Money and Growth

Selected papers of Allyn Abbott Young

Edited by
Perry G. Mehrling
and Roger J. Sandilands

Routledge Studies in the History of Economics

ROUTLEDGE

Money and Growth

Allyn Young is one of the central figures in the development of American economic thought, and is one of the originators of modern endogenous growth theory. However, it has been difficult to appreciate the full extent of Young's work because many of his most significant contributions are buried in obscure journals and unsigned articles.

This volume addresses this by reprinting much of Young's lost work, as well as other selected pieces that reveal the scope of his vision which encompasses two of the grand themes of economics, growth and money. The biggest "finds" are 36 encyclopedia articles, originally published anonymously more than 75 years ago. They are important because they shed light on the unity of Young's thought which is based on the single abstract idea that trade is the source of wealth.

The volume includes sections on:

- the socialist movement
- the First World War and its aftermath
- money
- theories of growth

The volume concludes with some writings that are pointers to the nature of the two treatises Young was planning before his untimely death in 1929. A comprehensive bibliography is also provided.

Perry G.Mehrling is associate Professor of Economics at Barnard College, Columbia University. His research mainly involves the intellectual and theoretical foundations of monetary theory. His most recent book is *The Money Interest and the Public Interest: American Monetary Thought 1920–1970*.

Roger J.Sandilands is Senior Lecturer at the University of Strathclyde, Glasgow, Scotland. For many years he worked closely with Allyn Young's student Lauchlin Currie, and is the author of *The Life and Political Economy of Lauchlin Currie: New Dealer, Presidential Adviser, and Development Economist*. He also edited Nicholas Kaldors "Notes on Allyn Young's LSE Lectures, 1927–29" which appeared in the *Journal of Economic Studies* in 1990.

Routledge Studies in the History of Economics

1 **Economics as Literature**
Willie Henderson

2 **Socialism and Marginalism in Economics 1870–1930**
Edited by Ian Steedman

3 **Hayek's Political Economy**
The socio-economics of order
Steve Fleetwood

4 **On the Origins of Classical Economics**
Distribution and value from William Petty to Adam Smith
Tony Aspromourgos

5 **The Economics of Joan Robinson**
Edited by Maria Cristina Marcuzzo, Luigi Pasinetti and Alexandro Roncaglia

6 **The Evolutionist Economics of Léon Walras**
Albert Jolink

7 **Keynes and the 'Classics'**
A study in language, epistemology and mistaken identities
Michel Verdon

8 **The History of Game Theory, Vol 1**
From the beginnings to 1945
Robert W.Dimand and Mary Ann Dimand

9 **The Economics of W.S.Jevons**
Sandra Peart

10 **Gandhi's Economic Thought**
Ajit K.Dasgupta

11 **Equilibrium and Economic Theory**
Edited by Giovanni Caravale

12 **Austrian Economics in Debate**
Edited by Willem Keizer, Bert Tieben and Rudy van Zijp

13 **Ancient Economic Thought**
Edited by B.B.Price

14 **The Political Economy of Social Credit and Guild Socialism**
Frances Hutchinson and Brian Burkitt

15 **Economic Careers**
Economics and economists in Britain 1930–1970
Keith Tribe

16 **Understanding 'Classical' Economics**
Studies in the long-period theory
Heinz Kurz and Neri Salvadori

17 **History of Environmental Economic Thought**
E.Kula

18 **Economic Thought in Communist and Post-Communist Europe**
Edited by Hans Jurgen Wagener

19 **Studies in the History of French Political Economy**
From Bodin to Walras
Edited by Gilbert Faccarello

20 **The Economics of John Rae**
Edited by O.F.Hamouda, C.Lee and D.Mair

21 **Keynes and the Neoclassical Synthesis**
Einsteinian versus Newtonian macroeconomics
Teodoro Dario Togati

22 **Historical Perspectives on Macroeconomics**
Sixty years after the 'General Theory'
Edited by Philippe Fontaine and Albert Jolink

23 **The Founding of Institutional Economics**
The leisure class and sovereignty
Edited by Warren J.Samuels

24 **Tradition and Innovation in Austrian Economics**
A Mengerian perspective
Sandye Gloria

25 **Marx's Concept of Money: the God of Commodities**
Anitra Nelson

26 **The Economics of James Steuart**
Edited by Ramón Torajada

27 **The Development of Economics in Europe since 1945**
Edited by A.W.Bob Coats

28 **The Canon in the History of Economics**
Edited by Michalis Psalidopoulos

29 **Money and Growth**
Selected papers of Allyn Abbott Young
Edited by Perry G.Mehrling and Roger J. Sandilands

30 **The Social Economics of Jean-Baptiste Say**
Markets & virtue
Evelyn L.Forget

31 **The Foundations of Laissez-faire**
The economics of Pierre de Boisguilbert
Gilbert Faccarello

Money and Growth

Selected papers of Allyn Abbott Young

Edited by Perry G.Mehrling
and Roger J.Sandilands

London and New York

First published 1999
by Routledge
11 New Fetter Lane, London EC4P 4EE

Simultaneously published in the USA and Canada
by Routledge
29 West 35th Street, New York, NY 10001

Routledge is an imprint of the Taylor & Francis Group

This edition published in the Taylor & Francis e-Library, 2002.

British Library Cataloguing in Publication Data
A catalogue record for this book is available from the British Library

Library of Congress Cataloging in Publication Data
Young, Allyn Abbott, 1876–1929
Money and growth: selected papers of Allyn Abbott Young/edited by Perry G.Mehrling and Roger J.Sandilands.
p. cm.
Includes bibliographical references and index.
ISBN 0-415-19155-6 (alk. paper)
1. Young, Allyn Abbott, 1876–1929. 2. Economics. 3. Economists—United States. 4. Economic history—20th century. 5. Endogenous growth (Economics) 6. Economic development. 7. Socialism. 8. Money. 9. World War, 1914–1918—Economic aspects. I. Mehrling, Perry. II. Sandilands, Roger J. (Roger James), 1945–. III. Title. IV. Title: Selected papers of Allyn Abbott Young
HB119.Y68A25 1999 98–41828
330–dc21 CIP

ISBN 0-203-45282-8 Master e-book ISBN

ISBN 0-203-76106-5 (Adobe eReader Format)
ISBN 0-415-19155-6 (Print Edition)

Contents

Editors' preface xi

Acknowledgements xxvii

PART I

The nature and scope of economics 1

1 Economics as a field of research 3

Quarterly Journal of Economics (1927) 42, 1 (November): 1–25

2 English political economy 17

Economica (1928) 8, No. 22 (March): 1–15

3 National statistics in war and peace 29

American Statistical Association (1918) 16, No. 121 (March): 873–85

4 Economics and war 38

American Economic Review (1926) 16, 1 (March): 1–13

5 Increasing returns and economic progress 49

Economic Journal (1928) 38, No. 152 (December): 527–42

PART II

Theory and practice 63

6 The social dividend 65

Richard T.Ely, Thomas S.Adams, Max O.Lorenze and Allyn A.Young. *Outlines in Economics* 2nd edn. (1908), New York: Macmillan. Appendix to chapter XXV, "Profits": 448–56

7 Socialism 74

Lecture notes, Washington University, St Louis, April 1912

8 **Pigou's wealth and welfare** 86
Quarterly Journal of Economics (1913) 27, 4 (August): 672–86

9 **Public borrowing for road building** 97
Cornell Civil Engineer (1915) 23, Nos 6–7 (March/April): 301–15

10 **The economics of farm relief** 110
The Independent (1926) New York: 117, No. 3972 (July 17): 64–6

11 **Economics** 115
Encyclopaedia Britannica (1928) London: The Encyclopaedia Britannica Company. 925–32

12 **Capital** 135
Encyclopaedia Britannica (1928) London: The Encyclopaedia Britannica Company. 793–7

13 **Supply and demand** 143
Encyclopaedia Britannica (1928) London: The Encyclopaedia Britannica Company. 579–80

PART III
Commerce: The marketplace of the world 147

14 **The creator of wealth** 149
The Book of Popular Science (1924; revised 1929) New York: The Grolier Society. Group IX Ch. 1:110–6

15 **The rise of population in great countries** 153
(*Grolier* Ch. 2:254–5)

16 **The three great powers** 155
(*Grolier* Ch. 3:397–403)

17 **America's natural endowment** 161
(*Grolier* Ch. 5:677–89)

18 **Our wealth in minerals** 174
(*Grolier* Ch. 6:817–27)

19 **Our water and forest wealth** 177
(*Grolier* Ch. 7:951–60)

20 **Our wealth in cereals** **181**
(*Grolier* Ch. 8:1101–12)

21 **The economic interdependence of nations** **189**
(*Grolier* Ch. 9:1243–52)

22 **The reign of king cotton** **196**
(*Grolier* Ch. 11:1529–36)

23 **Trade and the railroad** **200**
(*Grolier* Ch. 19:2529–37)

24 **Wealth and well-being** **203**
(*Grolier* Ch. 20:2665–74)

25 **The annual wealth product** **208**
(*Grolier* Ch. 21:2793–800)

26 **Our foreign trade** **217**
(*Grolier* Ch. 23:3117–25)

27 **The making of wealth** **220**
(*Grolier* Ch. 25:3377–85)

28 **The sources of wealth** **231**
(*Grolier* Ch. 26:3505–13)

29 **Labor and wealth** **239**
(*Grolier* Ch. 27:3627–36)

30 **Combination and monopoly** **244**
(*Grolier* Ch. 29:3971–80)

31 **The meaning of value** **252**
(*Grolier* Ch. 30:4097–105)

PART IV
Money and credit **261**

32 **Exams in money and banking** **263**
Harvard University, 1922

33 **The mystery of money** **265**
(*Grolier* Ch. 31:4231–40)

34 **Monetary system of the U.S.** **277**
(*Grolier* Ch. 32:4291–4303)

35 **Mobilizing banking credits** **293**
(*Grolier* Ch. 33:4437–46)

36 **Dear and cheap money** **307**
(*Grolier* Ch. 34:4705–15)

37 **Insurance and speculation** **322**
(*Grolier* Ch. 35:4861–71)

38 **Money and prices** **337**
(*Grolier* Ch. 36:5109–20)

39 **An analysis of bank statistics for the United States** **352**
An Analysis of Bank Statistics for the United States (1928) Cambridge: Harvard University Press. Extracts from Chs I and II, pp. 1–32

40 **Branch banking in the United States** **362**
"Introduction" to Jean Steels, *La Banque à Succursales dans le Système Bancaire des États-Units* (1926) Ghent: A.Buyens, vii–xx

41 **Downward price trend probable, due to hoarding of gold by central banks** **369**
The Annalist (1929) New York. Vol. 33:96–7. January 18

42 **The French franc** **374**
"Introduction" to Eleanor Lansing Dulles, *The French Franc, 1914–1928* (1929) New York: Macmillan. Pp. xi–xvi

PART V

Growth and fluctuations **379**

43 **Industrial fluctuations** **381**
"Nicholas Kaldor's Notes on Allyn Young's LSE Lectures, 1927–29, "*Journal of Economic Studies* (1990) 17, 3/4:76–85

44 Particular expenses and supply curves **391**

"Nicholas Kaldor's Notes on Allyn Young's LSE Lectures, 1927–29" *Journal of Economic Studies* (1990) 17, 3/4:41–8

45 Economic changes since the war **399**

(*Grolier* Ch. 37:5239–48)

46 Big business: how the economic system grows and evolves like a living organism **411**

(*Grolier* Ch. 38:5387–94)

47 Bibliography of Allyn Young's writings **421**

Index **426**

Editors' preface

In recent years, in the specialist literature on the history of economic thought, a picture has been emerging of Allyn Young as a central figure in the development of American economics (Blitch 1995). His fingerprints are everywhere: co-author of the best-selling textbook *Outlines of Economics* (Ely et al. 1908, 1916, 1923, 1930) and of two others besides (Riley 1924, Reed 1925); patient builder of professional infrastructure as head of the Stanford economics department (1906–1910) and Secretary of the fledgling American Economics Association (1914–1920); devoted public servant, most notably as chief economist and statistician for the American Commission to Negotiate the Peace at Paris; inspiring teacher of a generation of economists, most notably Frank Knight and Edward Chamberlin but including also Holbrook Working, Lauchlin Currie, James Angell, Arthur Marget, and Nicholas Kaldor.

Modern interest in Young stems less, however, from his contribution to the economics profession, than from his contribution to economic thought. Young's 1928 presidential address to Section F of the British Association, titled "Increasing Returns and Economic Progress" has never lacked fans (Kaldor 1972, 1985; Currie 1981; Sandilands 1990), and the recent flurry of interest in theories of endogenous growth (Romer 1989; Currie 1997; Aghion and Howitt 1998) has brought Young's thinking on growth to the attention of a much broader audience. By contrast, Young's work on money and banking, the area of his special expertise, was largely forgotten. For example, Friedman and Schwartz in their comprehensive *Monetary History of the United States* (1963) make no mention of Young, not even of his book *An Analysis of Bank Statistics for the United States* (1928b). Only recently, with the work of Laidler (1993, 1998) and Mehrling (1996, 1997), has Young begun to come into focus as a figure of fundamental importance in the field of monetary economics, and this side of Young, it is fair to say, is still much less well known than the side concerned with economic growth.

Growth and Money, then, are the two themes of Young's work and of this volume, the main aim of which is to make more widely available a number of key texts that, until now, were accessible only to specialists, and often not even to them. Even in the specialist literature, appreciation of Young has suffered because an unusual fraction of his output has been either unknown, or inaccessible. The bibliography at the end of the volume addresses the first problem, and the major bulk of the volume addresses the second. In this regard, the biggest "finds" are the 36 chapters originally published anonymously in the Grolier Society's *Book of Popular Science* (Young 1924a), and especially the final six chapters on money and banking which constitute a small book by themselves, the longest sustained effort by Young on the subject of his special expertise, and a book previously unknown. (Two new chapters, Chapters 37 and 38, were added later for a revised 1929 edition.) In addition, the 1912 Lectures on Socialism and the 1922 exam questions on money are published here for the first time. A number of the other papers practically qualify as "finds" on account of the obscurity of their publication—the *Cornell Civil Engineer, The [New York] Independent,* and *The New York Times Annalist* are all defunct and difficult to locate.

Mere obscurity was however neither sufficient—we have not reprinted Young's early work for the Bureau of the Census, or his 1923 commencement address at Hiram College—nor necessary for inclusion. Papers can be known and accessible but not adequately appreciated until they are viewed in the context of the author's work as a whole. Scholarly appreciation of Young has, in our view, suffered not only from lack of access but also from lack of context, and we were concerned to present a rounded view of his work. Mindful of space considerations and the budgetary constraints of interested scholars, we have sought to produce a volume for the most part complementary to the collection *Economic Problems New and Old* (1927b)—only the important paper "Economics and War" appears in both. For similar reasons, we have reproduced only a limited selection of Young's LSE lectures and his *Encyclopaedia Britannica* entries, the full text of which is published in a special issue of the *Journal of Economic Studies* (1990; Young 1929c). Nevertheless, subject to those constraints, we have tried to produce a volume that stands on its own as a kind of Young reader that, we hope, will inspire scholars to consult the other collections, as well as the various worthy papers that remain uncollected but fairly readily accessible in back issues of major economics journals.

Growth and Money are the two grand themes of Young's work, but a rounded view of him, a view made possible by the material published in this volume, reveals them to be two sides of a single theme: Trade is the source of wealth. At the highest level of abstraction, growth is nothing more than a cumulative process of trade expansion, a process driven by the progressive cheapening of goods as the ever-widening market opens the way for increasingly productive methods of business organization. From this point of view, the institutions of money and credit are no mere veil hiding from view a world of purportedly "real" transactions, but are instead the key infrastructures that make markets work and enable them to expand. Young never went so far as to say that superior monetary institutions cause growth, but he was quite sure that inferior monetary institutions stymied growth, and for that reason he was intensely interested in monetary reform. Much like Joseph Schumpeter, whose *Theory of Economic Development* (1912) painted a picture of economic development made possible by the allocation of bank credit to business entrepreneurs, Allyn Young saw growth and money as inextricably intertwined.

Life

Allyn Abbott Young was born into a middle-class family in Kenton, Ohio on September 19, 1876 and died aged 52 in London on March 7, 1929, his life cut short by pneumonia during an influenza epidemic. He was then at the height of his intellectual powers and current president of Section F of the British Association. Uniquely, Young had also been president of the American Statistical Association (1917) and the American Economic Association (1925).

As documented in a recent biography (Blitch 1995), Young was a brilliant student, graduating from Hiram College in 1894 at the age of seventeen, the youngest graduate on record. After a few years in the printing trade he enrolled in 1898 in the graduate school of the University of Wisconsin where he studied economics under Richard T.Ely and William A.Scott, history under Charles H. Haskins and Frederick Jackson Turner, and statistics under Edward D.Jones. In 1900 he was engaged for a year as an assistant in the United States Bureau of the Census in Washington DC where he established lifelong friendships with Walter F.Willcox, Wesley C.Mitchell and Thomas S.Adams.

He returned to Wisconsin as Instructor in Economics for the 1901–02 academic session. After graduating in 1902 with a doctoral dissertation on age statistics, he embarked on what Blitch has called a peripatetic academic career, beginning with posts at Western Reserve University, 1902–04; Dartmouth, 1904–05; and Wisconsin, 1905–06. He then assumed the position of head of the economics department at Stanford, 1906–10, followed by a year at Harvard as visitor, 1910–11, and two years at Washington University, St Louis, 1911–13 (with the 1912 summer term at the University of Chicago). From 1913–20 he was professor at Cornell, but war took him to Washington DC in 1917 to direct the Bureau of Statistical Research for the War Trade Board, and to New York in 1918 to head the economics division of a group known as "The Enquiry" under Colonel Edward M.House, which group was charged with laying the groundwork for the Paris peace conference.

After the war, Young moved to Harvard in 1920 where he stayed until 1927 when he accepted William Beveridge's offer of the chair vacated by Edwin Cannan at the London School of Economics. He intended remaining at the LSE for three years before returning to Harvard. In December 1928 he traveled to the University of Chicago to explain in person why he felt unable to accept their invitation to be chairman of their economics department. It was shortly after his return to London that he succumbed to the fateful influenza epidemic.

At the time of his death Young was reputed to have been preparing a treatise on money (Morgenstern 1929; Taussig *et al* 1929); and T.E.Gregory (1929), a colleague at the LSE, wrote that he had in the last few months "begun work on a systematic treatise on economic theory and had resumed the writing of the work upon monetary theory which he had begun at Harvard." He continued:

> A passion for thoroughness would drive him on to explore every inch of the field in which he was for the time interested: he was always convinced that economic truth was not the monopoly of a single school or way of thinking, and that the first duty

> of a teacher and thinker was to see the strong points in every presentation of a point of view. Such an attitude of mind, combined with great personal modesty, made for unsystematic writing: for scattered papers and articles and not for a comprehensive treatise. In many respects he resembled Edgeworth, for whose work he felt a growing admiration; and if Young's work is ever collected, it will be seen that, like Edgeworth's, it amounts in sum to a very considerable and impressive achievement.

The nature and scope of economics

The papers reprinted in section I provide a general introduction to Young's thinking, and a framework for understanding the more focused papers that follow. "Economics as a field of research" (1927f) and "English political economy" s(1928c) represent Young's mature reflections on the nature of the field and on his own place in it (compare 1925d). Among very many points of interest, it is worth emphasizing two. First, although as a student of Ely's "look and see" method Young was definitely an empiricist at heart, he had a much greater appreciation for the role of theory than did Ely. As he put it: "In any really creative research, however modest in scale, there is this process of continuous give and take between the search for general relations and the scrutiny of particular details, between thinking and concrete observation" (1927f:13). Second, although as a professional economist Young worked constantly to improve his narrowly economic expertise, he never lost sight of the fact that economic relations are only part of the larger social picture, and that economics is only one of the social sciences. As he put it: "The final terms of every chain of economic inferences reach out into other systems of relations, often non-economic in character, and it is from these other relations that the final terms get their meaning and significance" (1928c:11).

"National statistics in war and peace" and "Economics and war" both argue, from very different standpoints and for very different audiences, that war and its aftermath were the determining events for the development of economics at the time. Significantly, Young places less emphasis on the purely economic problems (mobilization of resources, control of monetary disorder) and more on the general changes in society which alter the meaning and significance of economic problems (compare 1920a, 1921a, 1921b, 1923b, 1924c, 1924d). "If the war continues it is certain that the field of activity over which the public interest will be deemed to extend must be much further widened" (1918:2). "Political organization has not kept pace with economic organization" (1926:11).

This context provides for a rather different reading of the justly famous "Increasing returns and economic progress" (1928h). Posing as a gloss on Smith's dictum that the division of labor depends on the extent of the market, the paper can be read also as a reflection on the likely economic consequences of the gradual restoration of free international commerce in the aftermath of World War I. For Young, the history of American economic development showed what lay ahead for world economic development. Widening the extent of the market would inevitably stimulate economic progress, and economic progress would then stimulate further widening of the market in a cumulative process of endogenous growth. What was needed was an institutional basis, both economic and political, that would do for the world what the hard-won institutional basis at the level of the nation had done for the development of America.

Theory and practice

The papers in Section II backtrack a bit to trace the prewar development of Young's thought. The early extract on "the social dividend" (1908) shows Young's conception of the economy in its "togetherness," a conception remarkable for its synthesis of the individual with the social point of view (his "attribution" theory of distribution), the static with the dynamic (his "waiting" theory of capital), and the real with the monetary (his "money capital" theory of interest). The economy is not only a process in time—Young emphasizes that past sales, not current ones, pay for currently produced output—but also a process of growth (increasing roundaboutness), and a process in which the flow of funds through the financial system from surplus to deficit agents plays a critical role. The extract appeared as an appendix to the chapter on profits in the 1908 edition of the *Outlines*. Later editions (1916, 1923) gave increasing attention to the role of money and credit institutions, but already in 1908 Young's vision was clear of the way strains and maladjustments arise and contribute to business fluctuation, a vision that lies behind his later work on the way the banking system may exacerbate or moderate these strains.

The lectures on socialism (1912) show Young's conception of the nature of the communal problems that, in his view, were directing the development of economics as a science before war intervened. For him, the significance of the socialist movement came mainly from an increased appreciation that "private property is not a simple thing, but a complex thing; not a single right, but a bundle of rights." More specifically, the socialist movement was responsible for the conception that there is a general interest in increasing the social dividend and in distributing it more equitably. The challenge for economics was to figure out how such new goals might best be achieved in the context of a private property market economy.

This context provides for a radier different reading of another of Young's famous articles, his review essay on Pigou (1913b). Posing as a critique of some technical aspects of Pigou's theorizing, the paper can be read more deeply as a byproduct of Young's ongoing reflection about the practical consequences of a commitment to maximize the social dividend. His suggestion that Pigou might have benefited from "more intimate acquaintance with the recent work of American public utility commissions" reveals Young's conception of where the best work on the question was being done, not by theorists but by practitioners. In this regard, his paper on "Public borrowing for road building" (1915c) can be seen as more than just an occasional contribution, and instead part of a more general inquiry into the fundamental principles of public finance (compare 1913d, 1914b, 1914c and 1915a, 1915b, 1915d). The consideration (and rejection) of agriculture as a kind of public utility in the paper on "The economics of farm relief" (1926e) shows that Young carried his prewar viewpoint into the postwar period. The mature essays on "Economics," "Capital," and "Supply and demand" (1929b) show Young returning to the mode of his early "Social dividend" essay, attempting to draw a picture of economics once again in its togetherness after the distorting pressures of war had passed, so he thought.[1]

Commerce: the marketplace of the world

Section III contains selections from the first thirty chapters written by Young for the *Book of Popular Science*. These chapters were the result of an invitation from W.F.Kellogg of the Grolier Society of New York to participate in the production of an American edition of a well-known British encyclopaedia, the *Harmsworth Book of Popular Science* (1913). Young was asked to write thirty-six chapters on "Commerce" that paralleled, for the US and Canadian market, a similar number of chapters that had been written for Harmsworth by Sir Leo George Chiozza Money.[2]

The first versions of Young's original thirty-six chapters for the Grolier Society's *Book of Popular Science* were completed by Young between January and August 1922, but with revisions and updating (and two new chapters) over the next few years as new editions appeared. Young apparently used Chiozza Money's text as a starting point, though his revisions and additions were extensive. By comparing the Grolier edition with the Harmsworth, it was possible to separate Young's contribution. The selections included here represent about three quarters of Young's total contribution. Space considerations regrettably indicated deletion of much purely statistical or descriptive material, as well as the single chapter that surveyed Canada's prospects.[3]

At first glance the chapters appear as a mere compilation of facts about the American economy, but closer reading reveals a fascinating experiment in pedagogy (compare Riley 1925), and more. Rather than beginning, in the manner of most textbooks, with abstract concepts which are then used to illuminate some aspect of lived experience, Young begins in Chapter 1 with a big idea—trade is the source of wealth—and then proceeds to spend the next twenty-three chapters building a statistical portrait of trade in all its dimensions. The level of abstraction rises a notch in Chapters 20 and 21 where the concepts of wealth and income are examined critically for the first time, but the discussion remains largely at the level of statistical measurement. Chapters 25–7 are the first essentially non-statistical chapters since the opening, and only in Chapter 30 on "The meaning of value" do we find the kind of abstract discussion of economic concepts with which most textbooks begin. What is Young doing? In effect, he has arranged his materials in such a way that the reader learns economics inductively, passing from the concrete to the abstract and from the particular to the general, rather than the other way around.

So seductive is the lesson, and so sure is Young's guiding hand, that it is easy to lose sight of how radical and far-reaching the underlying big idea is. Trade is the source of wealth, not the innate fertility of land (Ch. 5), not the diligence of labor (Ch. 25), and not the ingenuity of invention (Ch. 11), but trade. It is trade that determines whether the capacity of soil to produce a particular crop is also the capacity to produce value, and whether labor however diligent or invention however ingenious is labor or invention that produces value. The natural resources of the world, including population, are only potential supplies and potential demands. It is trade that makes those potentialities manifest, and so creates wealth. The extent of the market limits the division of labor, and so also the productivity with which wealth is created. It is a theorem of Adam Smith, but physiocratic blinders limited Smith's vision of its implications. Not so Young, who, in direct contradiction to Smith, viewed Great Britain's specialization in the carrying trade (shipping) as a wise move (Ch. 3). Let the reader beware! There is a powerful theoretical argument lurking within these apparently benign statistical chapters.

Indeed, the theme of the *Grolier* to a large extent anticipates Young's later, and much more well-known, work on increasing returns, which in turn anticipates modern work on endogenous growth theory. Note, for example, his emphasis on what we would call human capital: "industrial and technical knowledge which the members of the community possess, handed down from generation to generation, diffused by education and increased by the advances of science" (Ch. 26). Even more significant, because so unexpected, although Young admits that agriculture faces diminishing returns in the short run and when viewed as a separate sector (Ch. 27), nevertheless he insists on the high elasticity of agricultural supply as population and the size of the entire market economy extends in the long run (Chs 2, 8; see also 1926e). The point is that cost reductions in manu-facturing, services, agriculture, and especially transport interact symbotically. They depend on each other and they depend on overall growth in the national and international economy, and overall growth is in turn sustained by these sectoral improvements that together extend the size of the reachable markets and thus the size of potential demand. In this regard the discussion of cotton (Ch. 11) is especially revealing—Young is careful to emphasize that technological improvements are not the cause of growth, but rather large markets that make the use of new technologies economically feasible. This is endogenous growth, albeit not via the mechanisms usually emphasized in the modern literature (*see* discussion of Section V below).

Money and credit

Section IV is best approached, at least on first reading, as Young's attempt to work out his own answer to the exam questions he posed for his students, both undergraduate and graduate, in 1922. One sees in the questions that Young was working with the entire gamut of monetary thought, both the quantity theory of money and the real bills doctrine, both the gold theory of money's value and the legal tender theory, both the viewpoint of the monetary theorist and the viewpoint of the practical banker. And he was working with the entire gamut of monetary phenomena, both domestic and international, both long run and short run. One sees in the answers that Young took to heart his own dictum that theories are never completely right nor completely wrong, and that it is better to inquire about the significance of a theory for some particular phenomena. The farthest thing from an eclectic, Young played the entire scale of monetary theory because he was interested in understanding the range of monetary experience "in its togetherness."

The undergraduate questions get answered first in *Grolier* Chapter 31 ("The Value of Money") where Young draws the distinction between money and credit. Defining money by its function as the means of payment, Young includes in the money stock such forms of circulating credit as national bank notes (Ch. 32), but he nevertheless maintains the distinction between banks notes (money) and deposit accounts (credit) on the grounds that the latter depend on the personal credit of the depositor. Although, for historical reasons, the U.S. money stock is comprised of many different types of money (Young counts ten!), all the various types have one thing in common, and that one thing is what fits them all to serve as money, according to Young. They are all elastically "interchangeable" with the standard money, gold.

Elasticity is, for Young, the essential quality of money, the quality that enables money to serve as a means of payment. "An elastic demand in itself creates a safe outlet" (Ch. 31). In primitive monetary systems, the most elastically demanded commodity typically is chosen to serve as money (Ch. 31). The evolution of modern monetary systems (Chs 32–4) is the story of our attempt to perfect institutional mechanisms that ensure the elasticity of modern monies. More specifically, the evolution of modern monetary systems is a story of progress from the currency principle—such as lay behind both the 1844 Peel Act in Britain and the 1863 National Banking Act in the U.S.—to the banking school principle that lay behind the actual practice of the Bank of England (an elastic deposit currency, if not note issue) and both the design and functioning of the Federal Reserve System after 1913 (elastic deposits and note issue both). Young summarizes the difference between the two ways of thinking: "The currency principle stresses the analogy of bank-notes to government paper money, while the banking principle emphasizes the similarity of the bank-note and the deposit" (Ch. 33).

As between the two, there can be little question that Young's sympathies lie with the banking principle as he defines it. Against the currency principle, he finds a world of difference between irredeemable government paper money (a paper standard) and a bank note currency redeemable in gold. In the former case, exemplified for Young by the Greenback Era, Gresham's law operates to drive gold out of circulation and to depreciate the value of paper money against gold. In the latter case, the note issue is not a competing standard but only a subsidiary currency that gains its value from its exchangeability with the single standard. The better analogy is between bank notes and bank deposits—"A bank-note, like a bank deposit, is a bank's promise to pay on demand" (Ch. 31)—which is not to say the analogy is exact in all respects. For example, notes remain longer outstanding and so are more liable to excessive issue (Ch. 32). That said, however, there is no mistaking Young's enthusiasm for the new Federal Reserve System, nor the reasons for that enthusiasm. "Taking the system as a whole, it will be seen that it gives a thoroughly elastic supply of credit. It has all of the necessary elements: elastic note issue, elastic deposits and elastic reserves" (Ch. 33).

The evolution toward greater elasticity represents perfection of the monetary apparatus, which is a good thing in itself insofar as disruptive monetary crises can be avoided, but the really significant gains come from the indirect and longer term effects. Starting from the idea that trade is the source of wealth, it is not too far a stretch to the conclusion that the institutions of money and credit are also a source of wealth, insofar as they support the expansion of trade (Ch. 33). Young takes the lesson of history that the institutions of money and credit tend to follow rather than lead economic development, and often with a considerable lag because politics, not economics, most often drives financial development, at least in the short-run. (The 1926 introduction to Steels contains Young's most forthright account of the political forces involved—the debtor interest vs. the creditor interest, the frontier west vs. the New York "money power.") The elasticity of the monetary system is therefore a good thing insofar as elasticity supports economic development.

Elasticity having been achieved, certain perennial economic problems were solved, but Young saw new problems emerging in their place, namely the problem of the business cycle. Young thought that business cycles were a matter of strains and maladjustments in the dynamic adaptation of the pattern of production to the pattern of demand, and he thought further that the credit system was integrally involved in these strains because credit allowed the pattern of current income to diverge from the pattern of current spending. Starting from this point of view, Young came naturally to ponder the question how the new Federal Reserve System apparatus might be employed in order to smooth, or at the very least not to exacerbate, business fluctuation. This is the subject of Chapters 35 and 36.

As Young approached the problem, the point was to find ways to improve on the kind of automatic regulation of cycles that had occurred in the previous regime. Man's invention of a banking apparatus to ensure elasticity meant that the credit system was no longer automatically regulated by the scarcity of reserves. The point is not that the automatic system worked all that well—it didn't—but rather that regulation now requires conscious and deliberate effort by the monetary authorities. What should they do, if anything? Here it is important to be very clear that Young rejected the laissez-faire policy of passive accommodation. Though he favored an elastic credit system, he also believed (with Hawtrey) that the credit system was inherently unstable, and so required regulation.

More specifically, Young rejected the real bills doctrine as a guide for monetary policy despite his sympathy for the banking principle with which the doctrine is usually associated. Unlike real bills advocates, Young recognized not only the impossibility of drawing a strict line between real and finance bills, but also the undesirability of doing so for the purposes of monetary policy. No real bills advocate would ever have written, as Young did: "the speculator, so far as he operates wisely and shrewdly, is in reality a producer of wealth" (Ch. 35). Young understood business enterprise as a process in time, inherently involving speculation, and so found himself unable to condemn those who made a business speciality out of one dimension of speculation. What then were the principles on which the newly formed monetary authority could rely?

In this regard, Chapter 36 can be understood as an extended contrast between the work of Irving Fisher and Ralph Hawtrey, to the definite advantage of the latter (compare 1920b, 1923a, 1924e; *see also* Mehrling 1997, Ch. 4). Young's sympathies were with those who wanted to manage money, but he realized, as others did not, that managing money was a difficult thing, not a simple thing. Following Hawtrey, Young hoped that judicious discount rate policy, enforced by open market operations, will enable the monetary authority to stabilize the business cycle, raising the rate to slow an unsustainable boom and lowering it to speed recovery from recession. Young's discussion makes clear that the point is not so much to control the outstanding stock of money (notes) issued by the monetary authorities as it is to control the flow of expenditure by controlling the flow of new credit. Modern readers may be misled by Young's use of the framework of the quantity theory of money, a framework nowadays most often associated with the currency principle, so it is worth repeating. To the extent that credit control is successful, expenditure will remain in line with output so prices do not rise. At the same time, slower growth of bank lending means slower growth of bank liabilities, be they bank notes (money) or deposits (credit). Thus money and prices move together, but they do so because they are both effects of a third cause.

Hawtrey of course made much of the "art" of central banking, but in Chapter 35 Young seems to have been looking for more scientific principles by developing a theory of how the price of money, which is the interest rate, affects borrowers' demand for it. ("The price of money means the rate of interest asked for the loan of it." Ch. 34.) The importance of such an exercise follows from the fact that, in a world of elastic currency supply, not the quantity of money but only the price of money can be an instrument of monetary policy. The challenge of developing a theory of money demand comes from the fact that, as Young says, money is not demanded for itself, but only as a means of payment, which means that the usual demand and supply analytical apparatus may be unreliable. Young begins to make analytical progress on the problem by considering the demand for money to be a speculative demand, i.e. a demand based on expectations of price changes, not a demand for the underlying commodity.

The importance of this analytical direction is not fully apparent in the chapter itself, which merely mentions in passing the relation between stock market speculation and the money market, a relation Young deplores. It is only in subsequent work, most notably *Bank Statistics,* that Young spins out the implications for monetary control. Unlike in Hawtrey's Britain, the crux of instability is not in the money (bills) market, but in the long-term capital markets, aided and abetted by bank lending. Furthermore, the instability is not so much a trade cycle as it is an industrial investment cycle, and it is less connected with international swings and more connected with domestic rhythms. Unlike Hawtrey (and unlike his own gloss on Hawtrey in *Grolier* Ch. 36), Young came to think there was "no basis for the belief that these cyclical swings, once under way, were never halted until the resources of the banks had been exhausted." Because the channels of instability were different from those emphasized by Hawtrey, the nature of the instrument for control would also have to be different, not discount rate manipulation to control the pace of bank lending, but rather open market operations to influence the timing of capital refunding operations (see also 1923c).

Young's attempt to bring the theory of speculative asset pricing to bear on the field of money was not confined to the rather specific institutional setting of the United States. Note how, for example, in "Downward Price Trend Probable" Young traces the demand for gold reserves not so much to the quantity of gold relative to transactions but rather to the demand for gold as determined by banking institutions and the degree of confidence in their continued smooth operation. Even more interesting is his account of money demand in the inconvertible paper money system of postwar France (1929g), where foreign exchange speculation played the major role. Here the important difference from the gold standard case is that, as Young says, "There is no 'limiting' or 'ultimate' supply of inconvertible money apart from such restrictions as the government may choose to impose upon itself or upon the banks." For Young, the significant question is not whether a change in the money supply causes a change in prices, but rather whether or not the government is able and willing to impose restrictions and, even more significant, whether or not the (speculative) holders of money believe in the government's ability and willingness to do what needs to be done.

In this regard, Young found it hard to overestimate the importance of the establishment of the Federal Reserve System, a move for which he gives decisive credit to Woodrow Wilson's political skill (1926d). For Young, the establishment of the Fed was about much more than mere technical improvements in the operation of the monetary system. Note the emphasis he places on the fact (as he thought) that the System was designed not so much to mimic European central banks, but rather to mimic the clearinghouse mechanisms that banks had innovated during banking crises (such as that of 1907) in order to overcome the defects of the National Banking System. The Federal Reserve System took over the instruments designed in the very heart of the money power, and ensured that hereafter they would be utilized for the public interest (compare 1927d). A triumph of economics over politics, and of the general interest over merely sectional interest, the Fed would be a force for stability not so much on account of its wise intervention, since it would inevitably err, and not so much on account of its strong control over the system, since its control was anything but strong. Rather, the real importance of the Fed, according to Young, was that no longer would monetary issues be treated as a "football of politics." Stability would come first and foremost from the fact that, given the institution of the Fed, speculators had less reason than ever before to doubt the government's willingness and ability to do what needed to be done. It was to be expected, therefore, that what speculation remained would overwhelmingly be of the "prudent" sort that operates to stabilize markets, not to destabilize them.

Having been for so long a source of monetary instability for the world economy, the United States became after the establishment of the Fed the great bastion of stability, at least as compared with other central banks (compare 1924c, 1924d, 1924f). The next logical step forward, so Young argued in "Downward price trend probable" (1929a), would involve overcoming the merely apparent sectional interests of individual nations in favor of the true international general interest, in effect applying the lessons of U.S. history at the world level. Unfortunately, for lack of any overarching institution capable of taking the general view, atomistic competition of individual central banks for a limited gold reserve was beginning to place completely unnecessary strain on the world's monetary system (much as the defective National Banking system had done previously). Young thought the strain would cause long-term deflation, but he was too optimistic. As it happened, the strain instead led to collapse and worldwide depression, though Young did not live to see it.

Growth and fluctuations

In Section V all these monetary issues are in the background, while Young turns his attention more directly to the questions of economic growth and fluctuation. For both questions, Young seems to have felt that the standard economist's tools had little to offer, designed as they were to handle the rather different questions of value and distribution. Nicholas Kaldor's notes on Youngs LSE lectures are best read as an initial attempt to work out an alternative analytical apparatus, with mixed success.

In his discussion of the determinants of long-run growth, Young seems to be struggling, with some misgivings, to adapt the static and microeconomic Marshallian long-run demand and supply apparatus to the dynamic and macroeconomic problem of growth.[4] The problem Young faces is that he views growth as fundamentally a phenomenon of increasing returns, and "seeking for equilibrium conditions under increasing returns is as good as looking for a mare's nest." Analytical progress can be made by abandoning the idea of increasing returns, as we know from the neoclassical, constant-returns growth model of Solow (1957), but this direction was not open to Young, given his theoretical priors.

The problem was not just that Young viewed increasing returns as the essence of growth, but also that he would have been uncomfortable building a theory of growth on the neoclassical theory of distribution. Wages and profits measure compensation, which is not the same thing as contribution because of the pervasive externalities involved in the growth process.[5] In this regard, it is instructive to compare Young's growth accounting (*Grolier* Ch. 37) with the more familiar neoclassical growth accounting. Young notes that production per worker increased because workers were better equipped, and he notes that compensation per worker increased as well, but he never uses compensation as a measure of productivity. Further, he carefully avoids the vexed question of measuring capital, using instead a physical measure of horsepower to suggest the degree to which workers were better equipped. In the end, Young was too much the empiricist ever to be comfortable with the kind of analytical abstractions on which modern developments in growth theory have been based.

Young's discussion of fluctuations is more successful, since here analytical progress can be made by viewing fluctuations as deviations from equilibrium (*see* LSE lecture, 1 May), much in the same way that asset price fluctuations can be viewed as temporary speculative deviations from true valuation (compare *Grolier* Ch. 35). Notably, he presents this analysis only after a critical review of alternative explanations which he suggests don't quite get to the core of the question, despite their useful emphasis on the interplay of expenditure and income, of investment and savings, of purchasing power and output. The alternative view that business cycles are about speculation not only offers a simpler analysis, but also leads directly to the policy conclusion that credit control may be an effective lever of stabilization. Also possibly effective, improvements in business forecasting may help to ensure that prudent stabilizing speculation would come to dominate the imprudent destabilizing sort.

The discussion of deviations from equilibrium may leave the misleading impression that Young thought equilibrium was an achievable policy goal. Not so: in his mind, the essence of a modern capitalist economy was not stability but growth and structural change. For him, not only aggregates but more importantly the pattern of output and the pattern of demand were in constant flux, flux that showed up not so much in the overall price level as in the constantly shifting structure of relative prices. It is the stresses and strains caused by misalignment that give long-run growth its cyclical character, but those stresses and strains are the very stuff of economic growth, and cannot be abolished without abolishing growth. What Young wanted was to use the credit apparatus to buffer the economic system at times of stress, without allowing undue credit expansion or contraction to become an additional source of stress. For Young, fluctuation came with growth, but excessive fluctuation could be a barrier to growth. He hoped to use the new Federal Reserve System apparatus to walk a line between too much fluctuation and too little, both of which stand in the way of achieving potential economic growth.

If the LSE lectures may be viewed as the early stages of an academic treatise, the two *Grolier* chapters 37 and 38 (apparently written at about the same time as the lectures) show the broader vision of growth and fluctuation that lay behind Young's analytical efforts. "Economic changes since the war" poses as an account of the causes and consequences of the shift in the center of the world economy toward America, only to downplay the shift by placing it in the longer context of economic development at the world level. What is most significant, in Young's view, is not the relative rise of America, but rather the continuing expansion of the market at the world level. War, currency instability, and continuing bouts of economic nationalism all pose threats to further expansion, but Young views them as temporary setbacks only. At the world level, just as at the level of the United States, he was confident that economics and the general interest would win out in the long run, even though politics and sectional interests seem so often to win out in the short run.

It is a grand and hopeful vision, but what, one may ask, does it mean for me? "Big business" (Ch. 38) can be read as Young's attempt to answer the question posed by the impatient encyclopaedia reader. Doesn't the expansion of the market mean large-scale business, monopoly that threatens democracy, and standardization that threatens treasured variety of life? No, says Young. The expansion of the scale of production does not necessarily mean expansion of the size of the representative firm. It all depends on the particular product in question. Expansion of production means reorganization of production, which may or may not mean integration and standardization. On a number of specifics, Young's forecast of the future was apparently wide of the mark. The rise of agribusiness certainly contradicts his forecast of the survival of the family farm, and the integration of the printing industry contradicts his forecast of the survival of craft on account of the variety of products. Nevertheless, the larger lesson remains as much worth attending today as it was in his time. Small business does survive, much as Young expected it would, and even in industries apparently dominated by a few very large firms.

In this regard, Young's extended account of the forces changing the face of the retail trade in his time makes fascinating reading even today. Where he writes about the telephone and the automobile, the modern reader will substitute the internet and inexpensive package delivery services. Where he writes about the rise of the department store and the chain store, the modern reader will substitute the rise of large discount retailers (Walmart) and specialty catalogues (L.L.Bean). Young reminds us, as he reminded his own intended audience, that there are diseconomies of scale as well as economies of scale, that expansion of the size of the market does not necessarily mean expansion of the size of the representative firm, that standardization does not only crowd out craft but also provides additional scope for the expression of craft, that increasing returns are realized not just at the level of the firm, but also at the level of the industry, the nation, and, most significantly, the world economy as a whole.[6]

In his togetherness, the lesson of Young may be summarized. Trade is the source of wealth, and expansion of the market is the source of increasing wealth. The point, however, is not wealth *per se,* but rather the civilization that wealth makes possible, and the democratic civilization that a competitive market economy supports. In this respect, expansion of the market is also about expansion of liberty, and about the general interest winning over mere sectional interest. Young's enthusiasm for the market was the farthest thing from the apologetics of a laissez-faire advocate.

Where monopoly threatened competition (railroads and utilities), he sought principles of regulation. Where atomistic private property threatened the general interest (water resources and banking), he sought principles of public management. For Young, the market was always a means to an end, not an end in itself. The role of the state was to ensure that the potential general benefits of the market—in terms of liberty as well as wealth—were actually realized, and not dissipated in merely sectional or individual appropriation.

Notes

1 Taken together, "Social dividend" (1908) and "Economics" (1929) shed some additional light on the origins of Frank Knight's famous circular flow diagram (Patinkin, 1981). Part of Patinkin's search for the "wheel of wealth" led him to

Knight's 1951 *Encyclopaedia Britannica* entry entitled "Economics," but this was actually a lightly edited version of Young's 1929 entry. Patinkin's search also missed Young's 1908 "social dividend" discussion because he consulted only the first (1893) and last (1937) editions of the *Outlines* (Patinkin 1981:54), and Young's discussion disappeared after the 1923 edition, perhaps because it was found to be too difficult for students (Blitch 1995:56). Knight studied economics with Young at Cornell and would have known Young's 1908 revised version of Ely's text.

2 Chiozza Money (1870–1944) was, among other things, an economic journalist noted for his writings on the distribution of income in the UK, set forth in *Riches and Poverty,* London 1905. He was elected Liberal Member of Parliament from 1906–18, during which time he served on various official select committees dealing with economic matters. From 1915–16 he was private secretary to the then Minister of Munitions, Mr Lloyd George, and in 1917 he drafted the new Pensions Scheme while serving as private secretary to the Minister of Pensions. He resigned from the Liberal Party in 1918 to stand (unsuccessfully) as the Labour parliamentary candidate for South Tottenham. He was later the editor of the economic, financial, industrial, engineering and sociological sections of the 14th edition of the *Encyclopaedia Britannica,* 1929. Young was familiar with Chiozza Money's work from an early date (see Young 1911a). In addition to his work on Money's text for the Grolier project, Young also contributed eleven entries to the *Encyclopaedia Britannica* under Money's editorship, three of which are reproduced in this volume.

3 We have excluded those *Grolier* chapters where Young made only minor alterations to what was written by Chiozza Money for Harmsworth. These included chapters on coal, cotton goods, wool, and the shipping of the world. The chapters that were written mainly or exclusively by Young but not reproduced in this volume are: Chapter 10 on wheat in international trade; Chapter 12 on the trade in cotton goods; Chapter 15 on America's merchant marine; Chapter 18 on America's water routes; and Chapter 24 on Canada.

4 The diagrams in the lectures on "Particular expenses and supply curves" help explain the famously puzzling diagram and mathematical note at the end of his 1928 presidential address (see Reid 1989, Ch. 8).

5 Compare *Grolier* Chapter 26: "Appraised by his real contributions to wealth and welfare, not in dollars and cents, the scientist may easily outrank the millionaire or the captain of industry. His contributions to society's capital are, in general, free. For that reason they do not fall under the ordinary laws of supply and demand." Similar points were made at the end of his discussion of the "social dividend" (1908).

6 Young's agnosticism over the appropriate size of the individual firm or plant, his stress on the desirability and intensity of competition as the size of the market expands, and his skepticism over the desirability or effectiveness of patent protection, all contrast rather sharply with modern endogenous growth models (*see* Aghion and Howitt's recent survey, 1998) and "new trade theory" (pioneered by Krugman 1990). These models focus mainly on *internal* economies of scale; present a case for investment subsidies, patents and tariff protection; and view monopolistic competition as a necessary and integral part of the growth process.

References

Aghion, Philippe and Howitt, Peter. (1998) *Endogenous Growth Theory.* Cambridge, Mass, and London: MIT Press.

Blitch, Charles P. (1995) *Allyn Young: The Peripatetic Economist.* London: Macmillan Press.

Currie, Lauchlin. (1981) "Allyn Young and the Development of Growth Theory," *Journal of Economic Studies* 8, 1:52–60.
——(1997) "Implications of an Endogenous Theory of Growth in Allyn Young's Macro-economic Concept of Increasing Returns," *History of Political Economy* 29, 3 (Fall): 413–43.
Deutscher, Patrick. (1990) *R.G.Hawtrey and the Development of Macroeconomics*. London: Macmillan Press.
Ely, Richard T. (1893, 1908, 1916, 1923, 1930, 1937 editions) *Outlines of Economics*. New York: Macmillan.
Friedman, Milton and Schwartz, Anna J. (1963) *A Monetary History of the United States, 1867–1960*. Princeton, NJ: Princeton University Press (for NBER).
Gregory, T.E. (1929) "Allyn Abbott Young," *Economic Journal* 39:297–301.
Kaldor, Nicholas. (1972) "The Irrelevance of Equilibrium Economics," *Economic Journal* 82: 1237–55.
——(1985) *Economics Without Equilibrium*. Cardiff: University College of Cardiff Press.
Krugman, Paul R. (1990) *Rethinking International Trade*. Cambridge, MA and London: MIT Press.
Laidler, David. (1993) "Hawtrey, Harvard, and the Origins of the Chicago Tradition," *Journal of Political Economy* 101, 6 (December):1068–103.
——(1998) "More on Hawtrey, Harvard and Chicago," *Journal of Economic Studies* 25, 1:4–24.
Mehrling, Perry G. (1996) "The Monetary Thought of Allyn Abbott Young," *History of Political Economy* 28, 4 (Winter):607–32.
——(1997) *The Money Interest and the Public Interest: American Monetary Thought, 1920–70*. Harvard Economic Studies, 162. Cambridge, Mass., and London: Harvard University Press.
Morgenstern, Oskar. (1929) "Allyn Abbott Young" (as tr. and repr. in A.Schotter (ed.) *Selected Economic Writings of Oskar Morgenstern*. New York: New York University Press, 1976).
Patinkin, Don. (1981) "In Search of the 'Wheel of Wealth'," in *Essays On and In the Chicago Tradition,* Durham, NC: Duke University Press, Ch. 2:53–72.
Reed, Harold Lyle, (with Allyn Young). (1925) *Principles of Corporation Finance*. Boston: Houghton Mifflin.
Reid, Gavin C. (1989) *Classical Economic Growth*. Cambridge: Cambridge University Press.
Riley, Eugene B. (with Allyn Young). (1925) *Economics for Secondary Schools*. Boston: Houghton Mifflin.
Romer, Paul M. (1989) "Capital Accumulation in the Theory of Long-run Growth," in Barro, Robert J. (ed.), *Modern Business Cycle Theory.* Cambridge: Harvard University Press.
Sandilands, Roger J. (1990) *The Life and Political Economy of Lauchlin Currie: New Dealer, Presidential Adviser and Development Economist*. Durham, NC: Duke University Press.
Schumpeter, Joseph A. (1912: first German edition; English edition, 1934) *The Theory of Economic Development*. Cambridge, MA: Harvard University Press.
Taussig, Frank W, Bullock, Charles J. and Burbank, Harold H. (1929) "Allyn Abbott Young," *Quarterly Journal of Economics*. August.
Young, Allyn A. For these references, see Chapter 47, Bibliography of Allyn Young's writings at the end of this volume.

Acknowledgements

Chapter 1 first appeared in *The Quarterly Journal of Economics* 42:1, (November 1927), pp. 1–25 and is reprinted with the kind permission of MIT Press Journals © 1927 by the President and Fellows of Harvard University.

Chapter 2 first appeared in *Economica* (March 1928), pp. 1–15, and is reprinted with the kind permission of Blackwell Publishers.

Chapter 4 first appeared in the *American Economic Review* 16:1, (March), pp. 1–13 and is reprinted with the kind permission of the American Economic Association.

Chapter 5 first appeared in the *Economic Journal* (December 1928) and is produced with the kind permission of Blackwells Publishers on behalf of the Royal Economic Society.

Chapter 8 first appeared in *The Quarterly Journal of Economics* 27:4 (August 1913) pp. 672–86.

Chapters 11, 12 and 13 are reprinted with permission from *Encyclopaedia Britannica,* 14th edition © 1929 by Encyclopaedia Britannica, Inc.

Chapters 14–31, 33–38 and 45–46 first appeared in *The Book of Popular Science,* 1924 edition. Copyright by The Grolier Society. Reprinted with permission.

Chapter 32 is part of the Allyn Young Papers and is reprinted courtesy of the Harvard University Archives.

Chapter 39 first appeared as two journal articles in *The Review of Economics and Statistics* 6:4, (October 1924), pp. 284–96 © 1924 by the President and Fellows of Harvard College and *The Review of Economics and Statistics* 7:1 (January 1925), pp. 19–37 © 1925 by the Presidents and Fellows of Harvard College. This material is reprinted with the kind permission of MIT Press Journals.

Chapter 40 first appeared in Steels, J. *La Banque à Succursales dans le système bancaire des Etats-Unis* (1926), and is reprinted with the kind permission of the Faculteit Rechtsgeleerdheid, Universiteit Gent.

The publishers have made every effort to contact authors/copyright holders of works reprinted in *Money and Growth*. We would welcome correspondence from any individuals/companies we have not been able to trace.

Part I

The nature and scope of economics

1 Economics as a field of research

Quarterly Journal of Economics (1927) 42, 1 (November): 1–25. Read before the Institute for Research in the Social Sciences, of the University of Virginia, on May 20, 1927, as the second of a series of lectures dealing with the fundamental objectives and methods of research in the social sciences.

I. The social sciences differ from the physical in that the observer's interest lies within them.—The contractual and the institutional views of society.—Corresponding types of investigation.—The genetic point of view.—II. Group research and its promise.—Induction and deduction.—Fruitful hypotheses essential.—Individual research; the constructive imagination.—Promising types of research.—The limitations and promise of research.

I

The social sciences, like the natural sciences, proceed upon the one great premise that the intricate flux of events can in some way be explained. What appear to be arbitrary or capricious happenings can be fitted into a scheme which has no room for anything but dependable uniformity and regularity. Such is the first article of the scientist's creed. The second article of that creed is that the one way to come to a knowledge of these hidden uniformities is by means of those patient and methodical inquiries which we call research.

The social sciences, however have to be distinguished from the physical sciences, not only because the phenomena with which they deal are more complex, because their data are less exact, and because the experimental method which the more rigorous physical sciences employ is generally not available to them, but also because they encounter problems of *orientation* which are peculiar to them and from which the physical sciences are free. The physical scientist sets himself, as an impartial observer, outside of nature, inquires into nature's processes, and tries to reduce them to simple general relations. He does not hope to be able to change nature, or even in any literal sense to gain "increased power over nature." But he knows that as we come to understand nature's processes better we are able to make better use of them—which means merely that in our ways of doing things we take account of our new knowledge. The data of the physical sciences are physical phenomena, but the problems which these sciences seek to solve are born of human interests, and so far as the knowledge which they yield has instrumental value, it serves human ends and leads to modifications of human arrangements.

The social scientist cannot, in any comparable way, put himself, as an impartial observer, outside of society, so as to get a view of social processes as a connected whole. His interests, his values, his ends, lie *within* that connected whole. Every occurrence in the contemporary life of a society enters into two separate sets of relations. In the first place, every such occurrence is a phenomenon, a scientific datum, which has to be fitted into the ordered scheme of social processes. In the second place, every such occurrence has its own immediate and concrete significance, and has to be accorded its due weight in any system of social values. We seek to understand the impersonal processes of nature and to take account of them, but we neither approve nor disapprove of them. We also seek to understand and to take account of social processes, but we reserve the right to approve or disapprove of them. We do not hope to change nature's uniformities; but the processes of organized society, we believe, are in some degree plastic. So far as the knowledge which the social sciences yield has instrumental value, it serves social ends and leads to modifications of social arrangements. In any complete view the realm of the phenomena of organized society and the realm of ends are coterminous. The great first premise of the scientific method compels us to view these phenomena as rigidly determined and predictable, while the interests that prompt our scientific inquiries imply that they are in some measure amenable to control.

Upon the general philosophical aspects of the predicament in which the social scientist finds himself I do not propose to dwell. My present concern is with the practical devices by means of which men interested in social problems have been able to get something of value out of the scientific study of social processes. These devices all involve some particular orientation and some particular ordered scheme of abstraction. The traditional type of economic theory, for example, rests upon the common interest in increasing the production of wealth and securing its juster distribution. The data which it submits to scientific scrutiny (the pertinent aspects of the physical environment, along with other commonplace facts, being taken as given) are the reciprocal relations between certain types of human conduct that appear to be fairly stable and dependable in the mass, and the variations of such economic magnitudes as product, prices, wages, costs, profits, and interest rates. The economic processes of society are thus viewed as constituting an intricate but reliable mechanism, operating in an orderly and predictable way.

But this economic mechanism is something more than an object for scientific analysis and contemplation. It may be controlled, directed, or interfered with. It is a social instrument, to be *used* as our communal interests may dictate. Above the economic man stands the political man, free to limit and define the field of the economic man's activity, to impose conditions upon him, to prevent him from doing certain things, to encourage him to do others. It is incorrect, therefore, to say that the traditional political economy implies a wholly mechanistic view of human society. All that it implies is a particular orientation, with one particular set of social processes viewed as a mechanism by free agents who want to understand the workings of the mechanism because they want to know how best to control it and use it. They want to know how far to control it and how far to leave it alone, and it is desirable that they should be able to predict the more remote as well as the immediate effects of particular measures of control. Agents, mechanism, instruments, and ends are thus all in the picture. Doubtless they are seen in a one-sided and partial way, and yet this view of things has proved itself to be practically serviceable, and the traditional political economy which embodied it was one of the great intellectual achievements of the nineteenth century.

Every social science has to be defined in terms of its problems, and accordingly includes agents, instruments, and ends, as well as a mechanism, among its postulates. But every social science has its own particular orientation. Thus for political science the behaviour of the political man may well be an object of scientific scrutiny, just as educational science may focus its attention upon the learner and criminology upon the law breaker. The same human activities which one science regards as sufficiently uniform and dependable in the mass to make scientific analysis of them profitable, appear in other social sciences as free or plastic. To the economist the citizen, the voter, may be a free agent; to the political scientist his conduct may be in some measure determinate; to the student of education he may be a bit of malleable human material.

There is no necessary conflict between these different views, for each is a partial view. Held to consistently, they would separate the different social sciences rather more narrowly and rigidly than is practically desirable. A worker in any part of the field of the social sciences needs to be aware of the importance for his own problems of more orientations than one. But I venture to hold that no complete *scientific* synthesis of all the different social sciences is possible, if only for the reason that, as I have said, the inquirer, with his interests, must stand somewhere *within* society and its processes.

There is another problem of orientation, which cuts across all the social sciences, for there are two different possible views of the general structure of society. Both views can be traced back as far as the Greeks, but sometimes one view and sometimes the other has been dominant. These two views, or ways of conceiving the structure of society, are the contractual and the institutional. In the contractual view social arrangements are deliberate contrivances resting upon voluntary agreements—instruments which men use in attaining their purposes. In the institutional view these same arrangements appear as social habits, the products of history, not really shaped by the rational prevision of men, but dominant factors, themselves, in determining what men's purposes and values shall be, and establishing the patterns which human behavior follows. In the one view, the institutions which make up the structure of society are human expedients; in the other view, man himself, except for his endowment of native powers and propensities, is the product of life in society. These views are variously distinguished, as individualistic and social, rational and genetic, atomistic and universalistic, mechanistic and organic. Each pair of names conveys a particular emphasis, or invokes a particular analogy, but each expresses the same fundamental contrast or opposition.

I see no satisfactory ground for any other position than that both of these opposed views take account of necessary aspects of the structure and the processes of organized society, and that neither view, taken by itself, is adequate. Yet the opposition between these two views has at one time and another divided social scientists into two warring camps. We have had, and still have, too much of what Mill, in his essay on Coleridge called "the noisy conflict of half-truths, angrily denying one another." Mill added these wise words: "All students of man and society who possess that first requisite for so difficult a study, a due sense of its difficulties, are aware that the besetting danger is not so much of embracing falsehood for truth, as of mistaking part of the truth for the whole." These are words for all inquirers in the field of the social sciences to remember. Our work is retarded and our intellectual energies are dissipated in useless quarrels because of our intolerance of methods and points of view other than our own. There are only two things of which we have a right to be intolerant: first, positive errors of fact or of inference; second, intolerance itself.

Since the two views of which I have spoken are really supplementary, one to the other, it follows that in the social sciences we must make room for two different general classes or types of investigation. In the first type we concern ourselves with certain aspects of the nature and the operations of a complicated social mechanism. We search for uniform and dependable relations that will help to explain the degree of order that is apparent in our social environment. In the second type of inquiries we seek to get an understanding, not of those general and dependable relations among things which we call "laws," but of specific events, particular institutions, and unique situations. We look for explanations of *differences,* of the *new* forms which our institutions and our activities assume from time to time.

What am I trying to emphasize is the distinction between the field of "science," in a narrow and strict sense, and the field of "history"—a distinction which many philosophers have recognized, but which has been curiously neglected in current American discussions of the problems and methods of the social sciences. Because both the natural and the social sciences, as commonly defined, extend over both fields, I prefer to follow Cournot in distinguishing, not between science and history, but between the abstract sciences and the historical sciences, between the sciences which have to do with those dependable abstract general relations which we call laws, and the sciences which deal with given situations or particular events in terms of their specific relations to situations and events which have preceded them.

Now it is a capital error to hold (with Thorstein Veblen and some of his followers) that the explanation of things in terms of their historical antecedents is in some special sense a scientific mode of explanation; that, as Veblen puts it, modern sciences are characteristically "evolutionary sciences," and concern themselves primarily with "unfolding sequences" and "cumulative causation." The truth is, of course, that the goal towards which the natural sciences are always pressing—even though it may be an unattainable goal—is the explanation of this world of changing and evolving forms and types of organization in terms of some simple and stable mechanism. Mathematical physics has not abdicated to descriptive genetics its place as the perfect type of science, and in a manner the ultimate type.

It is far from my purpose to belittle the importance of historical and genetic inquiries for the social sciences. I am merely trying to correct what seem to me to be prevalent misconceptions respecting the part they play in increasing our knowledge. I shall not even attempt to support the thesis that the unique and special character of historical events make "historical laws" impossible—for that thesis seems to depend partly on the way in which we define "history," and partly on what we mean by "law." There can be no doubt, however, that the sort of knowledge that we get from those historical inquiries which assume the institutional view of the structure of society, is not the sort of knowledge that we get from those inquiries into abstract general relations which assume a mechanistic or contractual view of the structure of society.

The mechanistic or contractual view of society is of necessity an instrumental view. The knowledge we get from researches into the nature of the general form of the economic relations that obtain in such a society is practical working knowledge, and can be formulated in working rules. It tells us what the general character of effects of a particular measure of control will be, what will happen if we pull a particular lever. Historical and genetic inquiries do not lead to working rules. They extend the range of our experience, they give us a better understanding of ourselves and of our possibilities and our limitations, they lead to new appraisals of our social arrangements, but they tell us little or nothing about means. At their best they add to our wisdom, to our judgment respecting what things are worth accomplishing, but they add little to the technical equipment required for successful accomplishment.

Researches into the "unfolding sequence" of institutional forms encounter the difficulty that the results they give are never scientifically verifiable. Wholly different interpretations of the course of history may have equally good credentials. A countless number of threads of continuity ramify backward into the past, and are woven together into what Maitland called the seamless web of history. Selection among them has to be made on the ground of present interests, and there is always the danger that it will be made on the ground of present predilections or present prejudices. Every account of the origins and the development of any of our contemporary institutions involves a revaluation of the past as well as of the present. (Consider, for example, the contrast between Alfred Marshall's summary account of what he calls "the growth of free industry and enterprise," and any one of the various socialistic accounts of the origins of what the socialists prefer to call "modern capitalism.")

Of course, the worker in this field cannot give free rein to his imagination, for he is controlled and limited by the facts. But his task is not merely to ascertain the facts: he has to select them, evaluate them, and relate them so that they will tell their story. His task is not merely one of research, but of esthetic construction as well. What he sees and reproduces will depend not only upon what there is to be seen, but upon what he looks for, and that will depend upon himself, his training, and his interests.

I do not mean to suggest that within the limits set by the facts the historical or genetic interpretation of our existing economic order depends solely upon the personal equation of the investigator. If he is an honest workman he will be controlled by the circumstance that all of the knowledge he gets, by whatever methods of inquiry, must fit together so as to be a consistent whole. In practice the lines between different views of the structure of economic society and different methods of inquiry cannot be drawn so sharply as I may have seemed to suggest. The economic theorist does not "deduce" his results from a few simple premises. Even when he controls his findings by using statistics, he works in the midst of a context of experience, and the system of general relations which constitutes his theory is empty of meaning unless it is consistent with that body of experience, and explains and organizes some part of it. Similarly, whatever new views of the structure of economic society we get by looking backward to its development must supplement and be consistent with that abstract and general view of economic relations which we call economic theory. Every economic theorist ought to be something of an historian, and every student of the development of economic institutions ought to be something of a theorist.

It may be that I have dwelt overlong on these preliminaries, but this has seemed to me to be an appropriate occasion for entering a protest against the fruitless quarrels of the methodological sects, against their intolerance, and against their pretensions to exclusive possession of the only right points of view and the only effective methods of research. We ought to welcome sound work in the field of economics—work that really contributes to our understanding of economic problems,—whatever its orientation and whatever method or technique it employs. The prerequisite to this degree of tolerance is the recognition of the fact that no one orientation can possibly lay bare the whole field of the economist's interests.

II

I hesitate to try to say towards what particular economic problems research could most profitably be directed just now. The difficulty is partly in the necessity of fitting research problems to the interests and equipment of the individual investigator and to the resources available to him, and partly in the rich diversity of important problems. Much depends, moreover, upon whether group research or individual research is contemplated.

Group research is an important and promising new development. It involves a common attack upon a particular problem or set of problems, by an organized body of investigators who apportion their work so as to get some of the advantages of the division of labor, and who may be able to turn over routine parts of their tasks to a corps of clerical assistants. This sort of organized research undoubtedly has advantages when what is wanted is a definite answer to a definite question, and when the question is one of fact. The task then is one of assembling materials and of putting them through appropriate technical processes so as to get a finished product. The form, though not the precise content, of the product is known in advance. The product must always be got by assembling facts in a particular way, or by relating them in a particular way. Doubtless research of this kind, directed toward a definite objective, will often yield important by-products; and doubtless, also, individual investigators who are engaged in this kind of research, will often hit upon new methods of dealing with their materials, or will find that new explanations and possible new inquiries come into their minds. But the specific goal of such research, as I have said, is a definite answer to a definite question of fact.

We have made hardly more than a beginning in organized group research in economics, and I look for a considerable increase in the number and importance of such undertakings. There are many important tasks which are beyond the powers and the resources of the individual investigator and which call for the cooperation of a number of investigators, with different capacities and different training. The advantages of this kind of organized cooperation are so obvious that I need not enlarge upon them. Its limitations are, or ought to be, equally obvious. These limitations are bound up with the fact that effective research is more than mere routine, more than a manufacturing process. The multiplication of research activities and the increase of endowments for research will not of themselves afford any assurance that there will be a corresponding increase of our understanding of the economic life of society. The assembling and systematic ordering of historical documents and statistical data is not enough. Willingness and industry are not enough. A perfected scientific technique is not enough. The really important thing is that research be directed towards the answering of significant questions, and it is hard to frame significant questions except in the light of definite hypotheses. Formulating questions and hypotheses is the first and most important task of the investigator.

Just because we can make a formal logical distinction between deduction and induction, we are prone to exaggerate the difference between deductive and inductive methods of inquiry. In the practical work of getting knowledge, we pass from a generalization to the facts and from the facts back to new generalizations in a way that blends deduction and induction. We begin, let us say, with a hypothesis—a tentative generalization. We then look into the facts, knowing that if the hypothesis is sound the facts we find within a certain range will not be inconsistent with it, and we determine our field of inquiry accordingly. This much is deduction. If the facts prove to be consistent with the hypothesis, our tentative deduction is transformed into an induction (or, as we say when we are testing some existing theorem, into a “verification”). If the facts are inconsistent with the hypothesis we cast about for a new hypothesis, for a generalization that brings the facts into some sort of orderly relation. In any really creative research, however modest in scale, there is this process of continuous give and take between the search for general relations and the scrutiny of particular details, between thinking and concrete observation.

But the process is generally not nearly so orderly and schematic as I have made it appear. Whatever the degree of perfection to which we have brought our methods of investigation, however conscientiously we try to conduct our inquiries so that our findings shall be impartial and objective, we have to proceed in the directions in which our interests and our questioning minds lead us, and we have to rely upon the subtle and obscure processes by which new hypotheses, new perceptions of possible relations among things, build themselves upon our minds as we bring new materials under survey.

Moreover, the materials which we consciously scrutinize, and which lie, perhaps, on the table before us, are only a part of the materials on which we levy. We work, as I have said, in a context of experience. Some of it may be formulated in general principles or in a consistent system of theory, some of it be organized in the form of orderly views of historical sequences. But a considerable part of it must be made up of that unsystematized knowledge of the relations of things which we get out of the immediate experience of life, as well as out of what we hear and what we read. The new knowledge which our researches yield has to be fitted into, not merely added onto, a comprehensive view of economic life, into which all our knowledge enters. This remains true regardless of whether, as in what we call deduction, we scrutinize such experience as is already at hand, and try to discern more clearly the systematic relations which run through it, or whether, as in what we call induction, we reach out for new experience and use it in testing and extending our knowledge.

In any case, the prerequisites to really successful research are significant questions and fruitful hypotheses. Successful research, of course, calls for industry and a command of the appropriate technical methods. But if it is to be anything more than mere fact-finding, it calls also for imagination, for the ability to see a problem and to devise hypotheses that are worth testing. Industry fortunately is not an uncommon virtue. Technique may be acquired. But imagination, and especially the kind of imagination that keeps its moorings, is rare. That is one reason why we ought to put our emphasis upon the individual investigator rather than upon a fixed program of research; why we should try to make it possible for the man with ideas to do the particular things he wants to do rather than the things we want to see done.

On the other hand, because men with really fruitful ideas are rare, and because there are a few men who combine a clear vision of some of the major economic problems with the ability to direct research effectively, it may sometimes be wise economy to make it possible for these exceptional men to control and direct the work of other investigators. In this way apprentice investigators may learn their trade while devoting themselves to more important tasks than they might have hit upon if left to find their problems for themselves. There are wastes in such arrangements, however. The energies which men of first-rate capacity give to directing the work of others might sometimes be employed more profitably in their own work. The largest contributions to economics have been, and, as I believe, always will be products of individual scholarship and research. There is no substitute for first-hand and intimate knowledge of one's own materials. Everyone who has undertaken a piece of original research knows how, even in the course of the routine handling of materials, the active mind notes at one point an apparent discrepancy, which calls for some recasting of hypotheses, while at another point it finds a suggestion of some hitherto unsuspected relation. The technical processes of research play a rôle auxiliary to that of the constructive and co-ordinating powers of the mind. Withdraw the investigator from immediate personal touch with his materials and, while you may increase his output, you are sure to impair the quality of his work.

In what I have just said I have had statistical research particularly in mind. In historical studies the case for individual research is, of course, even stronger. Constructive imagination counts for more, for the reason that in historical research it has a freer range. However objectively the investigator controls his findings by his materials, the task of appraising their significance, of relating them, and of fitting them together so that the finished product shall be history and not merely an enumeration of events, calls in a peculiar way for insight, imagination, and judgment. History is true in the way in which a picture is true; not in the way in which a physical law is true.

It will be apparent now, I trust, just why I hesitate to point to certain particular economic problems and say that those are the problems to which investigators could best devote their energies. The man who has hit upon a significant problem and who sees its significance has already taken a long step forward in research. Now there are of course a number of research problems in which I have an especial interest and which seem to me important. But I could not make their importance, as I see it, clear to anybody else, without a long preliminary account of the general setting of each problem and particularly of its relation to the other problems and the tentative hypotheses which are in my mind. I prefer, therefore, not to attempt to suggest specific problems, but to speak instead of certain inviting general types or fields of research.

I shall put my emphasis on what might be called neglected types of research, for there is no danger that the fields which just now are being more actively cultivated will escape anyone's attention. At any one time economists as a group have certain central interests in common. These central interests change as economic science advances, as the passing years bring new economic problems into the foreground, or when brilliant and challenging work by one economist attracts the attention of others. The war and the problems it bequeathed to the world have done more than anything else to determine the present central interests of economists. Problems in the fields of money and banking, of public finance, and of international trade have come into fresh prominence, as has commonly happened after long wars. Our war-time experience with government control of production and trade has helped to turn the attention of economists toward such questions as the future adequacy of the world's food supply, the distribution and control of supplies of raw materials, and the possibility of reducing wastes by introducing a larger element of conscious planning into the economic life of organized society. There is a new interest, also, in the nature of the national economic rivalries that make for war, and in ways of getting rid of them or controlling them.

Even before the war an increasing amount of attention was being given to the nature of the commoner types of industrial fluctuations, and, as everyone knows, a large number of investigators are now at work upon problems in that field. It is a field that lends itself particularly well to exploration with the aid of statistical methods. New materials are being put under survey, statistical technique is being perfected, and some of the intricate relations between the fluctuations of different series of economic phenomena are beginning to be perceived more clearly. This new interest in establishing empirical correlations between different economic variables with some approach to quantitative precision has been carried over into other fields. A promising beginning has been made, for example, in extracting from statistics a more precise knowledge of the relations between supply and demand and price. In general, there has been a notable growth of interest in determining empirical uniformities of relation that are sufficiently stable to afford some basis for prediction. We can safely count upon a steady increase of research activities in such fields. For this reason I shall say nothing about the alluring possibilities in these fields, or the unsolved problems with which they teem.

The neglected fields to which I want to call attention lie close about us on every hand. One only has to look to see great stores of unexplored materials, rich with the crude ore of knowledge, awaiting only patient delving and artful refining. More than once a promising young economist has complained to me that, where he lived and taught, the materials for research were inadequate. To one such I said recently that a set of census reports contained enough material to occupy his energies for the rest of his life. In our preoccupation with time series and correlation coefficients we are forgetting other aspects of what Sir Robert Giffen called "the utility of common statistics." A glance at the apportioning of space in some of the recent textbooks on statistics will suggest that we are unduly narrowing our notions of the field of profitable statistical inquiry.

The reports of our federal Census constitute, as a whole, the best general record that any country has of its economic life. Few economists use them, however, for other than reference purposes. I cannot think of any other research task that would promise surer or more valuable results than a systematic use of census materials in an inquiry into any one of an indefinitely large number of problems. Some years ago the Advisory Committee of the Bureau of the Census (a committee made up of representatives of the American Economic Association and the American Statistical Association) recommended that the Bureau undertake to publish a series of monographs, each to be the work of a competent scholar, in which census figures were to be analyzed and interpreted. The Bureau has now published seven of these monographs, and a few more are to follow. Anyone who looks through them will appreciate their importance and value. There is room for almost any number of studies of this kind, for the materials are well-nigh inexhaustible, and endowments for economic research would be wisely used in promoting them.

With the recent general growth of interest in population problems it is to be hoped that a larger number of investigators will occupy themselves with problems in the general field of demography—a field which American scholars, with a few conspicuous exceptions, have unaccountably neglected. It is to be hoped also that inquirers endowed with patience and insight and adequately trained in economic theory will make a first attempt to get from our successive censuses of manufacturers and agriculture a new comprehension of some of the forces that have been transforming the economic life of the United States. And there are large economic and social questions upon which careful studies of the changing importance of different ways of earning a living, as reported in the Census, would be certain to throw light. I shall not particularize further, but I think that it would be fairly easy to name as many as a hundred different important studies for which the reports of the federal Census would supply the more important part of the materials.

Of course there are other accumulating statistical records, imperfectly explored as yet by economists, which also provide inviting fields for research. I cannot dwell upon them or even specify them in detail. Many of them are by-products of the administrative work of governments. In the aggregate they cover a great variety of economic activities, and we should know more about a wide range of economic problems if investigators who have ideas and whose minds are open to new ones would address themselves to the study of these easily available materials.

Outside of the statistical field there is special need just now, I think, of careful and scholarly historical studies. There is always the temptation to paint on a large canvas, although painstaking work in miniature may have a larger permanent value. The man of genius may be able to see new sequences in the old materials that have been combed over by others, but the average investigator is surer of making his contribution if he gets hold of new materials, and uses them with the utmost care. This means, in practice, monographic work on a rigidly defined and limited subject. In my reference to the "average investigator" I did not mean to imply that the miniature may not be on every account as important an achievement as work on a larger scale. I think that I have learned more about some important aspects of the economic development of the Middle West from Professor B.H. Hibbard's history of agriculture in a single Wisconsin county than from any of the larger and more ambitious accounts. I do not see why the economic history of some American town or village should not be written in a way that would make it a contribution of the first importance to our understanding of the development of the economic life of the United States.

Many of our monographs on economic history have dealt with states. This is inevitable, of course, when the inquiry is concerned with the legislative or administrative aspects of some matter within the control of the state, such as taxation or banking or poor relief. Furthermore, some of the materials that are most easily available for the investigator are records of the law-making and administrative activities of states. But a state, after all, is an economic unit only in respect of matters of public economy. There is need for a series of concrete studies of various aspects of the economic development of carefully defined homogeneous regions and communities. There is also need for careful historical studies, not only of industries, but of individual business undertakings, of the careers of successful captains of industry and finance, of particular products or commodities, and of changing modes of consumption as well as of changing forms of production.

Perhaps I can make clear what is in my mind by saying that we need to supplement our statistical inquiries, which have to do with aggregate and averages, by historical studies in which the individual and concrete aspects of economic activities shall be emphasized. Or if I have not yet made my meaning clear, look again into the *Wealth of Nations,* and ask yourself how much of the power of that book comes from Adam Smith's ability to take a broad and general view, how much of it comes from the rich concreteness of his interests and his knowledge, and how far it is born of his rare capacity to see things in *both* their general and abstract and their immediate and concrete relations.

It may be objected that to discover and bring to light *new* knowledge by means of these researches, so that the past shall not only "live again" but shall disclose new aspects of itself, requires not only the methodical study of sources but a degree of creative genius. Now I have to grant, of course, that most historical writing is imitative, just as most literature is imitative, for the power to see things at once truly and as no one has seen them before is given to few men. But in historical research, as I have already suggested, the investigator of average ability has it in his power to make substantive contributions. It is necessary only that he should be insistent in his search for new and fresh materials, and that he should weigh and ponder those materials until they fall into place in some consistent account of the particular episode or series of episodes with which he is concerned.

In the history of banking for example, it is not so important to us as economists that we should know more about banking laws or about the administrative control of banking by public authorities, as it is that we should know more about the actual operations of banks and the actual uses of credit in representative communities in different parts of the country. The careful study of the records of some particular bank—and it need not be a large bank—over a period of years would establish a basis for an important and useful contribution to economics. There is opportunity for research of this kind, involving the gathering and careful scrutiny of new materials, in a large number of other fields.

I put special stress upon the requirement that some, at least, of the materials used shall be *new*. I mean that they should be not merely first-hand materials, "original sources," but new kinds of materials. If an investigator uses only materials of a kind that have often been exploited, he is likely to write, let us say, just another history of banking, of a familiar standardized sort, adding little or nothing to our understanding of the complicated structure of the economic world in which we live. Best of all, of course, is the capacity to ask really new and significant questions, and to attack one's materials with new and pregnant hypotheses in one's mind. But that capacity is rare, and any conscientious and thoughtful investigator is sure to find that new materials have a way of asking their own questions and of falling into new sets of relations. The goal of such research, of course, is not the mere accumulation of records, such as might delight the heart of the antiquarian, or even the disclosing of the "lessons of history," but rather a new and fresh perception of some of the different factors that have entered into the total economic situation and have helped to make that situation what it is.

I shall not attempt to particularize further, for I do not want to try to list a series of specific problems. The difficulty, as I have said, is not that problems are scarce, but that they press in upon us in such abundance and variety that selection is difficult. If I were to point to some of them and say that in my judgment those particular problems are the ones to which investigators could most profitably devote their energies, I might be diverting attention from other problems which equally deserve investigation. The important things are that the investigator concern himself with a real problem; that some goal be seen, however dimly, towards which his inquiries should converge; that he be openminded enough to permit new evidence to lead him in new direction; that he remember that successful economic research calls for thinking as well as for routine processes.

In an economist's opinion there could be, of course, few wiser uses of money than in endowing economic research. Yet we must remember that our first and most difficult task is that of developing trained economists, so that the interests and energies of an increasing number of really competent investigators may be turned towards the study of economic problems. And it is not sufficient that the investigator be a "trained" economist, for he must have, of course, a native endowment of judgment and insight. In fact, as I look back over what I have written up to this point, I find that much of what I have been trying to say has probably been prompted by my fear that we are in danger of expecting from systematic research more than systematic research can possibly give us. There appear to be some who think that through research, and research alone, the social sciences might be as completely revolutionized in the course of the twentieth century as the physical sciences were during the nineteenth. As a result, we are asked to believe, society would be in command of its own destinies, in the same way that, in a sense, man is getting a better command of the forces of physical nature. Now all this seems to rest upon a failure to see certain fundamental differences between the physical and social sciences, and especially upon a misapprehending of what we really mean when we speak of "controlling" the processes of nature. But I shall not enlarge upon that topic here. My concern is merely with the rôle allotted to research.

Now research of itself—as a mere formal process, I mean—never accomplished anything. Routine research will give a routine product. The only kind of research that really advances our working knowledge of the economic mechanism or that really adds to our understanding of the complex structure of our economic society, is research that serves as the tool of the active, questioning, and relating mind of the investigator. Let the individual investigator, therefore, if he has passed his apprenticeship and proved his quality, have all the encouragement, all the freedom, and all the assistance we can give. In short, in the actual administration of funds for economic research, let us put our emphasis upon the quality and promise of the investigator, and let us be careful not to hamper him by prescribing too narrowly just what he shall do and how he shall do it.

I recognize, of course, that the young investigator's interests are likely to be narrow, and that if he is put to work upon new problems, he will acquire new interests. For this and other reasons it appears to be desirable that a group of research workers should try to agree upon the general range of problems to which they are to devote their effort. As their studies proceed, a common field of interests will be created; new methods and new ideas will become common property; one good piece of work will set a standard for others. As a result of building up a group interest in a common range of problems in this natural way, the work of the group will have a natural unity, and will itself grow in a natural way. I should expect that the results would be better than if a fixed and detailed program of research were drawn up at the beginning, into which the work of each individual investigator would have to be fitted.

Some eighteenth-century philosophers professed to believe that all the imperfections of human society might be got rid of, if only men would put their trust in reason. The same faith is held today, but the word "reason" has been replaced by the word "research." One does not have to subscribe to this creed—and I cannot subscribe to it—in order to believe that the increase in the number of able men who are bringing the spirit of scientific inquiry into the study of economic problems gives us ground for hoping that we shall learn how to deal with those problems more effectively and more wisely. I say "more wisely" as well as more effectively, because I believe that social wisdom as well as a better knowledge of ways and means ought to be one of the goals of research in the social sciences.

2 English political economy

Economica, (1928) 8, No. 22 (March): 1–15. Inaugural lecture delivered at the London School of Economics and Political Science on October 11th, 1927.

Two thoughts contend for the uppermost place in the mind of an economist who turns aside from whatever special problems have been engaging his energies and steps away a little so as to get a general view of the present state of economic science.

In the first place there is an oppressive sense of the utter inadequacy of his own knowledge, and even of the knowledge which he could anywhere lay hold of. Economic science is still in its infancy, and despite the increasing numbers of its students, its growth is slow and uncertain. The world is asking more questions of the economist than it ever asked of him before, and it is asking its questions more insistently. There are old questions among them, and for these the economist has some sort of an answer, although not always an answer upon which economists would be agreed, and hardly ever a really complete and adequate answer. The new questions may not even reach his ears. So far as he does attend to these new questions the economist often finds that except in one respect—and I shall want to return later to that saving exception—he is little better equipped than a layman to deal with them effectively.

Then comes the more comfortable thought that, after all, the old problems about which economic science has something fairly definite and positive to say are exceedingly important problems. They are persistent problems, too, though they often recur in new forms and in new relations. There is much in the experience of the past twelve years from which the economist may draw confidence and courage. The economic problems of the war and of the period of readjustment were new in their magnitude and sometimes in their form. But in their essentials they were mostly old problems, such as economists had encountered before. Economists in general have held pretty definite opinions with respect to most of these problems. When—as happened frequently—their opinions were at variance with the policies of governments, in practically every instance the event has proved that the economists were right. Again and again, under the pressure of circumstances, governments have veered around until they