for models, as in his work on fluctuating growth and in particular his 1965 paper on "A growth cycle".

By contrast, I have approached the subject from below, the worm's eye-view, starting with observations and generally accepted relationships and seeing how far one can get by combining them. I am less daunted than he is by the kind of difficulties just mentioned. I think we can deal reasonably well with interdependence, and opt, as he does, for a disaggregated dynamic model of the whole economy, thus steering a middle course between the excessive detail of attempting to deal with individual transactors and the excessive bluntness of recognising only major totals like income, consumption and investment. As to change, I agree with Dick, and for that matter with Edmund Burke who said "We can never plan the future by the past": but I would prefer to say "the past alone". In many respects things do not

change all that quickly. The past tells us a good deal about new ideas that have already surfaced and about the way the input-output matrix is likely to change as a consequence. It does not, however, tell us anything about the future effects of ideas that are still in the pipeline between the laboratory and the factory. Dick, heroically, tries to throw light on this problem by devising evolutionary models.

I, less heroically, think measurement would also help. It should not be impossible for statistical offices, without unduly infringing legitimate business secrecy, to get some information on these really new ideas and their probable rate of adoption. Such information, when fed into Dick's models, would enable us to look ahead in a manner not constrained by what is visible from the past.

Does all this mean, then, that Dick, to use his own language, is "an ivory tower dweller" and I "a vulgar empiricist"? I hope not. But perhaps it explains why we never collaborated. Be that as it may, I am very grateful to the University of Siena for giving me this opportunity of expressing publicly the worm's admiration for the bird.

Prof. R.M. S O L O W
M.I.T.

An American writer once called the art of boxing the sweet science. We all know what Economics has been called. But to those of us who have tried to learn to do economics the Goodwin way, with sharpness of vision, and lightness of touch, our science is anything but dismal, and may even be sweet. Dick has been a role model for free-spirited young economists in three great centers of research. Here is one no-longer-young student who wishes he could be with Dick and his others admirers today.

Prof. M. M O R I S H I M A
London School of Economics

At the beginning of my life I was very much departed from Dick Goodwin. I was on one side of the Pacific Ocean, he was on the other. Moreover we were fighting each other. Fortunately, however, when I entered the university in 1942, we could read Samuelson's first stability paper in Japan. As an undergraduate student, I started to read Hicks' "Value and Capital". Soon I was mobilized to the Navy, but I was lucky to be able to continue to think about "Value and Capital". Therefore as soon as I came back from the Navy to the university, I was able to read Econometrica papers by Metzler and others. Being stimulated by Samuelson's original paper, I was interested, very much, in Metzler's problem. But, I thought that Metzler's proof was incomplete. Therefore, I wanted to have an alternative complete proof, and I searched a lot of mathematical theorems. I found Frobenius' theorem on nonnegative matrices which later became very popular among economists. A few years later, I saw a lot of works by Richard Goodwin in the Economic Journal and a paper, he delivered to a conference held in the Netherlands, which Leontief mentioned. I was excited to find that he had also used the Frobenius Theorem.

Later I went to Oxford. As my English was terrible, I naturally hesitated to go anywhere abroad, and I wanted to study in Japan. But my friend, who was one of Samuelson's disciples, told the Rockefeller Foundation that I had an intention to apply to the Foundation's

Fellowship. I could not say to him that he should not do that without my permission. Fortunately or unfortunately, a director of the Rockefeller Foundation came to Osaka to interview me. The resistance that I made was that I did not speak English. I always spoke in Japanese and showed how unable I was to speak English. Finally, the director asked me: "What is your research interest?". Then, I said only two English words: "Economic Dynamics". As I answered in that way the Director of the Rockefeller Foundation said: "All right, you can go." Then, I made another resistance by telling him that I only wanted to go to England because I started economics with "Value and Capital", and was really interested in Professor Hicks. With this request, as the Rockefeller Foundation is an American foundation, I thought the director would refuse to accept my application. But contrary to my expectation he said instead: "All right, you can go to Oxford. But I make one condition. On the way back to Japan, you should go through New York and visit the Foundation." I had to agree.

It was July 1957, and I visited R. Goodwin in Cambridge. It was a lovely beautiful day. I thought that Cambridge was more beautiful than Oxford. Goodwin showed me Peterhouse, the room of a nephew of John Maynard Keynes and then he showed me his paintings. I did not know on that occasion how he loved paintings. Later I thought about that, and considered that that should be Goodwin's disease. Because of his love of painting, I think he did not publish, say, at least 10 papers which he could easily publish. Because of that, his number of publications is relatively small compared with those economists who could have worked until the age of 75. And this policy of minimization of the number of published papers I believe, should have given rise to maximizing the quality of his papers. I do not believe in the age theory of scholar's productivity. The age theory means that a scholar's productivity depends on his age "T". I rather believe in the marginal productivity theory concerning scholar's work, according to which the value of the work depends not on the age but on the number of papers previously published, in the past. If so, a scholar who has constantly worked throughout his life has inevitably, as he becomes older, a big number of papers he published in the past. Therefore the marginal productivity is not distinguishable from the age theory, in this normal case, so that in order to identify the validity of the

marginal productivity theory, we must have some special cases, for which very special experiments are needed. And one experiment which is known is the case of Weierstrass, who is a great mathematician but a late comer. He studied mathematics at the age of 40 and his productivity was very high, as high as young able mathematicians of the age of 20. Among economists Wicksell was a late starter. But these persons lack products in their twenties, while Richard Goodwin offers a rare precious example. He was very prolific in his 20's, then took ten years' sleep, and finally he woke up and wrote a beautiful article on the "Growth Cycle Theory". It is a masterpiece which immediately connects with his works before sleep. When we read Goodwin's works successively, no one will notice any sign of ten years' sleep between these works. We can only observe a smooth continuity of his interest and a remarkable conservation of his brilliance.

I think, during the recent several years after Cambridge, he would have been most happy, perhaps because of the Tuscan wine and Sienese natural and academic atmosphere. Not only did he produce books, at least one every year, but he also produced a lot of disciples. Siena had once a very prosperous era when talented professors such as Izzo, Romani, Gandolfo, Amendola, Zaghini, De Cocco were all in this university. It was a golden-age of Siena. However, they all unfortunately disappeared to Rome. This position of Siena is similar to the position of LSE where I am working. LSE produced or collected many first rate able young professors, but we have lost Oliver Hart to M.I.T., David Hendry and Steve Nickell to Oxford, Willem Buiter to Yale, Partha Dasgupta to Cambridge and many others. We may still lose more. This is a tragedy of a school of a corridor type. However, we have produced the second generation. In the same way, Siena succeeded in producing a second generation of professors under the leadership of R. Goodwin. He should be said to be very lucky and happy in having such fruitful academic years in Siena.

Congratulations.

Prof. A. LEIJONHUFVUD
University of California Los Angeles

So far Richard Goodwin has been addressed by people more or less of his own generation, by people who have known him for several decades, by people who belong in the same league as he, that is, in the Big League of mathematical economic theorists. I qualify on none of these accounts and have no other reason for being here than that Dick Goodwin and I have become friends. It is my privilege and I am happy to participate in this celebration of him.

I am one of those who had to get to know Richard Goodwin first through his works. In my first encounters, I was not able to infer very much about the man, perhaps because I had no training in mathematics and had to struggle hard to understand his papers. For the students of my generation, in the 1950's, there were other non-linear business cycle models around to contend with those of Samuelson, of Hicks, of Kaldor, of Joan Robinson, for instance. But in my case at least, it was in Goodwin's papers that I got my first horrified glimpses of what a non-linear dynamic system can do to you. I do not know whether I can convey to you what the impact of non-linear dynamics on a non-mathematical mind is like, but a memorable scene from a film of that period may help to convey the flavor. In it, the great French comic actor, Fernandel, does a parody of an unsavoury underworld figure playing poker with three other dangerous-looking characters. Much money changes hands, and so on,

but eventually they get bored and decide to play the most dangerous game of hazard known, *le mouche*. In this game, a sugar cube is put in front of each player; then a fly is caught and put under a glass in the middle of the table; each player bets all he owns in this world; the player on whose sugar-cube the fly alights first will win all. The glass is lifted - and there follows a long scene of incredible tension as no one is allowed to interfere with the fly in any way or even to move; cold sweat is breaking out on every brow as the players follow with their eyes the incredibly non linear path of the fly.... This is what it was like, thirty years ago: if you wanted to become a macroeconomist, you had to bet your human capital on understanding these non-linear Goodwin models.

It was much later, in 1974, in Cambridge, that I first met Dick Goodwin. He turned out to be a much less dangerous character than might have been inferred from this early experience. Indeed, he was extraordinarily nice to me, and we have been friends ever since a for me very memorable dinner at Peterhouse at which Goodwin presided as the most eminent of Cambridge's wine stewards. I remember vividly how wine stewards of other colleges and other connoisseurs present lined up in a long queue to pay homage to him after dinner.

One of the things we had in common at that time was a little paper of his, "Iteration, Automatic Computers, and Economic Dynamics," that no one has mentioned on this grand occasion, but which I had unearthed where he had pretty much hidden it from American eyes - in *Metroeconomica*. In this paper, Goodwin analyzed "the market mechanism" as a feedback-governed homeostat. I had used this paper in my undergraduate classes for years and eventually wrote a similar little paper, based on this, in which I described the market as two coupled homeostats, rather than one, one of them governing price and the other governing the output rate. (I hid mine better than he did his, in *Intermountain Economic Review*, a journal started by a friend from graduate school, which does not seem to have become a house-organ in Italy).

I mention it, not so much because we talked about it quite a bit at the time, but because I would like to leave Richard Goodwin with a question to contemplate in his retirement -hoping, of course, that he will report to us the result of his reflections on it. It concerns the

relationship between that little market mechanism of his, on the one hand, and his work on aggregative non-linear systems, on the other. The connection is not all that clear. One might take off in two different directions with business cycle theory from these two interests of Goodwin's. In my own case, I have approached the subject mostly by asking: How is it that a system consisting of a great number of interconnected market mechanisms, like Goodwin's deviation-counteracting homeostat, may fail to dampen cycles? Why is it that the market system sometimes behaves badly? I have been, I should confess, less interested in aggregative non-linear models because they seemed to give me no clue to this question.

Wicksell's rocking horse comes to mind. In discussing business cycles, Wicksell imagines the economic system as a rocking horse that was being hit by exogenous shocks and thus set in fluctuating motion. You could not predict when these shocks would occur, but by studying the curvature of the runners and the distribution of weight on the horse, you might be able to make scientific statements about how the horse would behave if hit by a particular impulse, and so on. This image of Wicksell's suggests that one should draw a line between exogenous impulse and endogenous propagation mechanism which, in practice, usually amounts to a nice way of saying that one draws a line between what one hopes one can explain and what one would rather leave to others to explain. This line is clear in the micro-context of the "market mechanism" model, it is frequently unclear and problematic in the macro-context of those non-linear aggregative models that produce sustained fluctuations "on their own". I hope one day Dick Goodwin will come back to the two and give us his reflections of how these two approaches can be unified. For many of us that remains a mystery and we believe, of course, that -retired or not retired- there is hardly anybody who is a better candidate for helping us out of the woods on this question than Richard Goodwin.

Prof. L. P U N Z O
University of Pisa

I am embarrassed to be in the company of such distinguished economists. I cannot claim to have known Richard Goodwin for as many years as profs. Samuelson, Leontief and Morishima. However, since he came to Italy in 1980, I have been in close contact with him. I have had the pleasure of his company, the privilege of his scientific support and the unique opportunity of observing a great man at work. As I also had the privilege of being one of his colleagues in the first eight years of his Italian adventure, I am expected to speak first in this capacity.

I believe it is important to recall how it all came about: how the Sienese began to know Richard Goodwin not only from his writings but also in person. The Faculty of Economics and Banking with which Goodwin has been associated, was only founded at the end of the 60s, by a group of young economists mostly from the University of Rome. Bruno Miconi has already mentioned some of these people. We thank them of having joined us today. They gave this Faculty a strong international orientation. The Siena Faculty was largely conceived as an experiment in the process of radical restructuring of the teaching of economics in this country. As part of this experiment, they established a college-like organization for the students at the Certosa di Pontignano and launched a program of international contacts and seminars. Goodwin began a series of regular visits to our Department. I

think this was at the very end of the sixties; I do not remember exactly, being a Sienese of the third or fourth generation. Siena soon acquired the fame at least in Italy, of being an international meeting place, where an undergraduate in economics not only studied but could also actually see and, it was whispered, sometimes even meet such mythical economists as Goodwin, but also Morishima and Hicks in person. It was this very fame that convinced many of us of the latter generation to leave the sweetness of their home towns and the care of their anxious and protective Italian mammas and enrol in Siena. I do remember one of Goodwin's seminars in the early seventies, it must have been around 1974, about that difficult contribution he made to the theory of linear structures that is known by the awful but technically appropriate name of "economic theory in generalized diagonal coordinates". We students were not put off by the difficulty of the subject he was so keen to teach us: the man was so fascinating.

So it had to be a man of the latter generation, one of the members of these fascinated audiences, who conceived the grand plan of getting hold of Goodwin, not just as an occasional Spring speaker, but actually as a colleague. Ugo Pagano conceived this plan and with his typical attitude and persistence spotted in the new University bill a rule or rather a phrase, according to which a foreigner could be appointed at an Italian University as a full professor. At Pagano's instigation, Goodwin, who was then retiring from Cambridge, U.K., competed for a chair at this University and was obviously appointed. It was the first time that this had happened in modern times. Let me say that the event was also of more general significance, not only at this University and because of the stature of the man. It was significant for the Italian academia as a whole. An old tradition was revived: the medieval tradition of scholars that had no frontiers and no homes but went to teach and learn wherever science would call. Italy always adhered to this tradition, until recent unfortunate times; with the appointment of Richard Goodwin the tradition was retrieved. It is nice to think that he is now retiring in the year in which Italy is celebrating the 900th anniversary of one of her Universities, Bologna, which can rightly claim to be the oldest University in the world. It is not fortuitous that we have here today some distinguished representatives of that sister Institution.

In the thirties Richard Goodwin began his career as an applied economist in the field of monetary economics, but no one here knows him for that. We all know him as a mathematical economist, for his work in linear and nonlinear dynamics, in capital and distribution, in cycle and growth theories. His contributions belong to the standard box of tools that is taught to economics students and is used in the daily work of an economist. But there is more to his life than economics. The interested person should consult the wonderful booklet in which Goodwin narrates in the form of an interview the incredible intellectual adventure that began in a small midwest town in Indiana and continues in this ancient and beautiful town in the centre of Italy. I should mention that the interview was put together by Dr. Maura Palazzi from the University of Bologna, and published in a series edited by prof. Guido Candela. They are both here with us today.

I need not remind you of all this, rather I shall refer to Goodwin's work since he joined us in Italy. It is a favorite story he tells that he has had three lives so far: the American one, the English one and the Italian one. It is by generalizing this notion that Velupillai produced his model called the Nonlinear Life of Richard Goodwin. I will limit myself to this third life so far, to use Goodwin's own expression, the life that began with a catastrophe, in the proper sense, induced by the fervid imagination of Ugo Pagano.

I believe that when he came to Italy there was a lingering idea in the back of his mind that he was going to have plenty of time to devote to his other parallel love. We all know that Goodwin is also a painter, and somebody here has a nice story about this. However, his time in Italy has all, or almost all been spent in scientific research.

The Italian life has been a wonderful mature summer, like the sunny Sienese month of August, rich with the ripe fruits of a lifetime of constant thinking but parsimonious publishing. Goodwin's work has followed three main lines: the theory of long waves (resurrected from the ashes of Kondratieff), the theory of technical progress and Schumpeterian innovation and finally his most recent love, the theory of chaotic motion of deterministic systems, as part of the theory of an economic system as a complex entity. Goodwin's scientific production has been enormous; while gathering the results of a lifetime, he was always also trying new lines of development. In eight years, he has

managed to add a whole new volume to his previous two volumes of collected essays, plus a whole book, and numerous articles appearing in leading professional journals.

In a way, Goodwin is thus a living example of a Schumpeterian innovational process, the imprint of his Austrian teacher has been so deep. He has now taken to computing and simulation which he does with youthful energy and dedication. If one walks into his house in the Sieneese countryside at any time of the day and most of the night, he is at his desk, bent over his personal computer, trying out new equations and new phase portraits. Printouts pile up on his sofa at such a speed that there will soon be nowhere for you to sit.

I have spoken of Goodwin as a colleague and as a mathematical economist. I hope I shall be forgiven if I also add a personal touch to this brief speech by recalling the circumstances that brought us together. I entered the picture while Goodwin was quietly enjoying his second life, without any forewarning of the impending Sieneese catastrophe. I had previously only been an admirer of the man: after that I also became a friend. I had just come out of the London School of Economics, where prof. Morishima had taught me all about the von Neumann revolution. The analytics of that model intrigued also me from the historical point of view: how could von Neumann have conceived such a model? I went to Cambridge, and more or less directly asked Goodwin. Goodwin had only indirect knowledge of the historical facts, but was intrigued just like me. He became a partner in this research, and put me in touch with Nicholas Kaldor. Kaldor had known von Neumann personally and had a lot of stories (or histories) to tell. So, I interviewed Kaldor a number of times, then Champernowne, then I approached John Hicks. Almost every afternoon I would turn up at Goodwin's door in Cambridge, to go for a walk and talk about this police story. In these animated chats, with the complicity of an unusually mild English Autumn, a plausible story took shape and a profound friendship was born. Goodwin told me, among many other things, that Schumpeter himself had directed his attention to von Neumann's paper not too long after its original publication in German. During these walks, Goodwin expressed his regret at not having seen and exploited the potentialities of that model. He explained that he disliked von Neumann's growth model because of one of its features,

joint production. But I believe that the reason lies elsewhere. The clue is in another sentence typical of Goodwin's conversation in those days: he said that he had never had any feeling for formal proof. What he meant by proof is proof of the existence of an equilibrium, which is precisely the content or essence of von Neumann's model. We know that modern general equilibrium and other similar contributions was born with that paper. I therefore surmise that the reason for Goodwin's rejection lies in a more general attitude to economics, its subject matter and its methodology. Goodwin sticks to the classical tradition of economics as political economy, as the macrodynamics of an actual economic system.

Today we are therefore honoring this tradition in the person of Goodwin. We all wish him a fourth life as intellectually rich as the previous three. We all hope we will have some part to enjoy in it.

PART II

WORLD ECONOMIC CRISIS AND CRISIS IN ECONOMIC SCIENCE

ROUND TABLE

A. B A T T A G L I A

Secretary of State for Industry and Trade

Io sono venuto non per partecipare alla tavola rotonda, ma per portare il saluto e il ringraziamento del governo a una persona che ha voluto dedicare una parte importante della sua vita di studioso e di maestro all'Università di Siena. Il segno lasciato dal Prof. Goodwin in questa Università e il suo profondo legame con la città dove, del resto, vive, ho potuto valutarlo parlando con i suoi allievi e leggendo, prima di venire qui, alcune sue recenti interviste. Sui suoi contributi in economia, altri si sono già soffermati in questa prima parte della vostra cerimonia. Mi lasci dire invece, Prof. Goodwin, che ciò che mi ha colpito è stata la molteplicità dei suoi interessi, la sua indipendenza intellettuale, se posso dirlo, che l'ha portata ad indicare nuovi problemi e nuove direzioni di ricerca piuttosto che seguire modi tradizionali. Accanto a ciò, la grande umanità e sensibilità che tutti le riconoscono e che l'hanno portato a circondarsi dell'affetto e del rispetto di tantissime persone qui a Siena, dove lei è forse più popolare, mi dicevano alcuni suoi colleghi, degli stessi professori della Facoltà di Economia che sono nati a Siena.

Ho letto che il Prof. Goodwin ha avuto tre vite, e almeno tre amori che probabilmente devono essere letti con una chiave sola. Tre vite: ad Harvard, a Cambridge e a Siena; e tre grandi amori: l'Economia, la Pittura e i Vini. Non so in quale ordine d'importanza: spero di non

essere stato irriverente nel rilevare, Prof. Goodwin, che, con le dovute differenze di conoscenza delle fattorie del Chianti, per il momento i vini costituiscono il contatto più sicuro che c'è in questo momento tra il Ministro dell'Industria e lei.

D'altra parte credo che la vita stessa di Goodwin dia contenuto autentico al principio dell'universalità della scienza, di una scienza non disgiunta dall'umanesimo, di una scienza che non può trovare confini geografici e condizionamenti politici. In questa ottica, lasciate che esprima l'apprezzamento per il carattere internazionale di questo incontro, che vede partecipare studiosi di fama indiscussa, di grande autorità e che avviene in una Università che, con il concorso del Consiglio Nazionale delle Ricerche, ha istituito delle "International Schools of Research", di cui una dedicata ai temi di economia.

Ecco, la vicenda di un eminente studioso, straniero come il Prof. Goodwin, si è inserita nella nostra struttura universitaria a pieno titolo, e sottolinea, a mio parere, la necessità di dare spazio ulteriore a forme strutturali di dialogo e di cooperazione tra centri di ricerca e di didattica di paesi diversi. E un tipo di impostazione che sta acquistando peso anche per l'iniziativa della Comunità e qui a Siena credo che il progetto Erasmus ne sia una testimonianza.

Come Ministro dell'Industria, naturalmente attento ai fenomeni che appartengono al mondo delle imprese piuttosto che all'economia della crisi o alla crisi dell'economia, vorrei allora sottolineare qui due punti che mostrano lo stretto collegamento tra il mondo della cultura e il mondo della produzione. Il primo punto si riferisce al processo di internazionalizzazione, che è del tutto analogo al processo che domina la vita culturale: un processo di internazionalizzazione delle imprese che ha anche il fine di acquisire nuove conoscenze e nuove occasioni di cooperazione per la ricerca. Il secondo punto tende a riconoscere quanto il successo nella competizione internazionale dipenda, oltre che dalla prontezza nel percepire le nuove tecnologie, dagli investimenti di capitale umano. Ora va detto con chiarezza, credo, che l'attività di ricerca è uno dei nodi fondamentali per stimolare l'innovazione tecnologica e dunque per favorire lo sviluppo di un paese; e qualunque indicatore s'impieghi, appare in tutta evidenza che l'Italia ha una posizione debole sul piano della ricerca, dello sviluppo e della produzione d'innovazione. Rilevare che c'è un nesso solo indiretto tra

la spesa per la ricerca e lo sviluppo, da una parte, e la spesa per l'innovazione dall'altra, non serve ad eliminare il dubbio che la spesa italiana in materia sia insufficiente. Inoltre, la nostra situazione assume una connotazione ancora più preoccupante quando si osserva che l'efficacia e l'efficienza delle spese della ricerca e sviluppo sono relativamente modeste, che l'impegno è ancora troppo concentrato nella ricerca di base, e poco su quella applicata, e che la ricerca pubblica è sfortunatamente polverizzata su troppi fronti e scarsamente collegata a chiare direttive industriali.

Negli ultimi anni si è realizzato in Italia un sostanziale riavvicinamento tra la politica per la ricerca scientifica e quella per l'innovazione tecnologica, e abbiamo seguito in questo senso -ad una certa distanza- una tendenza che si è più sviluppata in altre parti del mondo. Quindi abbiamo bisogno oggi di qualche cosa di nuovo. Un tratto comune ai vari interventi che oggi si vengono proponendo a favore di una nuova politica per l'innovazione, riguarda proprio i rapporti tra università e ricerca, tra università e industria, perché i rapporti tra università e industria sono straordinariamente importanti ai fini della ricerca, come moltissime esperienze internazionali dimostrano. Sono sempre più numerose le università nel mondo che orientano la ricerca e i ricercatori ad una maggiore collaborazione con l'industria. Sono sempre in numero maggiore i governi che svolgono la funzione di catalizzatori di questi processi. Quindi, anche questo è un aspetto sul quale è meglio osservare l'esperienza altrui con molta attenzione, considerando naturalmente la peculiarità della situazione italiana. Ed è chiaro che occorre lavorare, in un modo più incisivo di quanto non si sia fatto fino a questo momento, per abbattere le difficoltà che l'industria incontra nel collegarsi in modo costruttivo con l'università, per svolgere ricerca con finalità d'innovazione, e quindi di sfruttamento sul mercato.

Questo collegamento tra università e industria è in fondo poi una parte del più ampio problema del collegamento tra università e città di cui si parlava con alcuni amici e colleghi pochi minuti fa. Quindi, a monte di tutto ciò, per concludere, c'è la necessità di una politica dell'istruzione universitaria, e dunque dell'istruzione più in generale, che assegni il giusto peso all'educazione tecnica, scientifica e a quella economica. Paradossalmente, abbiamo un notevole numero di economisti a

cui manca una cultura tecnologica, e un grande numero di tecnologi sostanzialmente privi di cultura economica. Questa è una situazione che va mutata, senza mettere in discussione le radici umanistiche della cultura nazionale, ma anzi traendo da queste radici la varietà di stimoli che permettono alla cultura tecnica di svolgere il suo ruolo fondamentale, che prima di tutto è un ruolo sociale. E per tutto ciò, appunto, ci vorrebbe una politica dell'istruzione o dell'università che non fosse un insieme frammentato di provvedimenti tampone ma una politica con la "P" maiuscola, collegata e resa forte da una visione di fondo.

Avrei altre considerazioni da fare sul rapporto fra teoria economica e scelte politiche, ma non le svolgerò per brevità, per lasciare la parola agli eminenti professori che sono qui a svolgere il loro intervento. Ma c'è anche un' altra ragione che mi obbliga alla brevità e di cui mi scuso profondamente: io vengo direttamente da una riunione del Consiglio dei Ministri, che è terminata alle due e che ha esaminato problemi delicatissimi, come quelli della centrale nucleare di Montalto di Castro. La riunione si è svolta in Piazza Colonna, a Roma, affollata di mille manifestanti che sono tuttora là. La decisione del Consiglio dei Ministri è quella di promuovere una trattativa tra sindacati ed ENEL per vedere di eliminare la tensione sociale che si è sviluppata in questi giorni e che può dare luogo a manifestazioni molto pericolose. Non vorrei dire, per modestia, che queste mille persone attendono che io rientri, ma certamente, la trattativa non può cominciare senza il Ministro dell'Industria, malgrado siano presenti il Ministro del Lavoro e il rappresentante della Presidenza del Consiglio. Sono quindi dolentissimo di non potermi trattenere e ascoltare gli interventi dei professori che terranno il loro dibattito qui. Chiedo dunque scusa a tutti, e al Prof. Goodwin in primo luogo, se sono costretto ad assentarmi per fare fronte a questi impegni.

Prof. W. L E O N T I E F
New York University

The title, the subject of this meeting is very general and should I say also very ambitious. In 15 minutes, one must be very economical in what one says. Let me begin with economics and not with economic theory, and not with day to day events. I would say that possibly the effects of technological change, through the course of history are the most lasting and ultimately decisive. In our time when science develops fast, I speak about science, natural science, and consequently technology develops fast humans however remain the same. Human nature does not change. The difficulty of adjusting the economic system, the economy to the technological change and society to the change in economy, is one of the most important problems of our times. What modern technology does, and I know, not everybody agrees with me, is to reduce the role of humans in the production process; this is what essentially automation does. This increases of course the productivity, but the distribution of income factors of production is affected. It might produce, I think, a very serious problem for developed countries and possibly even greater for the less developed countries. In developed countries, the distribution of income between different groups of labour and between wages, profits and rents could tend to shift not necessarily in any desirable direction. The Japanese know what I mean when they observe the increase in the real estate prices in Japan, I do not need to elaborate.

It is a very serious problem and I think we did not yet find a solid answer to it. The role of the trade unions obviously enters into it. I just point it out. In the last hundred years, the less developed countries were able to acquire an important role in a world economy. The wages were low, so the less developed countries were able to enter the world markets and to sell and to buy, because they could produce commodities which required a lot of direct human labour, cheaply. That situation changes now, and now we, I mean the wealthy countries, can compete with the less developed countries on goods like textiles, in which up to now, they have maintained a very strong position, now we can compete with them, and lose those markets. This I think is likely to produce a very serious crisis in the relationship between developed and less developed countries. To deal with this crisis, to understand them, we need of course strong realistic economic science.

My feeling is, there is also a serious problem in economic science. Not everybody agrees with me, but this is my view. There is a separation between economic theorizing and actual observation of economy. There is a tendency in economic theory to go all into the stratosphere. To take off and circle around high up and never land back on the empirical ground, which makes the result of such analysis not very useful to policy makers, because they have to deal on the ground and not in the sky, except in the election period, of course they prefer to fly in the sky too. Economics has a tendency to be in the state in which alchemy was in the 16th and 17th centuries, a kind of metaphysical theorizing. I think that empirical analysis, which is extremely expensive, which requires large resources and is not very glorious, is quite indispensable in connection with economic theory; it should not be considered separately; theory and empirical analysis must work hand in hand, closely. This situation will ultimately lead, possibly has already led to a crisis in economics. I, observing my students, the economic students in our university, see that the young people feel extremely disappointed, about what they are being taught in macroeconomics and microeconomics and they do not dare to say so, because academic establishment is terribly strong and if they complain loudly they will not get their degrees so that had better to do what they are told to do. I think that it is particularly important when we are facing very serious practical problems in many countries and I

think economics as a science will not be able to contribute very much unless it mends its way and realizes that it is an empirical science which needs a close cooperation between theory and empirical analysis, which means a very large increase in the amount of resources devoted to empirical analysis. There are countries which do it better than others, for example, I ascribe some of the Japanese success to the fact that they take economics much more seriously. I worked with the Japanese and I always observed when confronted with a problem they not only theorize but also devote very large resources to do empirical analysis and then choose what course of action to take and in many cases succeed. I was very amused when I recently received from Japan a book in which a Japanese economist explains to us what is wrong with American economy. It was a very good, a very detailed study, among other things, in which, by the way, he did not collect new data, but he used input-output analysis to interpret the data which were already available and get very interesting results.

During the last three years, I had the opportunity to work for your government in developing what they called "Il piano generale del trasporto". There what we tried (naturally the working group was Italian) was really to get good information, good theoretical structure including dynamic systems, which essentially are non linear. I do not want to go into technicalities and one really came up with specific results indicating where what should be done. But this is only possible if we strengthen the empirical information. Since I did already say many disagreeable things let me add that I feel that modern econometrics does not contribute much to real understanding the operation of our economic system. Using incredibly complicated methods of indirect statistical inference does not provide an effective link, a working link, between theoretical and empirical analysis. I think the parameters with which we have to work, which enter into a theoretical construction and must be observed, should not be primarily derived by curve fitting. If interested for instance in technology, we better go for factual information to an engineer. There is also a remarkable separation of economic theory from all the neighbouring fields. The economists invented their own description of technology which has no relation to real technology. They developed their own new explanation of consumers behavior which has nothing to do with the work of

people who really analyze households like anthropologists, market analysts and housekeeping specialists. Economic analysis could profit greatly from close cooperation with the disciplines. I better stop now.

Prof. M. MORISHIMA
London School of Economics

I want to reverse the order of the topics. I will first discuss the crisis in economic theory and then briefly discuss the world crisis. I am happy to say that I agree with Prof Leontief in the overall assessment of achievements of the contemporary economic theories, though I may disagree with him in some minor detail points. I too must, unfortunately, recognize present economics, especially mathematical one, as a quasi-science which cannot be claimed as full science at all.

If you look at the Spinoza Ethica, which is obviously not a science, but a book on ethics, you find it takes the axiomatic approach; first a system of axioms is established on the basis of which we prove a number of lemmas and then, finally get theorems, from which we derive a number of corollaries. This is Ethica's structure. Then what is Spinoza's purpose in Ethica? It is to prove the existence of God rigorously.

Take the recent example of mathematical economics; it is structured as the axiomatic analysis. It assumes a number of axioms and establishes a number of lemmas, theorems, etc., with the purpose of establishing the existence of equilibrium. Apparently, equilibrium is the God for economists. Arrow and Debreu claim that our God has scientifically been shown to exist.

Science must have two main stays. One is the rational reasoning, the other is empiricism. Without empirical verification, mere rational

reasoning is just half way as a science. In fact, as Prof. Leontief emphasized, we, contemporary economists, are very bad on the side of empirical verification and our limited intellectual resources are badly misallocated to the direction of pure reasoning. Economists must of course correct this misallocation if they really want to have an economic science. In our time economics is almost standardized, in all places in the Western world, America, Britain, West Germany, etc. and Japan. We apply the same type of theory to analyse these economies. In the case of the soviet regions, however, if you insisted that the theory of socialism be applied to Soviet, Poland, Bulgaria, etc. evenly and uniformly, then we would say that it is absurd, there is no such general theory. Bulgaria is different from Hungary, Hungary is different from Poland, and Poland is not Soviet Union. Similarly in our Western world, America is different from Britain, Britain from Japan, and so forth.

If this is the case, then why can we accept a single theoretical model for all countries in the Western world? Japan is twice as large as Italy in population, while Italy is almost the same as Britain and West-Germany. Not only in terms of population, but also in terms of territory, these countries are comparable with each other but they are quite different from America. America is much bigger, and she has big area for agriculture. Japan is insufficient in the agricultural area; similarly in Britain, agriculture has shrunked, so that only 2% of the work force is working in the fields. Therefore, in such countries as Japan and Britain and even in the case of Italy whose agriculture is not reduced to the level at which Japan's and Britain's are now operated, we should neglect agriculture, for the purpose of abstraction.

Abstraction means that in drawing a picture of a concrete national economy, we, like Picasso, neglect some aspects for the purpose of emphasizing some other aspects. Our pictures inevitably distort from the concrete figures. Since in the case of Britain and Japan, agriculture is not important, we may decide to neglect it. Then how are materials for the manufacturing industry obtained, in those countries? Obviously we must import them, so that material always comes from abroad. Then this has an important consequence to the theory of the firm which has so far been separated from the theory of foreign exchanges. Where imports are made, a part of the total cost

(that corresponding to the imported materials) is given in terms of dollar, some other part in terms of yen and the rest in the pound sterling. Then to calculate the total cost, the problem of exchange rates has inevitably to be taken into consideration, so that the theory of the firm in those countries should be quite different from the standard theory of the firm which fits only to those countries within which the firm's products are sold and its materials are procured. We thus have different models, from among which the most appropriate one has to be chosen, in order to examine the actual economy of the respective Western countries.

Now I want to propose some ideas to improve our economic discipline. As economics is quite different from mathematics, and physics, but may be very similar to medicine, economists should be produced in the same way as medical doctors are educated. In the case of medical students some basic knowledge of medicine is first given, then students must work as interns in the hospital and then come back to their academic work again. Internship is a necessary part of the educational process of medical doctors. Similarly economics must have an intern system. Without any knowledge about the actual world, how can we produce appropriate theoretical models? Are we just making theoretical models for the purpose of theoretical elegance? If so, it is a terribly bad habit of economists.

Then how do we make intern system? This is not so difficult, because we have a number of institutes, empirically oriented research institutes. If universities could make contracts with such institutes, to the effect that they would accept new graduates for two years in their institutes, then the graduates start to learn about share prices, share markets, foreign-exchange markets and so on. Moreover they will become familiar with decision-making process of the government, etc. This is a very important education process to make economic students more useful and more practical.

Now, let us turn to the topic of the world crisis. I consider economics should be a multidisciplinary subject as Keynes once wrote to the effect of something like this: "To be an economist, a person must know some amount of philosophy, some amount of mathematics, some amount of politics; to some degree he must be a politician, a historian, etc." If so, it is impossible for a single man to be a

historian, a politician, a mathematician, and a philosopher at the same time, it is indeed impossible for a man to be an economist. I believe impossible even for Keynes.

If we believe Keynes did not say any nonsense, we have to interpret the above passage from his work differently. A first-rate economist need not be a first-rate philosopher; he need not be a first-rate politician, a first-rate mathematician, and even a first-rate economist. He should be satisfied with a second-rate politician, a second-rate mathematician, a second-rate economist and so on. These collection of second-rate abilities produces a first-rate economist. This multidisciplinary character conceived by Keynes is very different from what we understand when we talk about economics as a multidisciplinary subject. In the present-day so-called multidisciplinary conferences economists do not know about politics, politicians do not know about economics, and they talk to each other and accept what the other side says. They collect papers and produce proceedings. This can, however, be multidisciplinary in a very bad sense. However, in the way conceived by Keynes, each participant should be multidisciplinary. Then he can collaborate with other members of the conference in the true sense.

This multidisciplinary character is especially important for understanding the Third World. Third World reasoning or logic and Third World philosophy are quite different from the philosophy in America or in Europe. Their rationality is quite different from European rationality. Once Max Weber said that : "In the Occidental world it is meant by rationality that the nature is controlled rationally, while the Oriental people adapt themselves to the nature or environment rationally." This is the biggest difference in behaviour, noticed by Weber between Occidental and Oriental people. And because of this, he concludes that capitalism is impossible in the East. The rational adaptation as observed in the East is foreign to the Western spirit, and such attitude towards the nature does not produce science. However, Japan succeeded in establishing a capitalist economy with the Oriental type of rational spirit. In view of this and the events which are happening in other parts of the world, we find that our world is no longer homogenous. It accommodates several heterogeneous types of production systems. To study the Third World we must first learn the

spirit of Hindu, the spirit of Islam, etc. and then examine what their rationality is. In this way economists, especially development economists, must be multidisciplinary scholars.

Thank you very much.

Prof. P.A. SAMUELSON
M.I.T.

I have to react to the two topics, the present crisis in world economics and the crisis in modern economic science, by referring seriously to Sherlock Holmes' case: The Hound of the Baskervilles. The remarkable thing about the dog who howled in the night, you remember from the Conan Doyle story, is the fact that the dog did not bark at all in the night.

The remarkable thing about the present world crisis, I seriously aver, is that there is no present world economic crisis. Compounding my complacency you may say, but I shall defend myself, I will as solemnly assert there is not really a present day crisis in economic science.

True, unemployment in Europe is high, too high. True every model has but limited success in economic forecasting. If you do not like complicated Keynesian models, trot out a monetarist model; in case you think it can forecast, calculate its least squared minimized error and be disillusioned. I deny none of this, nor am I skeptical that some new insights from Robert Lucas, Tom Sargent and company have been provided in recent time.

Why do then some speak of the death of our science, or at least of its stagnation. I say, you can be disillusioned only if you have previously been illusioned. Long ago, by 1955 at least, I was led both by empirical experience and analytical reasoning to realize what it

means to have a fruitful economic science. For example, I will tell an anecdote, a true one. Tjalling Koopmans received a grant from a foundation to take a year off, and think about the fundamentals of our science. He was a very serious man, and one thing he did, was to come to my MIT office and say: "I want to talk to you about improvements in our ability to forecast, to identify, to develop better econometric methods. Tell me, Prof Samuelson, are we converging towards greater and greater accuracy of forecasting?" And since the man asked me, I told him my view; I said then: "There seems to be something almost like the Heisenberg indeterminacy principal, nothing so high-faulting as von Neumann's axiomatic system in Hilbert space was I referring to, which suggests that we will come to do only a little bit better in forecasting. For if, so to speak, God Almighty has not made up her mind what investment animal spirits will be in the American Fortune 500 companies, how can it be that we can move towards ever greater precision?" I said then: "I hope I am wrong." I say now, "I wish I had been wrong". Still we are right on the time table of progress that an understanding mind should have inferred from the state of economic science as it was at that time.

I do maintain that in Dick Goodwin's life time, which is my time too, economics has made remarkable progress. It continues today to grow, in its integral of accomplishment. Lying before it, economics has a great future as a science. Not least, we bequeath to our students plenty of exciting problems still to work upon. That was the trouble with old Alfred Marshall. He imbued his students with the feeling that everything that can be done had been done by the great old man Marshall. There is nothing left for Pigou to do but to cross the t's and dot the i's and fill in a few fourth place decimals. How refreshing it was to turn to Knut Wicksell in translation and to get an entirely different point of view.

Now I want to leave the arid area of scientific methodology. I want to move to the real world and to take the biggest problem that is on our platter today in the world economy. On October 19th 1987, there was a stock market melt down in Wall Street, "black Monday". You and I have been living in history, this was every bit as big, measured in the proper metric as anything that happened in 1929. A bubble or mania is not a new thing under the sun; it is the oldest cyclical

process that economists have ever studied and the one about which we have a good deal of knowledge but also a good deal of knowledge of our limitation of knowledge. The tulip bulb mania of Holland of the seventeenth century is a perfect Harvard Business School case study. Sir Isaac Newton, the master and warden of the Mint, got caught in the South Sea Bubble of 1720. I, a veteran, come before you as one who has been through three of these events. As a boy I saw in the streets of Miami, Florida, grown men in plusfours buying and selling land; until the hurricane of 1926 brought that bubble to an end. My high school algebra teacher and I worked through and lived through the Stock Market crash of 1929. Her income effects were more substantial than mine. Now let's take this particular 1987 event. This is a big event; it is like a cosmic shower that the physicists recently have been getting, and it has caused a great deal of interest. The short run causation of the crash is not interesting and not important. Causation moved outward from New York, but that is very misleading. Essentially this was a simultaneous worldwide melt-down. The really smart people who are fundamentalist analysts with a marvellous track record, I am referring to people like John Templeton, and so forth, what they knew was that the most inflated bubble of all by fundamentals was the Japanese stock market; and they sold short and were out of the Japanese market and they were the ones who lost the most money in so doing. I had the unenviable task of giving a public lecture on Tuesday after black Monday with an announced title "The Genesis of the Modern Theory of Finance" and my first sentence had to be that "I am changing the title". The sermon of my discourse was, "Never underestimate the power of chewing-gum. I mean Japanese chewing-gum".

Now, the question that every person in my audience was thinking about, and it needed to be asked, was "Will the stock market crash produce a great depression as the history books tell us, happened in 1929 to 1933 following 1929?". History books do not tell us the causation, they only give us the reduced form, the bare facts. That is a problem to challenge the mettle of any informed economic scientist, and I submit to you it is not a problem that can be well-addressed, God forbid, by brokers, or investment bankers or by the executives of the Fortune 500 companies, or their Japanese equivalent. And with

respect, I say a Cabinet Minister is not the person who can speak cogently on this subject.

Now the correct answer to this is not to tell us whether it will cause a great depression, but to marshal for us the evidence and the best way of analyzing that particular evidence. And I think for that purpose you could not do this in 1929, with the tools which were available to the best economic scientists. I have to tell you theirs was a shameful performance, of Schumpeter and company in the collected volume edited by the Harvard Department in the early 1930s. Thus Schumpeter really fell for that garbage of three cycles, a Kondratieff long wave interfering with a Juglar intermediate cycle, and both interfering with the Kitchin short cycle. And if Schumpeter had known about the Kuznets 18-year cycle, he would have thrown that in along with the kitchen sink.

It was the juxtaposition of all three at once which allegedly caused the depression! The ham-bone is connected with the shin-bone which is connected to the thigh-bone... That was surely not the best that economic science could do at that time.

Let me tell you, just so you do not go away empty-handed, now is that you should bet 10 to 1 or 20 to 1 against a depression at all like that of 1929. In order to arrive at that conclusion, I don't have to look at Trotsky's writings in the 1930's, as there is nothing in them that could be helpful. Nor did I read Dick Goodwin's Harvard thesis on Marxism. I do not want you to think that I am proud. If, in order to understand the real world phenomena, I had to hang by my heels from that tallest wall over there, I would not like to do it but I would do it. And if I could get from one theory of marxism or another insights that would enable me to narrow the odds on a crucial question like this, then like Dick, I would earn a second reputation: having worked the mainstream economics side of the street, I would work the other side of the street, the neomarxists' side. But alas there really aren't bits of information in that literature. Yet it is fundamentals that most condition my view. The 1929 crash that I lived through was part of the capitalism that I lived through in the formative years of my life. If Freud is correct that the age of 3 to 5 is all important, then I am the one who lived in the world of no government interventions at all. And in my view, the fact that we have better information in computers

would not make me complacent in the face of the recent melt-down, if we still lived anywhere in the world under pure capitalism, under a purely capitalistic market system where one domino can in a self-fulfilling way knock down two dominos. In the region that I grew up in, all the banks failed; you had to buy change, it was the failure of 8 or 10 thousand banks much better run, I may tell you, than recent Texas or Oklahoma banks have been run. An event of this magnitude, impinging upon pure capitalism would be an entirely different ball game. But it is a mixed-capitalistic system, with all of its defects, that changes completely the odds.

Now you may say, "Why do we need to have non-linear business cycles to understand these oddities?"

I must end with a puzzle, I do not know what it is that a thorough grounding in the best of economic principles does to a person, but it has been my observation that if you want to see who are the best members of a council of economic advisers in a Washington backyard, it is the Tobins, the Solows, the Modiglianis, all highly trained technical economists. They are the best at ending in reduced-form shirt-sleeves economics of the greatest relevance. So when any of my graduate students become a little skeptical- are they mispending their youth?- then I tell them: Our Lord's House of science has many mansions. Going through the rigmarole of advanced economics is a discipline which you will finally, years later, learn to unlearn. The residue left will, this is my faith, interact fruitfully with the real problems of the world. So, sing no funeral dirges for economics as a science. It is doing the best it can.

Prof. W. L E O N T I E F
New York University

I will try to be brief and concrete. Theory in economics is essentially a tool to deal with reality. It tells us how to employ factual information in order to get significant realistic results. One of the weaknesses of contemporary economic theory is that it uses a language different from that which people who really deal with that reality use. So it would be better for us not to invent our own terminology, but to employ the terminology employed by those actually engaged in economic transactions. It makes no sense to separate theories for each situation. The systems approach aims at constructing a model which can be applied to a very great variety of empirical situations. The aim of economic theory should not be deriving general theorems but simply providing a tool to observe, describe and analyse in a systematic way reality.

Prof. M. MORISHIMA
London School of Economics

I agree with Prof Samuelson, in saying that the best advisers of the economic council are Solow, Modigliani, Tobin, etc. If I were the president of the United States, I would certainly get advice from these persons. However, that does not mean the surrender of my proposition, rather it strengthens my position. It is true that intellectual abilities are usually allocated more to the traditional, orthodox camp than to the heretical schools. However, provided with more empirical knowledge, Solow would become better Solow; Modigliani would become better Modigliani. This is my point.

In usual views crisis in economic theory is connected with the emergence of neo-Ricardians, neo-Marxists, neo-classical economists, etc. From what I spoke, however, you would have realized that my view of crisis in economic theory is entirely unrelated to the problem of new theoretical schools, neo-Ricardian, neo-marxist, etc. It is related to the emergence of economics in a country with different structures. For example, in a country with a very small agriculture sector, the market mechanism is quite different from that of a country where agriculture is of a considerable size. In the latter the cobweb theorem would play a substantial role in the formation of prices while in the manufacturing activity it has only a negligible role. These two economies have different market mechanisms, though we are still talking about the market mechanism. That is my point.

QUESTIONS

Prof. S. L O M B A R D I N I
University of Turin

May I ask, especially to Profs. Leontief and Morishima, whether you think that there is a link between the evolution of the economic system and the crisis of economic theory. That is so since economic theory has given some explanations of the work of the system when technical progress is a continuous smooth process. But if you have a revolution, like the Industrial revolution, then the evolution of the system cannot be forecast by any economic model. If we consider what happened in the United States during the twenties, there has been an adaptation of employment only because Ford was able to convert a commodity which was invented as a toy for rich people -the automobile- into a mass-consumption commodity. Because of the evolution of electricity, a certain number of durable commodities have been brought to light so that these were structural changes that could not been foreseen by any economic model and without these structural changes, I wonder whether the mechanisms which we, economists, consider would have been able to create employment to compensate for the reduction of employment caused by the second industrial revolution. So, if this is so, and I believe in the usefulness of the economic theory, and from this point of view I agree with Prof. Samuelson, is it not better to conceive of, to try a new approach, which I would call computational economics. I discover that this is

carried on also in the Soviet Union by the new mathematical economists -they call it synthetic economics, because they stress the link between microeconomy and macroeconomy-. In computational economics we need a model, but then we need to do what Morishima has invited us to do, to devise future scenarios and we have to be a bit of an historian, a bit of a sociologist, a bit of a politician to see these scenarios. Then we need to work out, to see what these possible scenarios entail, by using our economic model. That brings me to emphasize what professor Leontief said, I completely agree with him, that econometric methods, which have recently been developed, are not very helpful to provide the link between the theory and the real world, as possibly this new approach, computational economics can do. Thank you.

Prof. P.A. S A M U E L S O N
M.I.T.

I am not sure I understand what the proposed computational economics is, but I am very happy to look at it and examine it and give my impression. But I do not want either to sell a pig in a poke or buy a pig in a poke. There are many things in economic life that are not predictable, and you should plunk a student who does time series analysis and predicts the unpredictable. I will give you two examples; from 1900 to 1939, there was a very clear pattern in demography all over the Western Europe and I will address myself to the United States. Anyone who went to High school, gymnasium, moved to the small family size, and the demographers finally caught on to this, and they made their projections rather boldly, and then along came the end of the Great Depression, World War II, and suddenly the middle classes, people like me, went crazy, and people like me had six children. When I told Mrs. Richard Stone that my wife was expecting triplets, she said "that is impossible, people like us do not have triplets". But we did. Most of the American people did not have triplets, that was not predictable. The only fault that I make of the demographers was that unlike the canny economists, who are a shifty lot, and cut their losses very soon, -if we predict Keynesian unemployment after World War II and within six months and we see that it is not happening, we immediately shift over to the other side of the street- the

demographers did not do that. I could give you many other examples. There should be at least the sign of a movement as big as a man's hand before you start using that as a causal explanation, but you do not want to be the last fellow on the block to recognize what is happening around you.

Prof. K. VELUPILLAI
University of Aalborg

There are two views. On my left, in the pink corner, or light red corner, Professors Leontief and Morishima with skepticism about economic theory, and on my right, Prof. Samuelson in the light blue corner. Now I, like many here, came out of '68 as a second or third or fourth year university student, and I remember being deeply impressed by Professor Leontief's Presidential Address to the Econometrics Society, about theoretical assumptions and non observed facts, and began life as an economist on the other side of the street from Prof. Samuelson, but now I have crossed over, I think, and I think I am on his side especially in everything he said today; I think I find myself on his side. Now with that background, I want to take up two points. Paul Samuelson said that the disillusion is because there was illusion, and then he said, and I agree, that this training gives a discipline that later with maturity we may unlearn, and keep a residue which can be useful. But Paul Samuelson I think often forgets that not all have the ability to unlearn or to distinguish what to unlearn. I carry in my pocket always a poem by Auden and few other poems and this one (I actually got it from a book once Axel lent to me "The Red and the Blue" by Andrew Sinclair) says as follows "So I wish you first a sense of theater; only those who love illusion, and know it, will go far, otherwise, we spend our lives in a confusion, of what we say and do

with who we really are". So, I am definitely on your side, but I want to know how to unlearn, and how to use what I should learn and keep, and that I believe is the task; we should also train students, to unlearn- that is the tough one.

Prof P.A. S A M U E L S O N
M.I.T.

The question reminds me of an occasion when I became officially "Emeritus" and you are expected to say a few words, we have been deprived as yet of those few words, but I still have hopes. One of the things I said (It was not original with me) was that during all my life only half of what I taught was true, but unfortunately I was never sure which half it was. In economics, as in "Through the Looking Glass", you have to run in order to stay stand still, you have to unlearn. Model T Keynesianism was a very good theory for the 1938-1939 that I knew. When I went to Harvard no bank would take my deposit; the rate of interest was $\frac{3}{8}$ of $\frac{3}{8}$ of $\frac{3}{8}$ of one per cent, so pushing on a string by the central bank had a meaning, and no book, however thick, by Mrs Anna Schwartz or Dr. Milton Friedman, will permit me to forget that. You know, most people have a mind that freezes at the age of 29. God pray that the professors of the University of Chicago, in any vintage year, teach something useful because most of those students will go out, frozen in their mind with those ideas. I thought the Radcliffe Committee report was a travesty, here were the greatest minds of English macro-economics frozen in Model T Keynesianism. Keynesianism is not a religion, it is a technique, although I heard Axel in 1983 declaring on the death of Keynesianism, and that all the new and able people were "whoring" in other directions. Well, I went out and I studied what does a non-Keynesian,

like Larry Summers, one of the most prolific actually, say and do. What about Martin Feldstein, who is a critic of Keynes and I find that I quack like a duck, and he quacks like a duck, and I fly like a duck and he flies like a duck. When we are discussing the problem of the balance of payments with respect to Japanese and American societies, the effect of the fiscal deficit, I discovered the eclectic version, the eclecticism that I learnt at the knee of Gottfried Harberler - he floored me once and said "Paul, how do you know that the nature is not eclectic?". So from that moment on, I eschewed simple theories and I made it my code of honour on my shield "I will be as eclectic as the facts required". Not needlessly so; an empty-minded acceptance of everything is an affirmation of nothing; so, of course, the art of our science, what Keynes was talking about, what makes it endlessly fascinating, is that it is not cut and dried, and you cannot, by learning the tensor calculus and a new method, learn it. If you think that I am preaching in favour of gullibility here, that the fanciest recent theory is the best one, let me say some words about deterministic chaos which I have written about, in a volume for a scholar whose name I will not mention. Deterministic chaos is extremely important, and when my colleague, Edward Lorenz, discovered it as the strange attractors although it had been buried in by Poincare earlier, he discovered that if we got better computers and predicted the weather seven days in advance, the very nature of that deterministic system with strange attractors was that the inevitable imprecision in the initial conditions would make it derogatory, would not help to predict by that method. In a MIT open seminar after the melt-down, a meteorologist there said, "maybe the stock market is like deterministic chaos." And the New York Times Sunday financial page had a section on that; and I have to tell you that is what it is not like. The short run behaviour of the stock market is a kind of white noise, that is the old-fashioned kind of chaos. That is because of discounting by speculators, who, if there was a clear cut trend, would already be waiting there for you and would already have acted upon it. So we have something much worse in economics of speculative markets than the deterministic chaos, we have the chaos without the early time determinism, and woe of two new graduate students not yet dry behind

the ears who come to the real world without a proper recognition of that complexity.

F. G A L I M B E R T I

Il Sole- 24 ore

I am an economist at the Italian Financial daily, Il Sole 24 Ore. My question is about what has proved to be in the past a point of friction between the real world and economic theory, namely the exchange rates and in particular the problem of the dollar. Two, three, four years ago, the dollar was flying very high, and many economists said that it should not be where it was, and they were treated by non-economists like those specialists in aerodynamics who used to maintain that the bumble-bee could not fly.

Now, the dollar has gone down, and many now explain its past trends in terms of what Professor Samuelson just called the case of the self-fulfilling bubbles. Even now, there are many divergent views, this time among economists themselves, about the future course of the dollar. Some, like Feldstein or Dornbush maintain it should go down another 20-30%, to force the adjustment among the main world economies, whereas others think that from a competitive point of view, the dollar is more or less where it should be, and you should give some time to the magic of devaluation to work its way through the economies and that central banks should resort to all the tricks in their trade to keep exchange rates where they are while the adjustment happens. I would like to hear the view of the panel about

this, both about the past of the dollar and some crystal ball gazing for the future. Thank you.

Prof. P.A. S A M U E L S O N
M.I.T.

The Nobel Prize in economics is not a proper prize. You are reminded of that when you go to Stockholm. They have to forge new traditions, and one of the traditions which they, the Swedish Chamber of Commerce, try to forge, was to have a little Nobel lecture. The laureate gives a scientific lecture as he must, because he cannot collect his money otherwise, but at the same time he gives to a popular audience a little Nobel lecture. When I was asked, it was a very busy time, so I simply refused. If Mohammed would not go to the mountain, it turned out that the mountain would go to Mohammed. So the Swedish-American Chamber of Commerce arranged for me to give my little Nobel lecture in New York City, which I could not decently refuse to do. In that, I predicted pretty much what it has happened with respect to the dollar exchange rate and the Japanese exchange rate and the balance of payments. Now, think of the power of my method. I did this without the failure of the American propensity to save, which we today recognize as one of the important part of the story. I did this long before crazy reaganomics and snake oil supply side economics, with a basic structural deficit. I am unrepentant because, you read in the newspaper -now I am being responsive to the press- this is treated as if it is simply the result of a macroeconomic tautology that the difference between what a nation invests and what

it provides out of its own savings, must be made up by the balance of payments deficit of that nation.

There is more than one cause - Professor Haberler and I will maintain - of the chronic dollar balance of payments problem.

The other part, which you must only get from studying microeconomics, is the theory of comparative advantage that I learnt from W. Leontief and G. Haberler. If you believe that Schumpeterian reductions in real cost were taking place at a much greater rate in the Pacific basin, in Japan first and then later in the four dragons, then, although it is not inevitable, it is extremely likely that that would create balance payments deficit, even if America had a balanced budget, even if America maintained a high propensity to save. So, you have to bring in both the tools of macroeconomics and microeconomics. I say all that to bring an answer to a brief terminus. If I had to go away and leave instructions in an envelope for my innumerable progeny, namely have a long position on the yen and a short position on the dollar, or the reverse, and I was forced to do one or the other, then I would have to say the dollar still has some distance to go. Now I know there is a Stanford professor who has calculated purchasing power parity and believes the dollar should be half way as high now as it was at its peak, but he never looks at the balance of payments discrepancy that needs to be, not wiped out because the goal is not zero equilibrium, but that needs to be reduced for political or other reasons. That would be my answer which I would be perfectly prepared to change when the evidence suggests otherwise.

A P P E N D I X

100

A P P E N D I X

M. D I M A T T E O
University of Siena

R.M. Goodwin: a Biographical Sketch

1. Richard (Dick for friends) Goodwin's life may be viewed from three different angles reflecting his multiform personality. Throughout his life he has been contemporaneously an artist, a political spirit, and an economist.

Born in Newcastle, Indiana, in 1913, the son of a lawyer turned banker, his love of the arts was fostered by his aunts, one of whom had lived in Paris and breathed the exciting atmosphere of the artistic vanguard. He decided not to become a professional artist because of what he regarded as lack of talent but continued painting all his life. A catalogue of his paintings is now being published.

He won a scholarship to Harvard where he read Political Science and graduated in 1934 with a dissertation entitled "A Critique of Marxism" [I]. While a student he took an active part in University life writing political and cultural pieces [I] for the student magazine, The Harvard Critic, of which he was an editor. His love of politics and curiosity about the big events of the thirties (Fascism and Nazism) have continued up to the present time, developing when he went to Oxford (St. John's) as a Rhodes Scholar in 1934 for three years. He

read Philosophy Politics and Economics but paid frequent visits to Italy and Germany to study nazifascism at first hand. He was a member of the Communist Party until the Molotov-von Ribbentrop pact.

The tragic events of the Great Depression (including his father and his grandfather both going bankrupt) and his experiences in Europe shifted his interests towards political economy and he enrolled as a Ph. D. student at Harvard. His thesis, initiated under H. Phelps Brown's supervision in Oxford, was "Studies in Money. England and Wales, 1919 to 1938" and the main results were soon published [II]. Here we find a first attempt at building a statistics of the money supply and velocity in the UK (needless to say he did it without a computer and this permanently put him off empirical work). He became convinced that trade cycles are not the result of mismanagement by the monetary authorities (a common opinion then as it is again now) simply because money supply responds endogenously to business conditions rather than vice versa.

During the late thirties, after his initial distrust because of the Austrian economist's right wing ideas, he was increasingly influenced by the teaching and personality of Schumpeter. When Goodwin was appointed at Harvard, as an assistant to the unorthodox J.H. Williams, Schumpeter and Gottfried Haberler attended the first course he ever gave on linear cycle theory [I]. His interest in cycle theory was stimulated by attending Jacob Marschak's seminar and Roy Harrod's tutorials in Oxford. There he also became infected by Keynes's ideas. This was always a major point of discussion with Schumpeter who never came to accept the central message of the "General Theory", despite Goodwin's repeated efforts of persuasion.

Already before the war, then, the fundamental elements of Dick Goodwin's vision were already in place, ready to be refined into what was later called the m-k-s (Marx-Keynes-Schumpeter) system by Goodwin himself. One of his first papers, "Innovations and Irregularity in Business Cycles" [V], is full of Schumpeterian and Keynesian inspirations. In its simplest version it is a lagged multiplier model in which investments, instead of being driven by expectations, are modelled as a sinusoidal function of time in accordance with Schumpeter's swarms of innovations, with the intention of bringing in historical events. This leads to cycles in income that reflect the

oscillations contained in the investment waves. So even if the inner mechanism of the economic system is stable, i.e. does not amplify the impulse, nevertheless the latter keeps the oscillation in income alive. This interpretation of the impulse seems to Goodwin more satisfactory than that of Frisch which resorts to random shocks. The model can be complicated in various ways. One of the most interesting is to assume that the waves in innovative investment are irregular oscillations so that they can be approximated by sums of sinusoidal functions: in this way the resulting fluctuations in income will be irregular as well, resembling of actual statistics [V: 183-4]:

An explanation of the reality of a tendency to periodicity is obtained without any strict periodicity ever existing. The historical individuality of each cycle is introduced into the rigidity of mathematical business cycle theory. The complex wave form follows. The ever changing severity of fluctuation can be explained. So long as capitalism is not stationary so long as there is any exciting expenditure the oscillations may temporarily become less severe or more severe, but they will not tend to die out. This results in spite of the fact that the mechanism itself has damping.

A few points can be made. First the subject of Goodwin's theorizing is the evolution in time of the capitalistic system. As far as methodology is concerned, no attention is paid to individuals but the idea is to build aggregative models consistent with what is shown in time series. Lastly Frisch's conceptual framework of impulse and response for analyzing cycles is accepted.

The final ingredient needed to produce the most famous paper of the Harvard period on cycle theory was provided by chance. During the war Goodwin taught physics to the army and became acquainted with the applied maths used by physicists. When he went back to economics he knew the limitations of linear analysis for explaining persistent oscillations. He had fruitful discussions with Le Corbeiller (a physicist in Harvard) and subsequently produced "The Nonlinear Accelerator and the Business Cycle" [II], read in December 1948 at a meeting of the Econometric Society in Cleveland. The central hypothesis concerns the behaviour of investments when there is a difference between actual and desired capital. In this case firms try to fill the gap as quickly as possible. When actual stock is lower than desired, investment is kept at the maximum rate allowed by productive capacity; when the opposite occurs disinvestment is at the maximum rate allowed

by not replacing capital goods that are scrapped (In other words gross investment is zero). Finally if desired capital equals actual capital, net investment is zero (only replacement takes place) and capital remains unchanged. The peculiarity of the model is that whenever the system is not in equilibrium, namely desired stock is different from actual stock, the system undergoes a cyclical pattern that is neither damped nor explosive. Suppose that actual stock is less than desired stock: net investment is determined according to the behavioural hypothesis described above. Therefore income is determined via the multiplier mechanism and consequently desired capital is determined as well, being closely related to the value of output. Capital will go on increasing until it reaches the desired level; at this point investment falls to zero. Income falls as well, via the multiplier; a new level of desired capital is determined. The stock of capital goes on decreasing until it reaches the desired value. Here net investment (which was negative) rises to zero: income rises too and desired capital with it, so that net investment again becomes positive. Therefore the cyclical movement repeats itself again and again. This case is later dealt with by catastrophe theory but was anticipated by Goodwin.

A few of the noteworthy features of this paper are as follows. First Goodwin rejects Frisch's approach by assuming that the cycle can be wholly endogenous and that no external forces are necessary to keep it alive. Secondly the values of the parameters are no longer crucial for an oscillatory solution as for example in the Samuelson paper. Thirdly as long as the (absolute value of) depreciation (assumed constant) and the maximum rate of production of capital goods are different, the upswing and the downswing will be of different duration: presumably the latter will be longer. Important generalizations can be made by including technical progress, lags in the multiplier process, and finally lags between investment decisions and actual expenditures in capital goods. The final model gives rise to a nonlinear model that admits a (unique) limit cycle. The latter designates a stable movement (all trajectories tend to it from wherever they start) and can be seen as a generalization of the concept of stable equilibrium since the latter can be regarded as a stable movement so small as to degenerate to a point. Of particular interest is the introduction of technical progress in the form of a constant rate of growth of capital. In this way the

desired capital is a function of both income and time. In later papers [II] to explain growth much emphasis is given to the Duesenberry (ratchet) effect that induces an asymmetry in the consumption, and therefore income, pattern.

Before leaving the Harvard period mention should be made of Goodwin's work on multi-sectoral theory. "The Multiplier as Matrix" and "Does the Matrix Multiplier Oscillate?" [III] are inspired by Leontief's input-output analysis. It is worth noting that for the first time in economics, the Frobenius theorems on positive matrices are used. (I have recently been informed by Prof. Morishima that in his "Dynamic Economic Theory" published in 1950 in Japanese he also applied Perron-Frobenius theorems). The main result of the papers is to show that, barring exceptional cases, in a suitably modified Leontief open model with a simple lag, national income will oscillate in response to changes in injections before reaching a new equilibrium level. The way in which this interesting result is shown relies on a device that is to be used by Goodwin time and again. The device consists in transforming the coefficient matrix so that we can simplify the solution of the problem at hand. Goodwin has always been of the opinion that economics is a hopelessly difficult subject due to the complexity of the relationships between its parts: "There is just one problem": he is always saying. Therefore ingenious simplifications are welcome. The starting point of the transformation is that generically any empirical matrix has n distinct latent roots (e.g. see sect.5 of A4 in the Mathematical Appendix to [VI]). There exists a modal matrix that will transform the system matrix into a diagonal matrix with its n distinct roots along the main diagonal. Putting the matrix in canonical form and redefining variables accordingly we actually reduce the system of n interconnected equations to n decoupled equations that are easily solvable. In this way the system can be seen as the sum of its parts.

From this superb work two directions can be taken, both of which were subsequently followed up by Goodwin. One is to apply the multi-sectoral scheme and the impulse-response framework to problems of demand and cost inflation and international trade and policy (see "A Note on the Theory of Inflationary Process" and "The World Matrix Multiplier" [III]). The other is the extensive use of the diagonalization procedure to gain insights into the dynamics of general linear

equilibrium (see his "Static and Dynamic Linear General Equilibrium Models" published in 1953 [III] but conceptually derived from the "Multiplier as Matrix" paper).

The other innovative paper that cannot escape our attention is "Iteration, Automatic Computers, and Economic Dynamics" [II] read in Varese in 1950 at a meeting of the Econometric Society. In it the old problem of the solution of the Walrasian equations of general equilibrium is given a novel treatment. The device of *tatonnement* is interpreted as a practical method, namely an algorithm for solving the system: a constructive proof of equilibrium is outlined. This interpretation was ignored until the sixties and seventies when Uzawa and Morishima took it up again. The iteration procedure that Goodwin envisages includes price, quantity, and mixed (price-quantity) adjustments.

2. This concludes what Goodwin himself calls his first life, namely the Harvard period. At the end of 1949, with the dissent of only Schumpeter and Haberler, Harvard decided that Goodwin had no future as an economist. He took a Rockefeller scholarship and went to work with Richard Stone at the Department of Applied Economics, Cambridge, UK. When his year was up the Keynesians (mainly Richard Kahn) offered him a Lectureship. He accepted the offer and settled into his second home. At this time McCarthyism was making life unpleasant for left-wing people in the USA. He remained in Cambridge, at Peterhouse, until his retirement nearly 30 years later.

By far the most important and influential piece in the field of cycles and growth produced during his second period was his superb "A Growth Cycle" [II] read at the first World Congress of the Econometric Society in Rome in 1965. The model is elegant and simple. Capitalists invest all they save, workers consume all their wages; the rate of change of the real wage depends on the unemployment rate: no money illusion is involved. There are fixed coefficients and two factors only, capital and labour; the labour force and the labour productivity grow at a constant rate. As has been noted, the model is an interpretation of Marx's contradictory process of accumulation as the following quotation shows [II:171]:

When profit is greatest employment is average and the high growth rate pushes employment to its maximum which squeezes the profit rate to its average value. The

deceleration in growth lowers employment relative to its average value again, where profit and growth are again at their nadir. This low growth rate leads to a fall in output and employment to well below full employment, thus restoring profitability to its average value because productivity is now rising faster than wage rates (...). The improved profitability carries the seeds of its own destruction by engendering a too vigorous expansion of output and employment thus destroying the reserve army of labour and strengthening labour's bargaining power.

The cyclical process and therefore the crisis is not caused by a Keynesian lack of effective demand but by the operation of the labour market through competition between workers that squeezes profits. The result is that income grows at a rate that fluctuates around the Harroldian natural rate which can be attained only if initial conditions are appropriate. Fluctuations will be wider, the farther the distance from the natural rate at time 0, i.e. the amplitude depends crucially on the initial conditions (past history). However for each cycle it is true that the average values of the unemployment rate and the wage share are constant and equal to the coordinates of the centre of the fluctuation. This is a feature of the mathematical structure of the model. The latter is formally identical to the prey-predator model originally elaborated by Lotka and greatly expanded by Volterra. In particular the (nonzero) stationary solution is a centre. This means that the solution is dynamically stable, though not asymptotically stable, and structurally unstable. This feature is not a drawback since the model has been interpreted (Vercelli, 1982: 186) to be:

suitable either as the ideal type at a high level of abstraction or as a general frame of reference for average and long term inter-cyclical comparisons.

The model has been generalized in a number of directions (by the introduction of financial and monetary variables, effective demand, government expenditure and taxation, etc.) producing a relevant literature but only in a few cases have its qualitative features been preserved.

During the Cambridge period the strand of thought expressed in "Multiplier as Matrix", i.e. the diagonalisation procedure was applied [III] to the problems of the labour theory of value and the transformation problem, to the dual dynamic theory of value and output, to the Wicksell theory of capital and the problem of reswitching

(it is argued that the latter is impossible in a purely circulating capital model where wages are paid in advance).

Finally we cannot overlook Goodwin's brilliant paper on "The Optimal Growth Path for an Underdeveloped Economy" [II] inspired during his period with the Indian Planning Commission under the direction of Mahalanobis. He spent nine months there and managed to build the first input-output table (12 sectors) for India despite poverty of reliable data. It is argued that for an underdeveloped country moving from a traditional low productive economy to a more advanced (high productive) one, it is optimal, under simplified hypotheses, to compress consumption in the early phases of the transition so as to raise the rate of growth and enable a rise in consumption later. It can be shown that this policy maximizes the utility of consumption per capita over the whole period. It is also shown with various numerical examples that the results are rather robust with respect to changes in labour force growth, value of the capital/output ratio, etc. From the mathematical point of view the problem is one of Calculus of Variations where an integral (representing the overall value of future consumption) is maximized over a finite interval, given technical conditions and labour force growth. The problem becomes then one of feasibility, namely how planners can induce the high and rising propensity to save necessary to build machines that will permit a rise in consumption per capita only much later. There are also ethical problems involved since the sacrifice of today's generations will benefit later generations. However the task of the economist is to present the problem and to indicate a range of solutions from which governments can choose.

3. The Cambridge period ended with Goodwin's retirement in 1980. In the autumn of the same year the Department of Political Economy of the University of Siena, taking advantage of a new bill that allowed foreigners to become professors in Italian Universities, invited Dick Goodwin to apply for a chair in Siena. This marked the beginning of the third period of his life which, under the stimulus provided by young Department members, has been both productive and happy. Already towards the end of the Seventies and increasingly in the Siennese period, Goodwin had come across exciting new developments in

mathematics that he thought could be happily employed in solving problems faced by economists in their analysis of the evolution of capitalism. I am referring especially to the catastrophe theory of Thom and Zeeman, chaos theory (Li and Yorke), and finally bifurcation theory.

Goodwin has emphasized time and again that to explain persistent cycles nonlinear models are necessary since otherwise one has to rely on random shocks to keep the cycle alive. The maintenance of the cycle would be the result of chance or at best exogenous events. In Goodwin's vision the role of random events is confined to rendering more irregular the perfect cycle that results from a deterministic model. However the existence of chaotic motions is regarded by Goodwin as a new source (in addition to random shocks) of irregularity of fluctuations occurring in time series. In this respect the economy is like the weather: both are highly irregular so that predictions are unreliable or even impossible. The existence of chaos undermines the simple dichotomy between deterministic models (whose trajectories, given the laws of motion and initial conditions, can be predicted) and stochastic models. Prediction becomes faulty since an arbitrarily small change in initial conditions may lead to trajectories that diverge from one another as time goes on. This is especially relevant in economics since initial conditions cannot be measured exactly. Goodwin built several models that display chaotic trajectories published in a book "Chaotic Economic Dynamics" [IV] which includes essays on the (Ricardian) corn economy, the von Neumann model and an integrated model of Kitchin and Juglar cycles based on multiplier-accelerator interaction.

The other major and new development in Goodwin's thought in Siena has been his interest in long waves. These were popularized by Schumpeter in his "Business Cycles" but were relegated to oblivion until mid-seventies (at least in English speaking countries). Since his participation in a Conference on Long Waves in Weimar in 1985 Goodwin has produced several models attempting to formalize the Schumpeterian idea that capitalism evolves in an unsteady fashion through short, medium and long cycles. In "Towards a Theory of Long Waves" [V] he formulates a model in which in addition to induced investment (via the accelerator), there is also innovative investment which follows a logistic

pattern. He assumes that inventions grow more or less at a constant rate but innovative investment goes up and down. Such investment is positively affected by the level of output since innovations are less risky to introduce when demand is high. By means of simulations he is able to show that within the time span of the logistic (approximately 45 years) the economy exhibits a complete long wave accompanied by shorter fluctuations similar to trade cycles. Here is what lies behind the upper turning point [V: 58]:

(...) there is a positive feedback from investment in the innovative concepts through a multiplier effect to output levels and growth rates, leading to rising effective demand and hence accelerating innovative investment. (...) However as innovation accelerates it gradually reduces the gap between potentially feasible conceptions and their embodiment.

These words contain an unusual justification of the logistic which is therefore not only an empirical representation of the innovative process but has a theoretical foundation. Again his approach combines Keynesian and Schumpeterian ideas, the two theories always being regarded as complementary. The pattern that results is broadly in accordance with observed facts as recently reviewed by Kleinknecht (1987).

Before turning to a general assessment of Goodwin's scientific work, it is well to include a short review of a representative paper on multi-sectoral analysis, "The Use of Gradient Dynamics in Linear General Disequilibrium Theory" [V] in which he summarizes his lifelong study. He examines the stability problem of a general equilibrium linear model of Walras-von Neumann type. The question one wishes to answer within the model is not "Does an equilibrium exist" but "Does a freely functioning, competitive economy achieve a given equilibrium solution?": If not, the equilibrium is devoid of meaning. Goodwin formalizes two pairs of adjustment processes. The first is that in each sector price change depends on the difference between price and unit cost, whereas output change depends on the difference between supply and demand: in this case the system is asymptotically stable and no fluctuations occur. The second hypothesis is that price variation depends on the difference between demand and supply whereas output variation depends on the difference between price and cost: in this situation there will be $2n$ (n being the sectors) centres exhibiting simple

harmonic motion. Considering pairs of motions there are almost certain to be one or more irrational ratios between periodicities, so that the shape of the resultant waves will never repeat and there will be no strict periodicity. If there are many irrational ratios the resulting time series may appear chaotic. This picture is made even more complicated by considerations about stocks and inventories that give rise to further reasons for instability.

4. In the field of political economy Goodwin devoted his efforts to the study of the evolution of capitalism, taking inspiration from Marx, Keynes, and Schumpeter. He rejected neoclassical analysis mainly because, being essentially a static theory, it leaves out precisely what most matters, and its greatest achievement, namely welfare prescriptions, is crucially based on the untenable hypothesis of full employment. From Marx he did not take the labour theory of value and its conundrums but his insights into the laws of movement of the capitalist system: in particular the industrial reserve army mechanism and the idea that in the long run labour is never in short supply. Coupled with the view of capitalists as an organ of accumulation, this is the core of the Marxist vision. From Keynes he took the rejection of Say's law so that output depends on demand. This in general will entail a positive level of unemployment so that the powerful analytical concept of the multiplier can be profitably used. In addition prices are determined basically by costs, especially labour costs (and raw materials costs). This duality between prices and output is perhaps his most long-standing idea. From Schumpeter he took the idea that cycle and development are not only linked but must be explained within a unified mechanism centred on the innovation process. The profits squeeze due to the working of the labour market is periodically relaxed by innovation swarms. He shares with Schumpeter the belief that Walrasian theory describes an ideal state never actually reached by the economy. He finds that von Neumann's model is not inconsistent with Schumpeter's vision since the rate of profits is positive only when the rate of growth is positive.

Coming now to some of Goodwin's more personal features we first stress his lifelong attention to sciences like physics, biology, and applied mathematics with a view to capturing methods or instruments to

help conceptualize the complicated world of the economy. Secondly we notice his faith in nonlinear models of the cycle that can only provide explanations for self perpetuating oscillations. Thirdly in his multi-sectoral models he employs a framework based on the impulse-response (or injection and diffusion) mechanism as a way of analysing the response of the economy to an external force. Hence the long run tendencies are described by a nonlinear mechanism whereas the way in which the injections are diffused within the system in the medium run, is analysed by linear multi-sectoral models. To see how this enormously difficult task is accomplished, the reader is referred to the first part of [VI]. It is a revised version of his Siennese Lectures to undergraduates, coauthored with Punzo, and is completely different from the Treatise on Nonlinear Business Cycles (really called "Mathematical Methods in Dynamical Economics" [I]) that Samuelson in his Preface to [VI] recalls to have been in preparation around 1950. But as Samuelson himself writes:

Good Burgundy is worth waiting for. Vintage produce from the Goodwin workshop repays us with compound interest for our long abstinence.

References

- [I] Goodwin, R.M., The original unpublished documents are kept in the Goodwin Fund at the Faculty of Economics and Banking Library in the University of Siena. The material is currently being catalogued.
- [II] Goodwin, R.M., 1982, *Essays in Economic Dynamics* (London: Macmillan)
- [III] Goodwin, R.M., 1983, *Essays in Linear Economic Structures* (London: Macmillan)
- [IV] Goodwin, R.M., 1988, *Chaotic Economic Dynamics*, to appear in a revised form in an Oxford University Press edition (1990)
- [V] Goodwin, R.M., 1989, *Essays in Nonlinear Economic Dynamics* (Frankfurt am Main: P. Lang)
- [VI] Goodwin, R.M.-Punzo, L.F., 1987, *The Dynamics of a Capitalist Economy* (Cambridge: Polity Press)
- Kleinknecht, A., 1987, *Innovation Patterns in Crisis and Prosperity* (London: Macmillan)

Vercelli, A., 1982, "Is Instability Enough to Discredit a Model", *Economic Notes*

Elenco dei Quaderni pubblicati

- n.1 (febbraio 1979) MASSIMO DI MATTEO, Alcune considerazioni sui concetti di lavoro produttivo e improduttivo in Marx
- n.2 (marzo 1979) MARIA L. RUIZ, Mercati oligopolistici e scambi internazionali di manufatti. Alcune ipotesi e un'applicazione all'Italia
- n.3 (maggio 1979) DOMENICO MARIO NUTI, Le contraddizioni delle economie socialiste: una interpretazione marxista
- n.4 (giugno 1979) ALESSANDRO VERCELLI, Equilibrio e dinamica del sistema economico-semantica dei linguaggi formalizzati e modello keynesiano
- n.5 (settembre 1979) A. RONCAGLIA - M. TONVERONACHI, Monetaristi e neokeynesiani: due scuole o una?
- n.6 (dicembre 1979) NERI SALVADORI, Mutamento dei metodi di produzione e produzione congiunta
- n.7 (gennaio 1980) GIUSEPPE DELLA TORRE, La struttura del sistema finanziario italiano: considerazioni in margine ad un'indagine sull'evoluzione quantitativa nel dopoguerra (1948-1978).
- n.8 (gennaio 1980) AGOSTINO D'ERCOLE, Ruolo della moneta ed impostazione antiquantitativa in Marx: una nota
- n.9 (novembre 1980) GIULIO CIFARELLI, The natural rate of unemployment with rational expectations hypothesis. Some problems of estimation
- n.10 (dicembre 1980) SILVANO VICARELLI, Note su ammortamenti, rimpiazzi e tasso di crescita
- n.10 bis (aprile 1981) LIONELLO F. PUNZO, Does the standard system exist?
- n.11 (marzo 1982) SANDRO GRONCHI, A meaningful sufficient condition for the uniqueness of the internal rate of return
- n.12 (giugno 1982) FABIO PETRI, Some implications of money creation in a growing economy
- n.13 (settembre 1982) RUGGERO PALADINI, Da Cournot all'oligopolio: aspetti dei processi concorrenziali
- n.14 (ottobre 1982) SANDRO GRONCHI, A Generalized internal rate of return depending on the cost of capital
- n.15 (novembre 1982) FABIO PETRI, The Patinkin controversy revisited
- n.16 (dicembre 1982) MARINELLA TERRASI BALESTRIERI, La dinamica della localizzazione industriale: aspetti teorici e analisi empirica
- n.17 (gennaio 1983) FABIO PETRI, The connection between Say's law and the theory of the rate of interest in Ricardo
- n.18 (gennaio 1983) GIULIO CIFARELLI, Inflation and output in Italy: a rational expectations interpretation
- n.19 (gennaio 1983) MASSIMO DI MATTEO, Monetary conditions in a classical growth cycle
- n.20 (marzo 1983) M. DI MATTEO - M.L. RUIZ, Effetti dell'interdipendenza tra paesi produttori di petrolio e paesi industrializzati: un'analisi macrodinamica
- n.21 (marzo 1983) ANTONIO CRISTOFARO, La base imponibile dell'IRPEF: un'analisi empirica
- n.22 (gennaio 1984) FLAVIO CASPRINI, L'efficienza del mercato dei cambi. Analisi teorica e verifica empirica
- n.23 (febbraio 1984) PIETRO PUCCINELLI, Imprese e mercato nelle economie socialiste: due approcci alternativi

- n.24 (febbraio 1984) BRUNO MICONI, Potere prezzi e distribuzione in economie mercantili caratterizzate da diverse relazioni sociali
- n.25 (aprile 1984) SANDRO GRONCHI, On investment criteria based on the internal rate of return
- n.26 (maggio 1984) SANDRO GRONCHI, On Karmel's criterion for optimal truncation
- n.27 (giugno 1984) SANDRO GRONCHI, On truncation "theorems"
- n.28 (ottobre 1984) LIONELLO F. PUNZO, La matematica di Sraffa
- n.29 (dicembre 1984) ANTONELLA STIRATI, Women's work in economic development process
- n.30 (gennaio 1985) GIULIO CIFARELLI, The natural rate of unemployment and rational expectation hypotheses: some empirical tests
- n.31 (gennaio 1985) SIMONETTA BOTARELLI, Alcuni aspetti della concentrazione dei redditi nel Comune di Siena
- n.32 (febbraio 1985) FOSCO GIOVANNONI, Alcune considerazioni metodologiche sulla riforma di un sistema tributario
- n.33 (febbraio 1985) SIMONETTA BOTARELLI, Ineguaglianza dei redditi personali a livello comunale
- n.34 (marzo 1985) IAN STEEDMAN, Produced inputs and tax incidence theory
- n.35 (aprile 1985) RICHARD GOODWIN, Prelude to a reconstruction of economic theory. A critique of Sraffa
- n.36 (aprile 1985) MICHIO MORISHIMA, Classical, neoclassical and Keynesian in the Leontief world
- n.37 (aprile 1985) SECONDO TARDITI, Analisi delle politiche settoriali: prezzi e redditi nel settore agroalimentare
- n.38 (maggio 1985) PIETRO BOD, Sul punti fissi di applicazioni isotoniche
- n.39 (giugno 1985) STEFANO VANNUCCI, Schemi di gioco simmetrici e stabili e teoremi di possibilità per scelte collettive
- n.40 (luglio 1985) RICHARD GOODWIN, The use of gradient dynamics in linear general disequilibrium theory
- n.41 (agosto 1985) M. MORISHIMA and T. SAWA, Expectations and the life span of the regime.
- n.42 (settembre 1985) ALESSANDRO VERCELLI, Keynes, Schumpeter, Marx and the structural instability of capitalism
- n.43 (ottobre 1985) ALESSANDRO VERCELLI, Money and production in Schumpeter and Keynes: two dichotomies
- n.44 (novembre 1985) MARCO LONZI, Aspetti matematici nella ricerca di condizioni di unicità per il Tasso Interno di Rendimento
- n.45 (dicembre 1985) NIKOLAUS K.A. LAUFER, Theoretical foundations of a new money supply hypothesis for the FRG.
- n.46 (gennaio 1986) ENRICO ZAGHINI, Una dimostrazione alternativa dell'esistenza di equilibri in un modello di accumulazione pura
- n.47 (febbraio 1986) ALESSANDRO VERCELLI, Structural stability and the epistemology of change: a critical appraisal
- n.48 (marzo 1986) JOHN HICKS, Rational behaviour: observation or assumption?
- n.49 (aprile 1986) DOMENICO MARIO NUTI, Merger conditions and the measurement of disequilibrium in labour-managed economies
- n.50 (maggio 1986) SUSAN SENIOR NELLO, Un'applicazione della Public Choice Theory alla questione della riforma della Politica Agricola Comune della CEE
- n.51 (maggio 1986) SERENA SORDI, Some notes on the second version of Kalecki's Business Cycle Theory
- n.52 (giugno 1986) PIERANGELO GAREGNANI, The classical theory of wage and the role of demand schedules in the determination of relative prices
- n.53 (luglio 1986) FRANCO BIRARDI, Crescita, sviluppo ed evoluzione: l'affermazione dei nessi strutturali sistematici e la combinazione di scienza, tecnologia ed economica nelle prime industrializzazioni
- n.54 (agosto 1986) M. AMENDOLA - J.L. GAFFARD, Innovation as creation of technology: a sequential model
- n.55 (settembre 1986) GIULIO CIFARELLI, Exchange rates, market efficiency and "news". Model specification and econometric identification
- n.56 (settembre 1986) ALESSANDRO VERCELLI, Technological flexibility, financial fragility and the recent revival of Schumpeterian entrepreneurship
- n.57 (ottobre 1986) FABIO PETRI, The meaning of the Cambridge controversies on capital theory: an attempt at communication
- n.58 (ottobre 1986) DANIELE CHECCHI, The international coordination of counter-cyclical policies
- n.59 (novembre 1986) ALESSANDRO VERCELLI, Probabilistic causality and economic models: Suppes, Keynes, Granger
- n.60 (dicembre 1986) LIONELLO F. PUNZO, J. Von Neumann and K. Menger's mathematical colloquium
- n.61 (dicembre 1986) MICHAEL BACHARACH, Three scenarios for fulfilled expectations
- n.62 (gennaio 1987) J. ESTEBAN and T. MILLAN, Competitive equilibria and the core of overlapping generations economies
- n.63 (gennaio 1987) ROBERT W. CLOWER, New directions for keynesian economics
- n.64 (febbraio 1987) MARCO P. TUCCI, A simple introduction to flexible functional forms and consumer behaviour theory
- n.65 (marzo 1987) NICOLA DIMITRI, Generalizations of some continuous time epidemic models
- n.66 (aprile 1987) MASSIMO DI MATTEO, Goodwin and the Evolution of a Capitalistic Economy: An Afterthought
- n.67 (maggio 1987) MASSIMO DI MATTEO, Early Discussions on Long Waves
- n.68 (giugno 1987) MASSIMO DI MATTEO, Warranted, Natural, and Actual Rates of Growth: Reflections of a Perplex
- n.69 (settembre 1987) LUISA MONTUSCHI, Inward-Looking development and import substitution in the Argentine Economy 1950-1980
- n.70 (dicembre 1987) GIANCARLO GANDOLFO and PIETRO CARLO PADOAN, The Mark V version of the Italian continuous time model
- n.71 (dicembre 1987) SILVANO VICARELLI, Impatience to consume, the utility of wealth and the rate of interest
- n.72 (gennaio 1988) OMAR CHISARI, Worker's assets, foreign debt and the dynamics of an economy
- n.73 (febbraio 1988) STEFANO VANNUCCI, On exactly strong-Nash-implementable power allocations
- n.74 (marzo 1988) PIER LUIGI LANDI, Aspetti economici della società anonima della strada ferrata Empoli-Siena o Centrale Toscana fino al 1859
- n.75 (aprile 1988) FRANCO BIRARDI, Generating the space of physical characteristics by means of elements which are discrete and limited over time (A note on innovation in chemical technologies)
- n.76 (maggio 1988) LILIA COSTABILE, Politica monetaria, debito pubblico e tasso di interesse

- n.77 (giugno 1988) GIUSEPPE DELLA TORRE, L'evoluzione dei sistemi finanziari nell'approccio contabile di Raymond Goldsmith.
Alcune considerazioni analitiche
- n.78 (luglio 1988) FRANCESCO FARINA, Fattori tecnologici e transazionali nella divisione interindustriale del lavoro
- n.79 (ottobre 1988) FRANCESCO FARINA, Gli obiettivi della Banca d'Italia e i tassi d'interesse sul debito pubblico
- n.80 (novembre 1988) GIULIO CIFARELLI, La razionalità del tasso di cambio a termine e le "notizie": una analisi empirica
- n.81 (dicembre 1988) MAURO CAMINATI, Cyclical growth in a long-period perspective
- n.82 (gennaio 1989) LUIGI LUINI, Crescita esterna dell'impresa e teorie economiche
- n.83 (marzo 1989) LUIGI M. TOMASINI, A Scheme to Allocate Wastewater Rights
- n.84 (aprile 1989) STEFANO VANNUCCI, On Consistency Properties of some Strongly Implementable Social Choice Rules with Endogenous Agenda Formation
- n.85 (maggio 1989) AGOSTINO D'ERCOLE, Economia e Moneta in Schumpeter
- n.86 (maggio 1989) CHARLES GOODHART, What do Central Banks Do?
- n.87 (giugno 1989) PAOLO DEL GIOVANE, Foreign Firms' Behavior in the United States Market during the Dollar Cycle
- n.88 (giugno 1989) GÉRARD DUMENIL and DOMINIQUE LÉVY, The classical legacy and beyond
- n.89 (luglio 1989) RICCARDO FIORITO, Inventory accounting and profit evaluation
- n.90 (settembre 1989) NICOLA DIMITRI, On the robustness of the rational expectations hypothesis
- n.91 (settembre 1989) PIERMARIO PACINI, Expectations, learning and equilibrium in a non-walrasian model: a constructive approach
- n.92 (ottobre 1989) AGOSTINO D'ERCOLE, Sulla teoria monetaria del tasso di interesse di Schumpeter
- n.93 (ottobre 1989) UGO PAGANO, Firms, co-ordination and disequilibrium
- n.94 (novembre 1989) UGO PAGANO, Determinazione endogena dei diritti di proprietà in una economia di mercato
- n.95 (dicembre 1989) UGO PAGANO, Property rights, asset specificity, and the division of labour under alternative capitalist relations
- n.96 (dicembre 1989) MICHIO MORISHIMA, Wage differentials in Japan: 1958-1985
- n.97 (gennaio 1990) PIERPAOLO PIERANI - PIER LUIGI RIZZI, An econometric analysis of the olive oil market in Italy
- n.98 (febbraio 1990) ENRICO GIOVANNINI, Squilibri settoriali e dinamica macroeconomica di breve periodo: il caso dell'industria italiana
- n.99 (maggio 1990) GIULIO CIFARELLI Sui determinanti degli errori di previsione a termine nel mercato dei cambi

Alla presente Collana "gialla" di quaderni il Dipartimento di Economia Politica affianca una Collana "celeste" di monografie. I numeri pubblicati a tutt'oggi sono i seguenti:

- 1) GIUSEPPE DELLA TORRE, Istituzioni creditizie ed Accumulazione del Capitale in Italia (1948-81), 1984.
- 2) SANDRO GRONCHI, Tasso Interno di Rendimento e Valutazione dei Progetti: una Analisi Teorica, 1984.
- 3) ALDINO MONTI, La Proprietà Immobiliare a Bologna in Età Napoleonica (1797-1810), 1984.
- 4) GIULIO CIFARELLI, Equilibrium and Disequilibrium Interpretations of Inflation, 1985.