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**Richard M. Goodwin: a pioneer in the field of
economic dynamics
between the two Cambridges**

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**Richard M. Goodwin: a pioneer in the field of economic dynamics
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Abstract

In the attempt to answer three (interrelated) questions, we concentrate on a crucial step in Goodwin's (RMG) life: his move from Cambridge, Mass. to Cambridge, UK. Why did RMG not get a permanent position at Harvard? How did RMG reach the decision to settle down in Cambridge, UK? Why was it that RMG never finished the book on economic dynamics he had started to write at Harvard? In answering these questions we show that the material available in the Goodwin Archive at the University of Siena is invaluable and adds much to the 'known story'.

KEYWORDS: Richard M. Goodwin; economic dynamics; archive; Harvard University; Cambridge University

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1. Introduction

The story of Richard M. Goodwin (RMG)'s 'nonlinear' academic life is well known and described in a number of contributions (e.g., Di Matteo, 1990, 2000; Harcourt, 1985; Palazzi, 1982; Pasinetti, 1996; Punzo & Velupillai, 1996a, 1996b; Velupillai, 1990, 1998a, 1998b). This story can be summarised as follows. RMG arrived at Harvard University in 1930, where he read Political Science. In 1934 he graduated *Summa Cum Laude* with a dissertation entitled *A Critique of Marxism* and was awarded the Rhodes Scholarship to Oxford where he spent the next three years, reading PPE and gaining first a BA in 1936 and then a BLitt in 1937 under the supervision of Henry Phelps Brown. In 1938 he returned to Harvard University, gained an MA in 1939, became an instructor in Economics and finally received his PhD in 1941 with a thesis entitled *The Supply of Money in England and Wales, 1919 to 1938*, which he wrote utilising the material he had collected for his BLitt Thesis at Oxford. During the Second World War, he remained in Cambridge, Mass. and, in addition to some courses in Economics, also taught Physics to undergraduates and to Army and Navy officers. After the War, in 1946, he accepted an appointment as assistant professor in the Department of Economics. At the end of 1949, he was rejected for permanent appointment. Following a suggestion by Richard Stone – whom he had met in Driebergen, Holland, in September 1950 at the First International Conference on Inter-Industrial Relations – he then applied for a Fulbright Scholarship and in the summer of 1951 left Cambridge, Mass. for Cambridge, UK to work for one year in the Department of Applied Economics, at that time directed by Stone. At the end of his period there as a Fulbright Fellow, he applied – supported by Joan Robinson and Richard Kahn – for a Lectureship in Economics. He got the job, started his appointment in October 1952 and remained in Cambridge, UK till his first retirement at the age of 67. At this point he took up a Professorship at the University of Siena where he taught until his second retirement in 1988 at the age of 75 and remained active as a teacher of graduate students until almost the end of his life. Just after his retirement, he was kind enough to donate to the library of the Faculty of Economics the whole set of his writings and papers. Over the years, this material has been organised in a catalogue, which is now available in the Archive Section of the library (for a short description of its structure and contents, see Di Matteo, Filippi & Sordi, 2006) and through the Internet on the Web site of the *Archivio Storico degli Economisti* (ASE), at <http://ase.signum.sns.it/>.

In this article we intend to concentrate on one of the crucial steps in RMG's life, namely his move from Cambridge, Mass. to Cambridge, UK, in the attempt to answer three (interrelated) questions. First, why did RMG not get a permanent position at Harvard? Second, how did RMG come to decide to settle

down in Cambridge, UK? Third and finally, why was it that RMG never finished the book on economic dynamics he had started to write at the end of his stay at Harvard? Our main purpose is to show that the material available in the Goodwin Archive at the University of Siena is of profound importance in this regard and adds much to the ‘known story’. To do this, the paper is organised as follows. In Section 2 we broach the thorny subject of RMG’s defeat at Harvard University. In Section 3 we discuss RMG’s difficult decision to leave Cambridge, Mass. for Cambridge, UK. In section 4 we consider the reasons why RMG never finished the book he had started to write in that period. Section 5 concludes.

2. The ‘defeat’ at Harvard

The decision concerning RMG’s tenure at Harvard University was taken at the end of 1949, just a few days before Schumpeter’s death.¹ There are a number of possible reasons for it, and they fall into three main categories.

First, there is a reason that we may call ‘*political*’. RMG, during his stay at Harvard University, both as a student and as a member of the faculty, was a passionate left-wing activist (having been a member of the British Communist Party until 1939). For this reason he can be considered as a ‘victim’ of McCarthyism (Harcourt, 1985, p. 416; Desai & Ormerod, 1996, p. 1433). This interpretation is strengthened by the circumstance that the Chair of the Department of Economics at the time when the tenure decision was taken was Harold H. Burbank, a right-winger who even had a reputation for being xenophobic and anti-Semitic.²

¹ In his intervention at the Celebration of RMG’s 75th birthday, held in Siena on 25 March 1988, Samuelson reconstructed the event as follows: ‘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 [...] 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 tenured position at Harvard”’ (Di Matteo, Ed., 1990, p. 18; emphasis added).

² This is stressed by Modigliani (Barnett & Solow, 2000, p. 226), who, in trying to give an explanation of why at the end of the 1940s he had turned down an offer from the Economics Department of Harvard University, says that ‘the head of the department, Professor Burbank, whom I later found out had a reputation for being xenophobic and anti-Semitic, worked very hard and successfully to persuade me to turn down the offer, which the faculty had instructed him to make me.’ Moreover, Mason (1982, p. 413), in his detailed description of the Economics Department from its beginning to World War II, adds that Burbank, who died in 1951, in his later years, ‘was a disappointed, disgruntled, reactionary figure who had influence in the Department mainly because of his willingness, even alacrity, in taking over the chairmanship, a job shunned by almost everyone else.’ It should also be stressed that, in addition to Goodwin and Modigliani, Burbank’s period as Chair of the Economics Department also saw other bright

Second, reasons of a more ‘*academic*’ nature can also be found. We are told that RMG could count ‘only’ on the support of Schumpeter, Haberler and perhaps Leontief.³ Moreover, and more importantly, in a letter John Williams sent to RMG on 27 July 1951 (II.a.337),⁴ when RMG had already left Harvard and was about to leave the United States for England, we find an illuminating reference to the so-called *Graustein formula*. After having expressed his regret for RMG’s departure, and explained to RMG that he had tried to say something on his behalf, Williams writes: ‘Our real trouble, however, is with the Graustein formula, and unless we can someday lick it, the future of Harvard is in serious jeopardy, at least in economics.’ Mason’s article (1982, pp. 426-429) helps us in understanding this reference to the Graustein formula. We learn that at the end of the 1930s, mainly as a result of the discussion about the Walsh-Sweezy case, it was firmly decided 1) that any decision concerning tenure at Harvard University had to be taken within eight years from the conferring of a PhD and 2) that each Department could make recommendations for the two ranks with permanent tenure – namely, for associate and full professorships – with a frequency strictly determined by a mathematical formula, precisely the Graustein formula (so called after the Harvard mathematician who had invented it). To make possible the application of this formula, in 1939 it was established that the average length of a permanent appointment was 34 years and all faculties were asked to determine the ‘historical size’ of each of their departments.⁵ Given these figures, the Graustein formula established that the number of years between appointments to a department had to be equal to the ratio between the average length of a permanent appointment and the ‘historical size’ of the department. For example, a department with a ‘historical size’ of 34 members could appoint a new tenured member every year whereas a department of half the size could do so only every two years.⁶ The combination

young men (such as Galbraith, P. Sweezy, Samuelson and Solow) leave Harvard University. Cfr. also Velupillai (1998a, p. 1436).

³ As stressed by Harcourt (1985, p. 417) there are reasons to cast doubt on the support of Leontief regarding the permanent appointment at Harvard University. Cfr. also Foley (1988, p. 120).

⁴ Here and in the rest of the article for each item we cite we indicate its catalogue number in the Goodwin Archive at the University of Siena.

⁵ In the *Official Register of Harvard University*, XXXVIII, April 10, 1941, No. 20, containing the ‘Report of the President of Harvard College and Reports of Departments for 1939-40’, there is a section entitled ‘The Application of the New Plan of Tenure.’ We learn that the University was in a period in which its income had ceased to grow and therefore its expenditures also had to cease to grow. For this reason (p. 43; emphasis added), ‘the *permanent staff, the salaries of which constitute the fixed charge of largest magnitude on our budget, should be stabilized at its present figure.*’

⁶ As noted by Norman (1949, p. 2), two disadvantages of such a system are self-evident. First, for a small department it could often happen that it had nothing to offer to outstanding scholars if they turned up looking for a permanent position in a year when no ‘slots’ were available. Second, adherence to the system ‘froze’ the structure and distribution of the faculties even when a drastic change would have been required.

of the two requirements certainly cut down drastically RMG's chances of success: he had defended his Ph.D. thesis in 1941 so that the decision about his tenure had to be taken before the end of 1949, exactly in a moment when his productivity was flourishing after the war years and when his contributions to economic dynamics were rapidly starting to make their impact on the field.⁷

Finally, there is another important reason for RMG's defeat which has often been indicated and which is of a more '*scientific*' nature, namely the fact that in 1949 he had not yet produced a book, something that was understood as a requirement for tenure. RMG himself appears to have been aware of his 'weakness' as is testified by a letter he sent to Schumpeter just before the decision about his tenure was due.⁸ Reading it, we understand that Schumpeter had asked RMG to send him a list of his publications that he could use to support his tenured appointment before the committee that had to take the decision. RMG writes (emphasis added):

Dear Prof. Schumpeter: here is the list you asked for. I am not altogether ashamed of it, but I must admit that it displays neither concentrated excellence, nor a vast mound of published [junk ?] so much admired here. So *I see clearly that I provide you no good case to make, and I accept the full responsibility for the defeat that is coming*⁹

It is difficult, on the basis of the information we have, to say which of the reasons we have discussed for RMG's defeat played the most important role, possibly all played some role and probably the fact that he had not yet produced a book was considered the most serious one. The impression however is that all together they 'shaded the evaluation of his scientific work' (Foley, 1998, p. 120). RMG's main publications at the moment of the tenure decision included Goodwin (1941, 1943, 1944, 1945, 1946, 1947a, 1947b, 1947c, 1948, 1949) – and even Goodwin (1950a, 1950b, 1950c, 1951a, 1951b, 1951c), although not in press at the moment of the decision, certainly belong to the Harvard period. Concepts such as the *dynamical coupling of markets* and the extrapolative/regressive expectations introduced in Goodwin (1947a), the *dynamic multiplier* of Goodwin (1947c), the *flexible accelerator* of Goodwin

⁷ We will come back later to this point.

⁸ A copy of this letter was sent back to RMG by Ester Fano on 15 December 1986 and its catalogue number in the Goodwin Archive is II.a.75. In the accompanying letter, Fano explains to RMG that she had found the letter at Harvard University, while inspecting Schumpeter's correspondence.

⁹ He then tries to give a reason for his failure (emphasis added): 'I am a victim of the averted gaze. I begun by studying *politics* and *philosophy* and felt that to understand them I had to turn to *economics*. I have failed to find the satisfaction of understanding, but in the pursuit I have had much pleasure and come to the conclusion that I might find some clue to economics in *mathematics*. But this is an infinite regression with little convergence and substantial defeat at the end.'

(1948) and the *modelling of the Leontief system* presented in Goodwin (1949) – not to mention the *nonlinear accelerator* of Goodwin (1950a) and Goodwin (1951a) – had an immediate impact on the literature and are still used to this day. A brief search using online resources such as JSTOR, EconLit, Periodical Archives Online and Elsevier Science Direct has shown us that Goodwin (1947a, 1947c, 1948, 1949) alone had, by the end of 1949, already been cited 14 times in outstanding journals such as *Econometrica*, *American Economic Review* (4 times), *Review of Economic Statistics*, *Southern Economic Journal*, *Review of Economic Studies* (twice), *Quarterly Journal of Economics*, *Journal of Political Economy*, *Economica* and *Ekonomisk Tidskrift* (twice).¹⁰ The number of citations grows to 29 at the end of 1951¹¹ – when RMG had just arrived in Cambridge, UK – to 71 at the end of the 1950s and to 231 by the end of his life. The corresponding figures for the whole set of papers belonging to the Harvard period are 20, 40, 125 and 486 respectively.

Thus, it appears reasonable to conclude that – at the moment of the decision about his tenure – RMG was a young talented economist who had already written a large number of innovative papers. However, Harvard was perhaps too conservative in its recruiting policy and unable to appreciate original scholars like him.

3. The difficult choice between US and UK

When, in December 1949, RMG was informed of his failure he was still left with some time to look around for a new job.¹² However, the ‘known story’ tells us only that he finally found ‘refuge’ in Cambridge, UK, following a suggestion by Richard Stone whom he had met in September 1950. The letters in the Goodwin Archive add much information to this simple story. On the one hand, they confirm it and help in understanding it; on the other hand they add some pieces that are completely new and worth discussing. These new pieces of information reveal that RMG’s decision about where to settle down was much more difficult than has hitherto been thought.

The letters in the archive that confirm and help us in understanding the ‘known story’ can be briefly summarised as follows. In a letter dated 19 June 1951 (II.a.24), sent to RMG by Brockway, vice-president of W.W. Norton & Company Inc., we find the first reference to the fact that RMG had got an appointment to the University of Cambridge. Thus we learn that RMG,

¹⁰ The two authors citing RMG in this Swedish journal are Erik Lundberg in 1948, who in his article makes reference to Goodwin (1947c), and Johan Åkerman in 1949, who makes reference to Goodwin (1948).

¹¹ The journals now also include *Economic Journal*, *Revue Économique* and *Canadian Journal of Economics and Political Science*.

¹² Indeed, he had been appointed as an Assistant Professor in 1946, on a five-year contract.

although at that time still in Cambridge, Mass., knew already that he had got a Fulbright Scholarship and that he was going to work, for one year, in Stone's department. Then, thanks to a press-cutting – containing information about jobs and with a short hand-written note signed by Richard Kahn and dated 21 May 1952 (II.a.143) – we learn that in May 1952, when the end of the academic year 1951-52 was approaching, two Lectureships, one in Economics, the other in Statistics, were advertised by the University of Cambridge, with 18 June as the deadline for applications and 1 October as the start date. Finally, thanks to a letter by Ruth Cohen, Secretary of the University of Cambridge, dated 5 July 1952 (II.a.37) we learn that RMG was appointed to the Lectureship in Economics, the decision having been taken by the Appointments Committee on 4 July.¹³ This – apart from some letters of congratulation on his appointment sent to RMG by Williams on 14 July 1952 (II.a.338), by Robinson and Kahn, undated, by Leontief on 27 September 1952 (II.a.173) and by Gerschenkron on 1 October 1952 (II.a.96) – is all that the material in the Goodwin Archive adds to the 'known story' about the beginning of RMG's long stay (thirty years) at Cambridge University.

Less known is the fact that RMG had already received – and was still receiving – many other offers, of which two were from UK universities (Manchester and Oxford), seven from US universities (Northwestern-Illinois, Stanford, Williams College-Mass., Carnegie Institute of Technology, Yale, Brandeis, Berkeley), and one from Canada (McGill University-Montreal). Thus, it appears that he was faced with a difficult choice and that his acceptance of the lectureship at Cambridge University was not such a foregone conclusion.

Regarding the offers RMG received from other British universities, it is astonishing to learn that when RMG left Cambridge, Mass. to go to work for one year in Cambridge, UK on a Fulbright Scholarship, he had already been offered the Chair in Economic Theory at the University of Manchester. We are unable on the basis of the letters we found in the archive to reconstruct exactly the beginning of the story, but we can follow the rest of it quite well. In chronological order, the first letter that contains some information in this connection is one by W. Arthur Lewis, at that time professor at the University of Manchester, dated 7 August 1951 (II.a.181) and sent to RMG at his address in Cambridge, Mass. Reading it we understand that there had already been a previous letter – which is missing in the archive – in which the University of Manchester had offered a chair to RMG, and that RMG was playing for time:

My dear Goodwin, many thanks for your letter. I understand very well that you will not want, at this stage, to commit yourself to

¹³ This letter also contains the following short comment on RMG's future teaching at the University of Cambridge: 'Austin Robinson intends to discuss with you the question of your lectures when both he and you are next in Cambridge' and then, hand-written, 'I have now seen Austin; he suggests that you give the equivalent of 1 lecture a week next year, if you like on your dynamic economics'.

Manchester, and do not wish to press anything upon you before you have had a chance to look around. I showed your letter to the Committee responsible for making our appointment, which fully appreciated your position. At the same time, members wished to have the opportunity of meeting you informally, and of discussing our subject with you (i.e., economics), and they hoped you would consent to such a meeting, when you visit us in Manchester, without any commitment. When will that be? We shall be glad to see you at any time, whether before or after the beginning of term. Bring your wife with you. The university will pay all expenses. Drop me a line about your movements

and, as postscript

P.S.: In spite of paragraph one above, I cannot resist adding that if you accepted a chair here, you could stay as long or as short a time as you pleased. The appointment would be for life, but you could resign at the end of one year, two, five, or whenever you liked without any ill feeling [...] In view of the dearth of suitable specialists in economic theory in England, it would help us immensely even if you came only for a year or two, to tide us over the period while the younger post-war graduates gain some experience

The content of a second letter by Lewis (II.a.182), sent this time to RMG's address in Cambridge, UK and dated 20 October 1951 makes us think that the negotiation for the Chair at the University of Manchester was going on well:

My dear Goodwin, The appointments Committee has been fixed for Monday November 12 at 2p.m. I hope this suits you. How about coming up over the week-end, on Saturday the 10th? And staying over as long as is convenient to you. At least until the following Wednesday, which would enable you to be present at our postgraduate & staff seminar, which meets at 2.30. One of the staff is reading a paper on international trade theory, so you would not have to speak, except as the spirit moved you, but could have a chance to meet all our staff & postgraduate students. As for yourself giving a lecture. I have not made any arrangements for this, but would be glad to do this if you wish. The best time would be Tuesday at 3p.m., which is the normal time for one of our third year honours seminars, to which everyone else (i.e. 3rd year + postgraduates) could also be asked. Let me know what you would prefer

Then, in an official letter dated 20 December 1951 (II.a.149), Vincent Knowles, in his role as Registrar of the University of Manchester, explains to RMG the details of the (permanent) appointment (emphasis added):

Dear Sir, I have the pleasure to extend to you an invitation to accept appointment as Professor of Economic Theory¹⁴ in this University, to hold office from a date to be approved by the Vice-Chancellor to September 29th next after you have completed your sixty-fifth year, at a salary at the rate of £1,800 per annum. The appointment is subject to the provision of the Federated Super-annuation System for the Universities and to conditions to be drawn up by the Vice-Chancellor and the Registrar. I will inform you when your Conditions of Appointment are ready for signature

Enclosed with this formal invitation, there is also the following short note by Knowles (II.a.150):

Dear Dr. Goodwin, I am enclosing a formal invitation to you to accept appointment to the Chair of Economic Theory. I understand that you may not be able to accept this offer on the terms stated, but wish you to know that the invitation is on "all fours" with all other invitations to scholars to accept appointment to Professorships and is without any reservation on our part. I shall look forward to receiving your reply in due course, and sincerely hope that the answer will be favourable

The end of the story, as reconstructed on the basis of the letters contained in the Goodwin Archive, is rather abrupt and unexpected. On the basis of the content of two letters, one by W. Mansfield Cooper (II.a.190), as Registrar of the University of Manchester, dated 26 January 1952, the other again by Lewis (II.a.183), dated 29 January 1952, we learn that RMG had not accepted the offer and that his decision did not arrive as a surprise at Manchester. Lewis writes: 'My dear Goodwin, of course we are all very disappointed that you will not come, but you always made your position clear, and we would probably be more surprised if you were coming.' A possible explanation for RMG's decision is that he was looking for a (permanent) position in a more prestigious university and that, in order to achieve that, he was even ready to give up certainty (a permanent Chair in Manchester) for the uncertain (a temporary position in Cambridge).

In a letter dated 28 May 1952 by Alvin H. Hansen (II.a.109) we find the first reference to a possible appointment at the University of Oxford and so we learn that the story was much more complicated than that. In the letter there is no

¹⁴ This is the chair that was held by John Hicks from 1938 to 1946.

mention of the offer RMG had received from the University of Manchester, but Hansen writes, 'I am delighted to learn that you have had a good year and *are looking forward to staying either in Cambridge or Oxford*' (emphasis added). We do not know from the letters we have in the archive when a job offer in Oxford was first made to RMG, but from a letter by G.D.N. Worswick (II.a.338) dated 31 May 1952 we learn that he was the person that had made the offer to RMG and that the offer consisted in a job at the Institute of Statistics of Oxford University.¹⁵ We understand, moreover, that RMG had yet not committed himself to the job and that, on the contrary, had clearly shown his own indecision:

I had reported to Frank [Burchardt] that *you had told me that you were still considering going back to the States next year*, and it has occurred to me since we met that in so far as this is still a fairly definite intention it might be better for both sides to make any appointment here in the first instance for one year. I think I can fairly say that the presumption would be that if you wanted to stay we would then make the usual five year appointment for a post of this kind. If, however, you are pretty certain that you are not going back to the States, we could, of course, put it up as a five year appointment straight away (emphasis added)

However, from another letter by Worswick (II.a.339) dated 10 June 1952, we learn that RMG had not accepted the offer.

What is interesting in the first letter by Worswick is the reference to the fact that RMG is considering going back to the States. If this was truly the reason for his indecision, then it must be case that he had also received some good offers from American universities. The letters we have in the archive allow us to reconstruct the facts in this connection rather precisely.

First of all, we learn that at the beginning of his year in Cambridge as a Fulbright scholar, RMG had received a letter, dated 19 November 1951, from Richard Heflebower (II.a.123), at that time chairman of the College of Liberal Arts of Northwestern University (Evanston, Illinois). Heflebower seems to have a rather high opinion of RMG:

For some time we have been looking for a man of standing and experience, primarily in the field of theory, who would round off our staff. Under the general guidance of S.E. Leland, there has been assembled here a group of men who are not only congenial and competent, but who also are interested in the problems of each other. What we need now is a man with strong qualifications in theory and who is interested in distilling principle from empirical

¹⁵ This is the institute where Michał Kalecki worked from 1940 to 1945 and where many refugees from continental Europe spent the war years.

evidence. Such a man would round out our faculty group and our program for advanced students. Would you be interested in being considered for this position? (emphasis added)

and, to make the offer more appealing, he then writes,

*Incidentally, Professor Strotz has just shown me some of the results from placing your model on the analogue computer. This is an indication I think of the interests of some of our group here which includes not only men from this department but also from the Technological Institute. The latter have been extremely helpful both by developing the analogue computer and in using it on economic models. I understand that you and Professor Strotz have been in correspondence about this work*¹⁶ (emphasis added)

We do not know what RMG replied to this letter, but probably he did not show much interest in the offer. Certainly, in the archive there is no sign of there having been any other correspondence between him and members of Northwestern University.

Secondly, in a letter dated 4 December 1951, Kenneth Arrow (II.a.6), at that time Assistant professor at Stanford University, invites RMG to accept a job there for the academic year 1952-53. We do not know what RMG's answer was, but reading a letter dated 7 January 1952 sent to him by the Chairman of the Department of Economics, E. Shaw (II.a.278), the impression one gets is that RMG, if he were to go back to the USA, felt he had to be offered a permanent position. Shaw in his letter explains that Stanford did not have a permanent position available to offer and that at that time the possibility described by Arrow was the only one available. He finally adds that,

It is not inconceivable that this might mature to a permanent spot within a year or two, although we can by no means count on it. We have no fixed deadline for completing the arrangements Arrow has in mind. Perhaps we had better let it remain this way, that you will advise us if the temporary position becomes more attractive to you

We do not know what happened in between, but in a letter dated 4 November 1952, Shaw (II.a.279) is still insisting, even if the length of the temporary position he is offering to RMG is now shorter: 'I am anxious to discover whether you would possibly have an interest in joining the staff of the

¹⁶ We do not know whether this supposition by Heflebower was right: no sign of any correspondence between RMG and Strotz is left and in the article that Strotz then published (Strotz, McAnulty & Naines, 1953), any help or advice from RMG is not acknowledged.

Department of Economics at Stanford for the Spring Quarter of the present academic year and perhaps the Summer Quarter as well.'

Thirdly, in a letter dated 2 January 1952, Emil Despres (II.a.48), his friend and former colleague at Harvard, offered RMG *a position as full professor at Williams College* (Williamstown, Mass.). The tone of the letter is very friendly. Despres however warns RMG that the College had recently adopted a rule against making tenure appointments without an initial term appointment. In case RMG is only interested in a tenure appointment, he adds, he could try to have the rule waived, but he is not sure he will succeed in that. The letter also contains a detailed description of the terms of the job and refers to the fact that RMG should let Despres know as soon as possible whether he would be interested because he needs to get someone for next autumn. Reading another letter dated 30 January 1952 (II.a.49), we then learn that RMG had not yet replied. Despres writes,

I am eager to know whether my recent letter inquiring whether you'd now be receptive to an offer from Williams caused a moderate flicker of interest on your part. If it did, I don't want to hurry you into making a definitive answer more quickly than you feel you can. If it didn't, I had better start exploring other possibilities, which I have so far refrained from doing because *we would much rather hold the job for you if there's any chance you might be interested*. Please let me know, then, whether you're considering the matter and how soon you might be able to give me a definite reply (emphasis added)

Another letter by Despres (II.a.50), dated 12 February 1952, allows us to understand that RMG has finally replied and gives us some information on the difficulties RMG, together with his wife Jackie, is having in deciding where to settle down:

Dear Dick, I'm sorry that my inquiry precipitated a crisis, but I would have been ever sorrier if it had called forth a categorical "no" in reply. Moreover, I'm pleased to learn that Jackie is all for Williamstown and I hope that her year of purdah at Cambridge¹⁷ is making her insist adamantly on settling here. *If you've decided you couldn't live in Manchester and are considering making New Haven your home*, this can only be because you haven't visited New Haven recently enough to have a clear image. Anyway, I am

¹⁷ To understand this comment by Despres, it is useful to remember that women, although admitted to full university membership since 1948, were not allowed to dine in Cambridge colleges until 1956! The topic of women's higher education at Cambridge is deeply and exhaustively discussed in McWilliams-Tullber (1975).

hoping that Yale, too, is afflicted by a budget freeze (emphasis added)

Thus, it emerges that RMG is considering making New Haven his home and therefore that he had also received a job offer from Yale University. From other letters we have in the archive (see below) we know however that in the meantime that offer had already been withdrawn. The negotiation between Despres and RMG about the job at Williams College ends with a letter by Despres (II.a.51) of 14 March 1952 from which we learn that RMG has refused the offer. Despres, however, insists. First, asks RMG to ‘convey my thanks to Jackie for her effort in our behalf’, then adds

If next year or at any future time you think you might be receptive to an offer from Williams, please let me know. If we have a vacancy at the top level, we will require only a little encouragement to come forward with an offer

Fourthly, there is some correspondence regarding a job offer at the Department of Economics of the Carnegie Institute of Technology. In a letter dated 22 January 1952, George L. Bach (II.a.8), who shows he thinks very highly of RMG, after having explained that his Department together with the Department of Mathematics has started a good graduate curriculum in mathematical economics, writes:

As you have by now doubtless guessed, we are unable to get away from the fact that you have been doing some of the most fundamental and interesting work in the mathematical economics area. [...] [T]he purpose of this letter is to suggest that you ought to come and combine researching and professing here at Carnegie, which, so far as we can see, is beginning to do, and should expand in doing, some unique work in the broad field dearest to your heart (emphasis added)

We then learn that this offer had originally been made to RMG in November 1950, well before all the others:

The area of work, as it looks now, would be somewhat different from that *suggested a year and a half ago*, in that it would focus more heavily on mathematical economics at the graduate level. But *many of the general observations I made in my letter of November 22, 1950, which first tried to lure you here*, are still equally relevant (emphasis added)

We also read that, although this was not yet a formal offer, the senior staff of the Institute had suggested an arrangement whereby RMG would go to Carnegie as an Associate Professor, starting in autumn 1952, on a regular five-year appointment. Then, rather than waiting until the end of the appointment, they would consider a promotion to full professor after a couple of years, by which time RMG would have known whether he wanted to stay permanently or not. The letter also contains a reference to the starting salary, which would have been in the region of \$7000. The end of the story, reconstructed on the basis of the letters contained in the archive, is rather abrupt and unexpected with regard to this offer too. From the content of another letter by Bach (II.a.9) dated 3 June 1952, we learn only that RMG had not accepted the job at Carnegie and that Franco Modigliani had accepted it instead: ‘For our major mathematical economics job we have appointed Franco Modigliani which finishes up his big project on business expectations at Illinois this summer and will come here in September.’

Finally, we understand that RMG is again ready to give up certainty (a permanent job at Carnegie) for the uncertain (temporary position in Cambridge): ‘I hope, since you obviously would like to sit there another year or so more, that things work out so that satisfactory arrangements are forthcoming.’

Fifthly, from a letter dated 1 February 1952 by Professor of Economics at Yale University, John Perry Miller (II.a.203), we learn that RMG had also previously received a job offer from Yale.¹⁸ The present letter, however, is to inform RMG that the prospects in Yale no longer look good. The fact, we read in the letter, is that Fellner is going back there from California next autumn and

This seems to be all we are likely to do within the immediate future in the theory field. As you know we appointed Wallich last year in banking. I am sorry that this is the way it is because I think you would be happy in New Haven and would add a good deal to the Department¹⁹

¹⁸ The interest of Miller in having RMG at Yale is confirmed in a letter of 1 January 1952 by Leontief (II.a.170), where we read: ‘Miller longs to have you.’ In passing, Leontief also refers to the offer RMG had got from Stanford: ‘Stanford seems to be interested in you too – you will be able to find out the details from Ken Arrow, who just sailed for Europe and England.’

¹⁹ The letter by Leontief we have quoted in the previous footnote also contains a reference to this fact, but the conclusion is rather more optimistic: ‘Fellner going back to Yale does not preclude the possibility of you being asked to come here too.’ Haberler too seems to have known about RMG’s interest in Yale. In a letter of 21 March 1952 (II.a.102) he writes: ‘I note that you may be interested in a job at Yale. We are going to have Triffin here this afternoon and I shall ask him what the chances are. They have quite a good department now, but I am not sure they have completely assembled their staff. It is quite possible that there may be an opening sooner or later.’ Then, in another letter of 28 May 1952 (II.a.103), ‘I haven’t heard anything about Yale, but Fellner is going there in the fall and I think he would be very favourably disposed.’

Finally, there are three letters referring briefly to another job offer. A letter of 12 February 1952, written by Leontief (II.a.171), contains the details of a job offer made by Sven Laursen, Professor of Economics and Dean of Social Sciences at Brandeis University. In proposing it to RMG, Leontief explains that Laursen is very happy to have chosen to leave Williams for Brandeis, partly because this means living in Cambridge.²⁰ In a letter dated 2 June 1952, M.M. Davisson (II.a.43), Chairman of the Department of Economics of the University of California at Berkeley, invites RMG to spend academic year 1952-53 there:

Dear Professor Goodwin: We are hopeful that out of the 1952-53 budget, which should be available within the relatively near future, will have provision for a visiting professorship for the next academic year. In the event that such a position is available, my colleagues and I would be very pleased to have you spend the 1952-53 academic year in Berkeley if your plans would permit this

In a letter dated 2 February 1953, F.C. James (II.a.139), Principal and Vice-Chancellor of McGill University, Montreal, informs RMG that his university is interested in offering him one of two Senior Chairs they are advertising and that RMG's name was suggested to them by John Williams.

This short description suggests that, in spite of his 'failure' at Harvard, RMG could have easily obtained a full professorship in the US and that a number of his former colleagues at Harvard – in particular Despres, Leontief and Williams – as well as other outside appraisers (such as Bach, Arrow and Heflebower), were very active in helping him to find a job there. To the best of our knowledge, despite the fact that he scrupulously kept the letters dating back to those years, he never told anybody about the choices he had to make in this difficult period of his life. The only short reference he ever made to this story, as far as we know, was during the interview he gave to Maura Palazzi (1982, p. 46) when he said that, after the failure at Harvard, he had considered many other possibilities in American universities. Immediately after he explained that

It was the height of the McCarthy period and so there was witch-hunt against those who had been or were still members of the Communist Party. I was no longer a member, but this did not protect me from the committee because they forced us to appear before it demanding the names of the others who had been communists. At that point, the choice was between responding or the risk of being sent to prison. It was a very grave situation and the atmosphere was getting worse. In reality they had not yet summoned me to appear before McCarthy's committee so it is not

²⁰ Brandeis University is located in Waltham, Mass., just nine miles west of Boston.

true that I left America as a political refugee as was subsequently suggested. It was however a fact that those on the left – like me – were very vulnerable. It was very difficult to find employment and hang onto it: the situation was therefore very unpleasant. This is probably the real reason why I did not want to stay in America (translated from Italian by James L. Newell)

By contrast, on the basis of the experience he had had there in the years from 1934 to 1937, he considered Britain as a place ‘where one could be left and not left out’ (Letter to Velupillai, 5 April 1993, quoted in Velupillai, 1998b, p. 6) and Cambridge in particular, as underlined by Pasinetti in his tribute to RMG (1996, p. 647), as a place ‘where he could teach what he wanted.’

The conclusion we can draw is that RMG was aware of his value and confident that sooner or later the offer of a tenured position from a prestigious University would reach him. And this is what actually happened. In this respect, Cambridge was more innovative and open minded than Harvard.

4. ‘Theory of Economic Dynamics’: a book that was never completed

The ‘known story’ tells us that RMG, during the period covered by the letters we have discussed in the previous section, had started to write what Paul Samuelson, in the Foreword to Goodwin and Punzo (1987, p. ix), defined as ‘his eagerly waited treatise on nonlinear business cycles.’ We will try now to reconstruct the development of this project in the attempt to answer the following questions: i) why was the book never published and ii) why did RMG continue to work on the project without completing it? In doing so, we will also briefly describe the book itself. The letters in the archive greatly help us in following these events and the copy of the book in the archive enables us to describe its contents.

4.1.

The first reference we found in the archive to the fact that RMG was writing a book is in a letter Emanuel Parzen²¹ (II.a.216) sent to RMG on 19 August 1950 where he wrote: ‘will be looking forward with great interest and expectation to your forthcoming work on mathematical economics.’²² This implies that RMG was writing the book already in the early 1950s or even before that. Palazzi, in

²¹ Parzen had graduated in Mathematics at Harvard in 1949. When he wrote the letter to RMG he was reading Mathematics at the University of California at Berkley to get first an MA and then a PhD in Mathematics and Statistics.

²² This is confirmed in a second letter Panzer sent to RMG on 5 January 1951 (II.a.215).

her Bibliographical Appendix to the interview with RMG (Palazzi, 1982, p. 55), states that a typescript called *Mathematical Methods in Dynamic Economics*, although undated, has to be considered as having been written in 1949.²³ On the other hand Daniel Ellsberg (a student of RMG at Harvard) writes: ‘The work on economic dynamics was finally begun last fall [...] The book will be finished at Cambridge’ (Ellsberg, 1951). If this were correct it would mean that RMG had started to write the book well after Harvard had refused to give him tenure. In addition in the same article Ellsberg goes on by writing: ‘[...] he has an obligation to fulfil. Schumpeter, before his death, made Goodwin promise to *finish* a book before his five years as assistant professor were up.’ Schumpeter died in January 1950 and therefore we can argue that RMG had started to write the book in 1949 with the idea of completing it before the summer of 1951 when his contract with Harvard ended. What is certain is that at the end of 1950 RMG received a referee’s comment on the first five chapters he had sent to Wiley. This is clear from a letter sent to RMG by James Helming (II.a.128) – the Editor of Wiley & Sons – dated 26 December 1950 from which we learn that RMG submitted to Wiley an outline and five chapters of a book entitled *The Theory of Economic Dynamics*. Helming informed RMG that they had received a report from a first reader and enclosed it with his letter. Therefore it is plausible to suppose that RMG had already completed a draft of the book in 1949 (perhaps with the title suggested by Palazzi), but had sent the publisher a more refined version some time in 1950 (when Parzen was writing his first letter referring to the book as forthcoming). By the way the outline and the titles of the chapters are rather similar to the outline of the course given at Harvard by RMG in 1948-9 (Economics 204b).

From what Helming writes, it appears that he was much interested in RMG’s project but also that he agreed with the reader’s suggestion that the chapters could be re-written in order to enable the volume to serve as a textbook. Reading the comments written by the first reader we get the impression that he is a person that knows RMG well. For example, he writes: ‘Much of the book is Goodwin, which is commendable but I suspect does not make the best text.’²⁴ In a second letter dated 12 March 1951, Helming (II.a.129) informs RMG that he has not yet received the comments by the second reader but that he has spoken with him over the telephone and that he understands the reader is enthusiastic. Helming also adds that he has spoken again with the first reader. We learn that the latter has confirmed his first reaction, i.e., that in his opinion

²³ It should be noted that the index that appears in Palazzi (1982) differs from the one forming part of the copy of the book in the Archive in Siena. More precisely, in her index Palazzi also lists chapter 9 (see Table 1 below) – a chapter which is missing in the Siena copy – and describes it as a manuscript. In the same index we find an introductory chapter (‘Introduction: Remarks on Methodology’) and a chapter on ‘Durable Goods Markets and Second Order Difference Equations’, which are also both missing in the Siena copy.

²⁴ RMG, for his part, had no problems in understanding who the reader was and wrote his name – Boulding – by hand, on the report he had received.

the main problem is that RMG does not make clear from the beginning and at every stage of his treatment where he wants to go. This first reader has also called the attention of Helming to RMG's recent article in *Econometrica* and said that if RMG 'would revise that article as the opening chapter of (his) book, the crux of the problem might almost be resolved.' There are no doubts that the article to which the reader refers is RMG's article on the nonlinear accelerator, which was published in the first issue of the 1951 volume of *Econometrica*. Helming ends his letter by once again stressing Wiley's interest in RMG's book and says 'I am more anxious than ever to know whether you will publish your manuscript elsewhere or whether we will have a chance to work together.' However, he then adds, 'in the hope that John Wiley can see sufficient market to make an offer of contract.'

From the contents of a third letter dated 6 April 1951 (II.a.130), we understand that Helming has been to Harvard to discuss the book proposal with RMG. He says that he is enthusiastic about it and is convinced that it would make a commercially feasible venture. Moreover, from a passage of the letter we understand (1) that he has convinced RMG to write a sort of preface to the first five chapters of the manuscript and (2) that he has decided, maybe on a suggestion by RMG himself, to send the chapters and the preface also to an engineer. The implication of this is that the Prefatory Note to the First Five Chapters we have in the archive was written by RMG later than the chapters.

Finally, from another letter by Helming (II.a.131) dated 4 June 1951, we learn the end of the story, namely, that Wiley decided not to publish RMG's book. The main reason was that, on the basis of the reports they received from the two readers and the engineer, they were unable to 'convince [themselves] that [Goodwin's] book would have sufficient sale to warrant commercial publication.' Then Helming adds

This is a real disappointment to me, because as you know I believe your book should be published. I have no doubt that it will, even though you may have to go to one of the university presses. [...] Since the manuscript has now served its purpose, I am returning it to you under separate cover

From this we understand that the copy of the manuscript we have in the archive is the one that Helming mentions in his letter and says he is returning to RMG. From this final letter we also understand that Wiley had received the second reader's report mentioned in the letter of 12 March. We do not have in the archive a letter bringing it to RMG's attention, but we have the report, which is kept together with the manuscript rather than with the other letters.²⁵

²⁵ There are reasons to believe that this second reader was a former Harvard student, most probably Paul Samuelson.

However RMG in his last Harvard year tried in various ways to get it published as we shall see.

4.2.

We learn, from a series of other letters from Charles Pettee of Oxford University Press (New York), that RMG had also submitted the same five chapters to Oxford University Press. In a letter dated 23 February 1951, Pettee (II.a.226) writes: 'Here at long last is our first critical comment on your manuscript.' The expression 'at long last' seems to imply that they had received the manuscript at least some weeks, if not months, earlier and thus, probably, that the manuscript had been sent to both publishers at the same time. What is even more astonishing is to learn, from the reports attached to the letter, that the title of the manuscript is now slightly different: *An Introduction to Economic Dynamics*. From the contents of the first report we understand that the reader had also received and read the first chapter, a chapter that is missing in the Goodwin Archive, probably as Oxford University Press did not send the material back to RMG. Moreover, from the same letter we understand that RMG and Pettee were intending to meet in Harvard a week later. This is confirmed in a second letter dated 21 March 1951 (II.a.227), from which we understand that they had in fact met and had decided, before taking a decision on publication, to wait for another critical reading of the book. We also find also a reference to the fact that RMG was going to be in England the following year. In a final letter of 19 April 1951, Pettee (II.a.228) sent RMG the second reader's comments, which include the following revealing and important sentence: 'Professor Goodwin's book may be as much as a generation ahead of its time.' Moreover, regarding the saleability of the book, we find the following important comment:

The possible saleability of Professor Goodwin's book is limited by two factors, neither of which reflects on its merits. One is the fact Professor Baumol, of Princeton, is bringing out a similar book this year (published by Macmillan), which may pre-empt the somewhat limited field for works of this kind. Another is that Professor Goodwin himself has taken some of the edge off his own larger work by the chapter he wrote for Professor Hansen's Business Cycle and National Income, which anticipates some of the substantive conclusions of these chapters

The first reader expressed a negative opinion: 'In no sense could it be called a text [...] is far too advanced for [...] the average graduate student of economics.' And goes on 'The present manuscript strikes me as falling squarely between these two stools. It is not systematic or simple enough for a text in applied mathematics for economists: it is too diffuse for a technical monograph.' There

are no other letters in the archive regarding the correspondence between Oxford University Press and RMG. However, all seems to imply that, on the basis of the two reports, they decided not to offer a contract to RMG.

4.3.

Thanks to other letters we have in the archive, we can infer that the fact that RMG was writing a book was in a sense ‘common knowledge’. These are letters from other publishers expressing interest in publishing the book RMG is writing.

In a letter dated 23 April 1951, Charles Calhoun (II.a.29), Vice-President of the Ronald Press Company, informed RMG that he was still waiting for the outline and sample chapters. He stresses that they ‘should like very much to explore with you the publishing possibilities for your material.’ Moreover, in a letter dated 17 April 1951, George P. Brockway (II.a.23), Director of Norton & Company, Inc., too let RMG know that they were interested in hearing more about the book he was writing and added: ‘I should be even more interested if you could be persuaded to turn your hand to an introduction to Econometrics.’ What he meant by this was that RMG should try to expand the article published in Hansen’s book so that the material would be more readily used in teaching advanced undergraduates. From another letter by Brockway of 19 June 1951 (II.a.24) we understand that RMG had refused to transform his book into a book on Econometrics and that he had suggested Jacob Marschak and Gerhard Tintner as possible authors instead. Finally, there is even a letter dated 16 February 1955 in which Alexander J. Morin (II.a.205), editor of the University of Chicago Press, let RMG know that they were interested in publishing his book on economic dynamics.

4.4

In spite of RMG’s efforts and the wide appreciation of his work the book was never published. There are several reasons for this. The main criticism was that the book was not suitable as a textbook as RMG did not endeavour to present with careful attention the new and advanced mathematical tools he was discussing. The material was really at the frontier of economic research and would have had a restricted market. Without this pedagogical presentation required by the referees at Wiley and OUP it was decided that the book had no market. And indeed the book by Baumol (1951) as perceptively noticed by one referee was a real textbook on topics not too dissimilar from those included in RMG’s book: and the book had been a success for many years. This is sufficient to explain why the book was not published.

And yet many thought that RMG was an excellent teacher. For example Daniel Ellsberg wrote in the *Harvard Crimson* (24.05.1951): ‘Lecturing in undergraduate and graduate courses he began to acquire an outstanding

reputation as a teacher. As several students commented Goodwin is one of the rare lecturers who can make Emerson D seem like a seminar room.' And again, in a short piece that appeared in the *Harvard Crimson* (5 May 1951), we read:

Richard Goodwin, assistant professor of Economics, has accepted a post at Cambridge University for next year and at least 30 of his students think Harvard missed the beat by not reappointing him. [...] He's the best teacher I've ever had, [Nicholas A. Beadles 8G said yesterday], his going will be a great loss to the University

One can finally add some comments included in a long letter by John P. Shelton of 19 March, 1952 (II.a.280):

The first thing I want to thank you for is a little diagram you once sketched on the blackboard in Econ 204a, showing the essential features of Keynes' General Theory. I consider it the neatest summation of a complicated and lengthy piece of analysis that I have ever seen. It was a masterly piece of pedagogy. I learned more about Keynes' General Theory in a few minutes from that than I did even from reading his book. The value of this sketch was greatest to me during my orals last spring. [...] No matter how involved or tricky their questions I found I was able to keep all the factors in mind simply by visualizing your diagram, so my grade (Excellent minus) came out much higher than I could have possibly deserved except for the lucky circumstance that they happened to get into an area of discussion where your teaching carried me through. I will never forget what a debt I owe to you for teaching me that as well as a great deal more that I learned in economics, from Marshall's quasi rents to econometric models of the business cycle

And then he goes on by writing:

My biggest success [...] has occurred as a result of my giving a seminar [...] about your business cycle model. I went over chapter 22 in Hansen and my notes from 204 and 245 very carefully and managed to present the central ideas without being asked any questions which I couldn't answer

Finally, he praises RMG's model with respect to the one advanced by Hicks (1950):

Your model may contain as many stimulating and constructive ideas as Hicks' but you did it in a half chapter and he took a book

[...] But how can one review your model? Any worthwhile review and critique would probably have to be longer than the presentation in Hansen's book was. And wouldn't that be embarrassing to write a review longer than the original? Thus you fail to get the attention and credit your ideas deserve.

Solow himself – who was a student of RMG in 1948-9 – wrote: 'Dick more than anyone else taught me about economic modelling as a research strategy and he taught me a particular approach to model building, the role of simple and pointed and sharp models.'

Yet appraisal by such exceptional students as Ellsberg and Solow induces us to reflect. Maybe RMG was an excellent teacher for excellent students who were really able to grasp the importance of a 'field whose worth was yet to be demonstrated' as Ellsberg himself writes in the same article. And therefore the comment by one of the referees, namely that 'the book may be as much as a generation ahead of its time' was basically correct. And yet the letter by Shelton expresses also the great force of RMG's approach, namely, the capacity to stimulate students to understand and to push them to deepen their knowledge and follow the new paths that start from there.

4.5.

To complete the picture, it should be noticed that rejection of the book by the publisher had not put off RMG from working on the book for months.

In a letter dated 21 March 1952, Haberler writes 'I am very glad to learn that you are making progress with your book.' Moreover, he adds, 'Robertson is of course extremely intelligent and very good in finding errors even in mathematical matters which he pretends is way above his head.'

The reference to Robertson is important. In the archive we have some letters from Robertson to RMG, written between July 1951 and June 1952 in which they discuss previous articles published by RMG, for example, the article on the multiplier published by RMG in 1947. The implication, we think, is that RMG, in order to write the chapters of the book, was starting from, and revising, articles he had already published (for example, Goodwin, 1947a, 1947c, 1951a, 1951b) and was making the revisions through discussions with Robertson.

4.6.

Thus, the question arises, why did RMG not finish his book? If we look at the index of the copy of the book available in the Goodwin Archive (see Table 1) we notice that RMG was serious in his determination to complete the first part and to add a second part called *General Interdependence Systems* which was going to include the works he had just completed or was completing in the 1950-1 period. It is apparent that chapter XII would have included material

from Goodwin (1947a), chapter XIII from Goodwin (1949, 1950b) and finally chapter XIV from Goodwin (1953). The latter work derives from the paper given by RMG at the Driebergen conference in September 1950.²⁶

The second part is not in the index reported by Palazzi and this seems to confirm the hypothesis that the 1949 book was a preliminary version. However, even with the addition of the second part, we think that the book would have maintained the structure of a collection of essays rather than that of an advanced textbook. The trouble was that the publisher wanted instead a well structured work that could be used as a textbook and this would have required a lot of time that RMG probably did not want to spend on such a task. Actually RMG was not too interested in writing books as Ellsberg stressed (*Harvard Crimson* 24 May 1951) in reporting a comment by RMG: ‘I did not want to write a book that would not be original [...]. When I have something new I write a paper.’ Another reason was that, as time elapsed, the promise RMG had made to Schumpeter appeared less cogent and the negative responses from the publishers did not encourage him. The result was that he continued to produce excellent papers like those of the 1950-2 period.

Table 1: Index of «The Theory of Economic Dynamics» (copy kept in the Goodwin Archive)

Book I	Micro and Macrodynamics
I	Introduction: Remarks on Methodology (missing)
II	Power Series, in Particular Linear Approximations
III	Simple Macrodynamics of Firms, Inventories and the Generation of Income
IV	The Elementary Dynamics of the Firm
V	The Elementary Dynamics of Markets
VI	The Pure Theory of Durable Goods (missing)
VII	Cobweb and Multiplier: First Order Difference Equations
VIII	Capital Theory with Lags: Second Order Difference Equations (missing)
IX	Advanced Methods in Economic Dynamics (missing)
Book II	General Interdependence Systems (missing)
XII	Role and Importance of Dynamical Interdependence: The Coupling of two Markets
XIII	Generalized National Income Analysis: The Multiplier as Matrix
XIV	Static and Dynamic Linear General Equilibrium Theory

²⁶ The first reader in his report wrote: ‘This judgment is based on a reading of the first five chapters only and might be revised considerably if the rest of the MSS were available; the outline sounds most intriguing.’

4.7.

Let us give a short overview of RMG's intentions in writing the book. In the introductory note he wrote:

The essential idea around which this book is built is that it is possible and desirable to combine the exposition of economic theory and mathematical methods. In order to understand properly very important aspects of his subject the modern economist needs a knowledge of some rather advanced mathematical notions. [...] Therefore I have tried to grease his path by taking a number of topics familiar to the economist and use them as clothes horses upon which to display a number of pretty mathematical methods [...]. The familiarity of the subject along with the unrigorous and unforbidding presentation should be a real help and encouragement to the economists.

Thus, the purpose was that of presenting mathematical methods rather than a discussion of economic mechanisms at the root of the dynamic evolution of the economic system – although he hastened to add:

[...] in this way I can slip a number of bitter but essential pills down the unsuspecting reader's throat. His interest and will to get through should be sustained by the subject matter about which he will get important new insights

It is in this ambivalence that both the strength and the weakness of RMG's approach resides. The weakness, as noted by the referees, is that the book does not contain a clear and pedagogically accurate discussion of the mathematical methods useful to economists; the strength is that this approach induces the reader to employ all his intellectual capacities to penetrate the argument and appreciate the analysis RMG presents: simple models that illuminate aspects of reality, as he himself wrote in a short essay recently discovered and published (see Di Matteo, Filippi & Sordi, 2006). Therefore it is reasonable to guess that the book would have had a very restricted market. And yet, if RMG had persevered and published the book (maybe with a university press) it would certainly have represented a truly innovative volume in the theoretical literature of the period.

4.8.

A brief outline of the book is now given. The first part of the book, after a brief chapter on Linear Approximations, includes a chapter (III in the index) where the basic theory of income determination is discussed 'in the guise of the

dynamical multiplier operating through stocks of finished goods' as a way of introducing first order differential equations. After that

Having investigated the generation of income, given the injections, we proceed to crude macrodynamic systems in which the injections (investment) are partially explained as well. Finally there is a cursory analysis of the Duesenberry-Modigliani hereditary consumption function as a part of a macrodynamic system.

The analysis included in this chapter is however far from being mechanical as in later developments of Keynesian theory and includes a detailed discussion of the case of full employment given alternative patterns of money supply and greatly enlarges the previous treatment of similar topics contained in Goodwin (1947c). The discussion of the mathematical instrument is fairly general and does not exclude the nonlinear case.

The following chapter (IV in the Index) refines the analysis by considering the lag in the production process along the lines described by Robertson and Frisch. In RMG's words:

The resulting picture of the typical firm is used to develop a simple macrodynamic theory of the economy and serves as an excuse for treating the second order linear differential equations. In the course of this a precise definition of stability, stability according to Liapunov, is discussed.

It is to be noted that part of the treatment is based on the recently published book by Andronov & Chaikin (1949).

There are two more chapters that have survived. The first (V in the Index) opens with an analysis of the dynamic stability of markets. Then he takes up the theories of market 'adaptation' associated with Marshall and Walras:

Marshall's theory runs in terms of the difference between demand and offer and involves us, if we make his system explicitly dynamical, in working out the elements of a theory of speculative stocks of goods. The Keynesian liquidity preference theory is investigated as an example. Finally the two theories of market behavior [ie Marshall and Walras] are combined into one theory which yields different results from either one separately.

In this part he analyses separately the short period normal price and the long period normal price before tackling the question of speculation and its effects.

The latter chapter (VI in the Index) is meant to introduce difference equations that appear to be especially suitable for dealing with lags. However as RMG aptly observes there are two types of lags:

In the first case we may have a market which meets once a month and at which all transactions are carried out, with none taking place in between. [...] This type of problem involves no serious difficulty and we shall use the multiplier as an example of it. The second type of problem is presented by a market in which decisions are taken continuously but which entail a considerable lag between the decision and the economic results of the decision [...] In the second part of the chapter we shall be dealing with this second and more difficult case.

The chapter ends with the stability analysis of nonlinear first order difference equations.

We would like to end the paper with two general comments leaving a more detailed analysis of the book to a future critical edition.

A basic idea of the book is that the introduction of dynamics in an otherwise static theory helps us to understand both that the results derived from it are partial if not misleading, and how special the static theory is. As an example take the case of the multiplier RMG discusses in chapter III:

We [have] to consider the realistic case of continuously varying injections [...] The system is always moving towards its instantaneous equilibrium (multiplier) value [...] But if the goal is constantly changing it may nonetheless be quite far from it, how far depending on both its rapidity of adjustment and the magnitude of the required adjustments. Clearly no multiplier is ever observable and we are inescapably involved in a thoroughgoing dynamic analysis.

We then get to a formulation that considers

the level of income at any time equal to the simple sum of the present results of the response to all past injections. Each has approached more or less closely to final multiplier value depending on the time elapsed since its occurrence. There exists a time t before which the effect of the injection has most completely exhausted its effects. Thus we can say that the system keeps a memory of what has happened in the past, up to a point.

RMG goes on:

The consequence of this memory (for stable systems only) is that we do not have to take account of all previous injections but only those back to a time [...] This also does away with the necessity of taking account of any initial condition [...] Thus it is not necessary to carry our summation over past injections back more than a certain period, θ . [...] Therefore in practice we may write²⁷ [...]:

$$\bar{y}(t) = -\frac{1}{1-\alpha} \sum_{\tau=t-\theta}^t \Delta I(\tau) \bar{u}(t-\tau)$$

This says that income at any time is entirely the result of the injections over a period θ immediately past. We may call the mechanism a backward looking multiplier, if we remember that income is *not* a multiple of any single present or past injection. [...] Only in the highly special case of a single ΔI maintained from now $(t - \theta)$ until t we will get $y = \Delta I/(1 - \alpha)$.

The quotation shows how this interpretation has been neglected. Recently and in a different context, an analogous interpretation of the solution of a differential equation was put forward in economic dynamics (see, for example, Azariadis, 1993, ch. 2).

The second comment is that the book includes not only analyses that had been or were about to be published, but also approaches that remain confined to the book, such as the case of speculation we have described above. In particular RMG shows very simply (chapter V in the Index) how speculation can entail a stable movement if the deviation from equilibrium is small but also an unstable one if the deviation is large. To get this result one has to postulate that the coefficient reaction is not a constant but a function of the price level of the good or the activity. And the possibility of multiple equilibria is further analysed when RMG discusses the possibility of generalising the concept of stability to the case of a trajectory in the context of a nonlinear differential equation. With the aid of a very simple diagram RMG shows the possibility of *hard oscillation* and *soft oscillation*.

5. Conclusions

In the paper we have discussed three set of questions concerning a crucial period in RMG's life: i) why did he not get tenure in Harvard?; ii) how did he

²⁷ Here α is the marginal propensity to consume, I the injections, y income and u is the weighting function (it determines the weights to be attached to each past injection).

choose to accept a position in Cambridge, UK.?.; iii) how was it that he did not finish his book on economic dynamics?

Our main conclusions are as follows:

i) Although RMG was at the end of 1949 already a talented and innovative researcher, Harvard's Department of Economics was too conservative in his recruiting policy to offer him tenure. He had a number of papers already published or being published, in top journals, but possibly he was ahead of his time, as we can surmise from published as well as archival material.

ii) RMG's excellence in research and teaching was recognised however, as confirmed by the behaviour of departments of other prestigious American and British Universities which were keen on recruiting RMG: this emerges clearly from our analysis based on the documents we have consulted in the Goodwin Archive. This made him aware of his excellence, inducing him to turn down several offers before finally accepting a lectureship in Cambridge, UK.

iii) The book on economic dynamics was not accepted for publication by several publishers on the basis that it did not meet the standards of a textbook as conceived at that time. This is made clear by analysing the letters and referees' reports held in the archive. Although he did work on the book for some time even in Cambridge, UK, in the end he gave up because he was born to write papers encapsulating simple, illuminating models. The material in the archive enables us to summarise the actual contents of the book and to envisage what the result might have been in its planned final form.

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