

# PROLEGOMENA TO RELATIVITY ECONOMICS

An Elementary Study in the Mechanics and Organics  
of  
An Expanding Economic Universe

BY

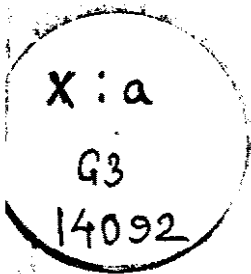
RALPH WILLIAM SOUTER, M. A.

*Lecturer in Economics in Columbia University*

"The element of Time . . . is the centre of the chief difficulty of almost every economic problem."—*Alfred Marshall.*

"Time, which is—Heaven knows what!"—*A. S. Eddington.*

SUBMITTED IN PARTIAL FULFILMENT OF THE REQUIREMENTS  
FOR THE DEGREE OF DOCTOR OF PHILOSOPHY  
IN THE  
FACULTY OF POLITICAL SCIENCE  
COLUMBIA UNIVERSITY



NEW YORK  
1933

Dhananjayrao Gadgil Library



GIPE-PUNE-014092

# PROLEGOMENA TO RELATIVITY ECONOMICS

An Elementary Study in the Mechanics and Organics  
of  
An Expanding Economic Universe

BY

RALPH WILLIAM SOUTER, M. A.

*Lecturer in Economics in Columbia University*

"The element of Time . . . is the centre of the chief difficulty of almost every economic problem."—*Alfred Marshall.*

"Time, which is—Heaven knows what!"—*A. S. Eddington.*

SUBMITTED IN PARTIAL FULFILMENT OF THE REQUIREMENTS  
FOR THE DEGREE OF DOCTOR OF PHILOSOPHY  
IN THE  
FACULTY OF POLITICAL SCIENCE  
COLUMBIA UNIVERSITY

No. 391 in the "Studies in History, Economics and  
Public Law" of Columbia University.

NEW YORK

1933

X:a  
G3  
14092

**COPYRIGHT, 1933**

**BY**

**COLUMBIA UNIVERSITY PRESS**

**PRINTED IN THE UNITED STATES OF AMERICA**

**"The next fifty years will make my poor *Principles*, with a lot  
of poor comrades, into waste paper."**

**ALFRED MARSHALL  
in 1920**

**TO ALL THOSE  
WHO  
GUILLESSLY BELIEVE THEMSELVES  
TO HAVE RENDERED THIS SERVICE  
DURING  
THE DECADE 1910-1930  
THIS BOOK  
IS  
SOLICITOUSLY DEDICATED**

## PREFACE

THE form of the following essay is in large part to be explained by the fact that almost all of it has now been in existence for nearly two years as one section of a much larger work the scope and arrangement of which have been several times revised, and the remaining sections of which are still not ready for publication. In its latest design, this larger work contained two other main sections, to which the present essay, under the title "On Moving General Economic Equilibrium", was to have been introductory. The first of these other sections takes up the complementary concept of "Maximum Net Social Satisfaction through Time"; and, by way of an immanent criticism of certain prominent concepts of current normative economics, seeks to display some important implications for economics of the elementary philosophical truism that purely positive quantitative concepts cannot of themselves be made to yield a rational social *norm*—though such a norm of course may (or rather must) have elaborate quantitative aspects. The last of these unpublished sections seeks to apply the methodological results thus reached to certain important recent developments in the search for a *normative* "dynamics" of monetary or credit control.

The justification (if any be needed) for embarking on so comprehensive an investigation rests partly on the belief that the whole field is capable of unification by means of a single methodological concept; partly on the very strong conviction that, in these days of alarmingly complicated "specialisation", a broad methodological study of the kind described might, despite (or because of) its generality, render worthwhile service in a number of special fields—particularly in

the field of monetary theory, where portentous activity, not lacking in rather severe friction, has now been going forward for a number of years.

The misgivings I should have had in withholding still longer the publication of the present essay until these other sections had attained to somewhat more satisfactory form, would have been even greater than those I now have in publishing it separately. Some of the more elementary reasons for believing that the time has now come for economic science to realise, as never before, that the Mecca of the economist lies in economic philosophy even more than in economic biology, and that economics should today regard an exclusionist Positivism as the most dangerous of all foes within its own household, are briefly and incompletely stated in an article entitled " ' The Nature and Significance of Economic Science ' in Recent Discussion ", appearing in the May 1933 issue of *The Quarterly Journal of Economics*. The possible applications of these considerations to the search for a normative monetary " dynamics " must await a more auspicious occasion.

In this first attempt to present, even in bald outline, one segment of much of my own thinking up to the present, I desire to make some intellectual acknowledgments of a personal kind. By far my most profound intellectual debt is to my father, to whose early and authoritative inculcation of the paramount duty to question all mere authority (including his own) and think for oneself, I undoubtedly owe a deplorably hearty (though I trust discriminating) appetite for " orthodoxy ". My sense of grateful obligation grows continually to my first teachers—to the late Dr. Francis Wallace Dunlop, formerly professor of mental and moral philosophy in the University of Otago, Dunedin, New Zealand, who taught me to beware of " gnosticism " in philosophy, and

therefore, by implication, in everything else; and to the late Archdeacon Robert Augustus Woodthorpe, formerly professor of economics in the University of Otago, who was accustomed to boast with genial humour, as "the proudest in his life", the day when he—"an Anglican priest!"—became the occupant of a chair in "a Scottish university!", and to whose noble catholicity of scholarship I owe an invaluable and ineradicable inferiority complex. Both alike, despite some difference of opinion concerning the exact significance of Hegel's philosophy in the historical development of the human mind, laboured with astounding patience, with unflinching goodhumour, and with enviable discrimination, to instil into the egregious youth of a young community some perception of the ineffable distinction between the best and the merely second best. To Professor Wesley C. Mitchell, who, if I have ever been tempted to forget in economics the lesson I learned in philosophy, has helped to keep me on the right track by means of that most potent of all educational instruments—the force of a distinguished personal example—I owe a deep and lasting intellectual debt.

I am profoundly indebted, in many and various ways, to former teachers and present colleagues at Columbia University. My especial thanks are due, not only to Professor Mitchell, but also to Professor James W. Angell and to Professor J. M. Clark, for helpful discussion and criticism on various points. For the general scheme of thought and interpretation which I have sought to outline in the following pages, mine is the sole responsibility.



# CONTENTS

## I

|  | PAGE |
|--|------|
| Introductory. A broad methodological examination will be made of the concepts of economic statics, economic dynamics, and economic biology, as an aid to an understanding of the notion of "moving general economic equilibrium". Methodological investigations of this kind may be powerful instruments of constructive work in "mature" sciences ..... | 1-3  |

## II

|  |      |
|--|------|
| Stationary general equilibrium has no "ideal" or normative implications. It requires to be carefully distinguished from the (conceptually unsatisfactory) notion of "perfect competition". It does not imply absence of "error". It is merely a balance of equal and opposite <i>dynamic</i> (or organic) forces | 4-10 |
|--|------|

## III

|   |       |
|---|-------|
| The "element of Time" cannot be excluded from Statics. A stationary general equilibrium, if realistically conceived, is not identical from moment to moment: it is a "cycle", occupying Time. And it may contain waste and unemployment ..... | 11-13 |
|---|-------|

## IV

|   |       |
|---|-------|
| The notion of constructing a self-subsistent comprehensive Statics as a prelude to Dynamics is a delusion. There is no radical difference between a "statical hypothesis" and "the" Stationary State. And there is no unique or "absolute" conception of "the" Stationary State. Moreover, to eliminate the "arbitrary" element from stationary <i>general</i> equilibrium would require prior exhaustion of Dynamics; and, if it were possible, would be <i>identical</i> with valid prediction, based on exhaustive knowledge, of the inevitable historical emergence of such a state ..... | 14-20 |
|---|-------|

## V

- The distinction between "static adjustment" and "dynamic change" is purely relative to the standpoint and assumptions of the scientific observer; and depends on his frame of reference. The Marshallian long period supply curve is not empirical but theoretical; and, so far from being *inappropriately* "dynamic", it is, when "theoretically perfect", the basic *static*, because general equilibrium, curve. Even when "theoretically perfect", it may be negatively inclined for a time. And, in a dynamic economy, a "theoretically perfect" long period curve is a contradiction in terms. The Marshallian method is essentially a flexible organon for the analysis of an economic "space-time" (price-quantity-time) *continuum* ..... 21-32

## VI

- Attempts to dichotomise economic mechanics (statics and dynamics) from economic biology are also misconceived. The concrete economic process is one of organic evolution, of which economic mechanics is simply the abstract *kinetics*. The progressive scientific elaboration of this abstract apparatus is the *fruit* of progressive realistic exploration—psychological, sociological, technological. The Marshallian graphical apparatus is in essence a dynamic mechanics of organic evolution ..... 33-39

## VII

- Marshall's principle of the relativity of the distinction between "elasticity" and "shift" in the case of Supply is applied to Demand analysis. The *irrelevance* of the "psychological principle" of "diminishing utility" to the long period (basic *static*) Demand curve is shown, and the Empty Demand Boxes are revealed. The long period *Law of Increasing Utility* is briefly discussed; and the internal and external economies of Supply analysis are found to have their Demand counterparts in internal and external *emerging benefits*. The diffused operation of "increasing returns" in a "closed" system is of course a technical "supply" phenomenon; but *Total Wanting* is a variable and may expand absolutely. Absolute "expansion" of a "closed" economy is a diffused organic property of the Whole—not a mechanical resultant of the interaction of falsely reified

CONTENTS

xiii

PAGE

elements. And the same is true of stationary general equilibrium. The real weakness of traditional demand analysis, frequently alleged to be "hedonistic", is its disintegrating mechanical atomism, which really vitiates the pseudo-"statics" of which it is the cherished bulwark ..... 40-64

VIII

The problem of the *short period* Demand curve. Difficulties of the deceptive phrase "absence of change in character and tastes". The "instantaneous static" demand curve, defined with "precision", proves to be a "theoretically perfect" *long period* curve. An alternately expanding and contracting psychic Universe—a plaything for the Austrian School. Functional differentiation of the Short Demand Period a task for Economic Psychology. The Pursuit of Precision and the Non-Conformist Conscience ..... 65-79

IX

General formulation of the theory of the thorough-going relativity of the distinction between "elasticity" and "shift" .... 80-91

X

"Marshall's theory of economic equilibrium" has *not*, even yet, been "absorbed". The pathological character of the recent Static Reaction in Economic Theory, and the profound and far-reaching implications for twentieth century economics of the doctrine that "the Mecca of the economist lies in economic biology rather than in economic dynamics", are inadequately indicated by means of brief illustrations of the trend of methodology in Psychology, in Biology, in Physics, and in Mathematics ..... 92-131

XI

The "holistic" character of economic change. New inventions and their utilisation. "Maximum Net Advantage" as universal social gravity. Social and political "interference". Freedom and Determinism in economics. The discussion is elementary and provisional: it anticipates systematic investigation of Maximum Net Social Satisfaction through Time 132-148

## XII

The expression "moving general economic equilibrium" may perhaps be given various connotations. It naturally suggests some implications and problems whose systematic treatment properly belongs to the study of Maximum Net Social Satisfaction through Time. Employed with reference to the concepts of positivistic, mechanistic "statics", the term "moving general equilibrium" should mean simply the actual historical continuum. Disguised and unavowed attempts to give normative significance to the concept are sentimental. Even when the cruder blunders of sentimental Liberalism are avoided, the concept remains treacherous: it offers subtle temptations to misconceive the problem of "social control", and to ignore or underrate the practical difficulties and social costs of realising vague and unexamined "norms" by means of devices of manipulative mechanics. The "purely positive" study, to which this Essay has been confined, requires for *its own* completion a complementary investigation of the concept of Maximum Net Social Satisfaction through Time .. 149-171

## I

"No science can be more secure than the unconscious metaphysics which tacitly it presupposes."

*A. N. Whitehead.*

A REALLY exhaustive critical analysis of the concept of economic equilibrium would be too elaborate for our present purpose. As the following discussion will amply illustrate, methodological discussion cannot be carried very far before its own further fruitful development begins to call insistently for investigation of specific scientific problems of fact. But while no sharp line can be drawn between the methodology of a science and the science itself, a wide difference in emphasis of treatment is possible. The emphasis of the present discussion is methodological. Many interesting avenues of concrete scientific inquiry must therefore perforce be left unexplored: they can at most receive only such inconclusive passing comment as may be helpful to the development of the general theme.

It seems sometimes to be supposed, even today, that methodological discussion belongs only to the infancy of a science; and that "maturity" is marked by an exclusive concentration on "real problems" and an aversion to fruitless preoccupation with merely "metaphysical" considerations. "The preoccupation of science is then the search for simple statements which in their joint effect will express everything of interest concerning the observed recurrences. This is the whole tale of science, *that* and nothing more. It is the great Positivist doctrine, largely developed in the first half of the nineteenth century, and ever since growing in influence."<sup>1</sup>

<sup>1</sup> A. N. Whitehead, *Adventures of Ideas*, p. 147.

It is vital for students of the human sciences to realise that the indict-

Among our ancestors, this popular view was supported by pointing in illustration to the natural sciences, and especially to physics. But—unless, perhaps, the term methodological investigation be invalidly circumscribed to denote merely jejune discussions of the “induction versus deduction” type, and to exclude philosophical criticism of the clarity, intelligibility and rational coherence of the fundamental, and for the most part uncritically accepted, concepts on which the whole superstructure of a science is reared—this mainstay of an old-fashioned Positivism has now collapsed. Even a science which has attained to the degree of “maturity” reached by Newtonian physics is, we now know, not immune from the disconcerting experience of having its ultimate truisms dissolve beneath its feet, and of being compelled to face the task of drastic theoretical reconstruction in terms of more adequate and more fundamental categories. It is no doubt possible to argue, rather meaninglessly, as to whether such fundamental advances in a “mature” science are achieved as a by-product of “realistic” investigation; or whether they are initiated as a result of original and constructive “theoretical” reflection. But this is merely the “induction versus deduction” type of controversy on another level.<sup>2</sup>

ment of an exclusionist Positivism, which would segregate “science” from “metaphysics”, is *methodological*; and that the shallow methodology which, in the natural sciences, would fence off “science” from “metaphysics” in the erroneous belief that scientific development can proceed without reliance upon metaphysical criticism of basic concepts, is *identical* with the shallow methodology which, in the human sciences, induces the illusion that “positive” studies can be kept segregated from “normative” or “value” problems. This matter will be explored, and its economic implications developed, in a subsequent study.

<sup>2</sup>It is like discussing whether modern relativity physics owes its emergence to such empirical investigations as the Michelson-Morley experiment; or to the work of the pure mathematicians who had previously laid the foundations for interpreting the disconcerting results of this experiment.

The heritage of fallacy and confusion which Positivism has bequeathed to the human sciences is more unwholesome and more inexcusable than anything with which it has encumbered modern thought in the natural sciences; and there is here a rich field, promising almost indefinite increasing returns, for critical and constructive scholarship. The present essay is a modest attempt to clear the ground as a preliminary to the cultivation of one small corner of this field. It is a mere beginning. Its purpose is, by surveying in broad outline the main implications of the basic economic concept of "equilibrium"—statically, dynamically and organically conceived—, to prepare the way for a complementary study of the *normative* concept of Maximum Net Social Satisfaction through Time: a concept whose systematic explication and development is an inescapable prerequisite of any real understanding of "the economic problem" in its comprehensive sense.

A general analysis of the concept of "moving general economic equilibrium" calls for examination in turn of a number of related concepts and problems: chief among which are "stationary equilibrium"; the relation of "statics" to "dynamics"; the relation of the "mechanical" to the "organic" or "evolutionary." These will be taken up in turn. Since the deeper purpose of this essay is to provide an introduction to a later attempt to seek a satisfactory general view of the *relationship* of "moving equilibrium" to "maximum satisfaction through Time", all reference to this relationship cannot be excluded from the present discussion. But what mention there is of this problem will here be of a negative character only: more positive and constructive treatment will be attempted on another occasion.

## II

"What I take to be a Static state is . . . a position of rest due to the equivalence of opposing forces which tend to produce motion."

*Alfred Marshall.*

To begin with, it is desirable—in view of the powerful lingering influence of the traditional identification of "equilibrium" with "maximum satisfaction"—to insist at once that there is nothing "ideal" about a condition of stationary equilibrium. The feeling that it contains at least *some* suggestion of "desirability" is difficult to dissipate entirely, because it is frequently thought of as a condition which, given the continuance of the "underlying conditions" necessary to produce and maintain it, represents the *consummation* of the "best" position attainable by each member of the system in view of the limitations imposed upon him by a like attainment by all the other members. Really thorough examination of this notion transcends the limited scope of this essay. But two points should be noted. First, this *detailed* conception of a "stationary state" is not the only possible one: in the more realistic types of "equilibrium" employed by Marshall and Pigou, a whole industry may be in "stationary equilibrium" while its constituent firms are in a continual flux of individual growth and decline.<sup>8</sup> Secondly, quite apart from this consideration (and

<sup>8</sup> And consider, with regard to a whole economy, or "the stationary state": "This state obtains its name from the fact that in it the general conditions of production and consumption, of distribution and exchange remain motionless; but yet it is full of movement; for it is a mode of life. The average age of the population may be stationary; but each individual is growing up from youth towards his prime, or downwards to old age. The average size of the business firms may be stationary; but at any moment almost every business is either rising or falling. The



quite apart, too, from problems of discrepancy between marginal private and marginal social net product), it should be evident that stationary equilibrium does not, of itself, imply the absence of "error" even in the atomistic, individualistic sense. Even if we conceive the equilibrium in the detailed sense, all that is necessary is that whatever "errors" are inherent in the system should be constant. Each individual may then be said to occupy the "best" position attainable, the actual state of his capacity, knowledge, foresight, etc. being assumed.

But even this carefully qualified way of expressing the matter is liable to (and has in fact frequently led to) the very gravest misunderstanding and misconstruction. It therefore becomes necessary to emphasise that such a conception of "equilibrium" does not provide, even in the very slightest degree, any intellectual foundation of any kind for philosophical acceptance of "laissez-faire," or "individualism," as an "economically" (that is, socially) *desirable* form of organisation. The very definite fallacy involved in supposing that it does can be exposed in a variety of ways.

In the first place, it may justly be characterised as a flagrant example of *the fallacy of composition*. The fact that, under an "individualistic" regime, a certain narrow and highly unrealistic form of "stationary equilibrium" involves universal individual attainment to the "best" position attainable by each member of the economy, *on the assumption* that the economy is an individualistic one, does not even begin to deal with the question whether the aggregate of individuals might not, on the whole, be better off under

average value of grain may be stationary; but the current price fluctuates with successive harvest flows. The study of such fluctuations about a centre of rest is really a dynamical problem, though the simplest form of it is always included in the study of a "stationary state," and indeed affords the chief inducement to the fiction of such a state." *Memorials of Alfred Marshall*, p. 315. See below, section iv.

some other form of organisation. To seek to defend "individualism" by means of the concept of "equilibrium" is to beg the whole issue.

It is, however, more effective and more adequate to characterise the intellectual confusion involved as an illustration of *the static fallacy*. For while, as Professor J. M. Clark has acutely pointed out,<sup>4</sup> it may plausibly be argued that "the strongest basis for individualism is not the intelligence of individuals and their irrevocable devotion to the pursuit of their own self-interest, but rather their stupidity and their susceptibility to moral suggestion"; to this the "realist" may plausibly reply that, human beings being what they are, this does not in any way alter the fact that "individualism" is the "best" thing under the circumstances. Any "static" discussion of the problem of the relative merits of different forms of social organisation inevitably ends in intellectual stale-mate. The problem must be discussed in developmental terms. We may therefore approach a step nearer to the true view of the matter by noticing that Professor Pigou's cautious—and perhaps ambiguous—utterance (by no means lacking in *practical* justification) that there is a "preliminary presumption bred of the doctrine of maximum satisfaction" against governmental interference with individualistic conditions,<sup>5</sup> is peculiarly liable to methodological misuse—especially if it be extended to apply also to "interference" by less comprehensive organs of collective action than the State. It requires to be emphasised that if this "preliminary presumption" be conceived in abstract or *a priori* terms, or as following either from the concept of "equilibrium" or from the concept of "maximum satisfaction," there is again involved both the fallacy of composition and the static fallacy. Economic science must

<sup>4</sup> *The Trend of Economics*, p. 97.

<sup>5</sup> *Industrial Fluctuations*, p. 225.

recognise, as clearly as political philosophy has now long recognised, that in a developing society there is no *a priori* preliminary presumption against new forms of collective action as such. The truly "scientific" attitude on this matter, proper to economic science on its strictly "positive" side, was clearly pointed out by Marshall:\*

It is often said that the modern forms of industrial life are distinguished from the earlier by being more competitive. But this account is not quite satisfactory. The strict meaning of competition seems to be the racing of one person against another, with special reference to bidding for the sale or purchase of anything. This kind of racing is no doubt both more intense and more widely extended than it used to be: but it is only a secondary, and one might almost say, an accidental consequence from the fundamental characteristics of modern industrial life. . . . There is no one term that will express these characteristics adequately. . . . They may and often do cause people to compete with one another; but on the other hand they may tend, and just now indeed they are tending, in the direction of co-operation and combination of all kinds good and evil. . . . We may conclude then that the term 'competition' is not well suited to describe the special characteristics of industrial life in the modern age. We need a term that does not imply any moral qualities, whether good or evil, but which indicates the undisputed fact that modern business and industry are characterised by more self-reliant habits, more forethought, more deliberate and free choice. There is not any one term adequate for this purpose: but Freedom of Industry and Enterprise, or more shortly, Economic Freedom, points in the right direction. . . . Of course this deliberate and free choice may lead to a certain departure from individual freedom when cooperation or combination seems to offer the best route to the desired end. The questions how far these deliberate forms of association are likely to destroy the freedom in which they had their origin and how

\* *Principles* (8th ed.), pp. 5-10.

far they are likely to be conducive to the public weal, lie beyond the scope of the present volume.

There is yet another, but very closely similar, way in which it is sometimes sought to give to the concept of "equilibrium" some penumbra of desirability. This is in terms of the concept of so-called "perfect" competition. But here again the static fallacy is involved, together, in all probability, with a number of other confusions of thought. This matter cannot be adequately discussed at this stage. Its very complex ramifications should be much clearer at the close of the ensuing discussion. But three points may be briefly noted now:

(a) "Equilibrium" does not necessarily imply "perfect" competition. It may be true that, on certain very simple assumptions as to the motives of action of competitors, a "determinate (static) solution" of problems of value and price may then also require the assumption of a certain *type* of "competition," sometimes described (I think most unfortunately) as "free" or "perfect." But there is no reason to suppose that the actual occurrence of (static) "equilibrium" must necessarily be confined to situations in which such conditions are present.

(b) Such a situation, if general, is of course not identical with maximum net social satisfaction.

(c) From a dynamic standpoint, and one which has regard to the comprehensive functioning of the economy as a whole through Time, this particular conception of "competition" has, to say the least, no clear claim to the title of "perfect" competition. To ignore still deeper difficulties (which lead inevitably straight into "metaphysics"), any thorough-going conception of the "maximum" functioning of "competition" must include the notion of a "maximum efficiency" of functioning, for the economy as a whole, of the Principle of Substitution. It cannot be accepted as self-

evident that the type of competition characterised by the presence in individual industries of a very large number of small competitors is most conducive to this. And there are, indeed, strong grounds for holding that certain forms of knowledge, both of particular markets and of the conditions of the economy as a whole, which are generally admitted to be necessary to the elusive concept of "perfect competition," are almost of necessity circumscribed by the presence of this form of competition.<sup>7</sup>

<sup>7</sup> It can perhaps best be labelled "simple competition".

More elaborate discussion of this problem belongs to a subsequent study. In my view, it is a delusion to suppose the concept of "perfect competition" *capable* of consistent and comprehensive explication in positivistic terms. For example, mere specific instances of "friction", "inertia", "lack of mobility", and so on, cannot be unambiguously described as "imperfections" of "competition" in the comprehensive and evolutionary sense implicit in Marshall's Principle of Substitution. For one thing, they may prove, on deeper analysis, to be necessary conditions of less "friction", less "inertia", more "mobility", etc., elsewhere in the system. But it can be shown, I believe, that there is then inevitably raised a problem of comparative *evaluation* essentially similar to, and intricately related with, that of inter-personal comparison of "costs" and "satisfactions"; and that any hope that resort to the "dimension" of philosophical evaluation can be satisfactorily evaded by trying to conceive, quantitatively and positivistically, of a *net* "minimum" of "friction" (or other form of "imperfection") for the economy as a *whole* is quite illusory.

For another thing, the ultimate logical consequence of any notion that more "perfect" competition is to be conceived as essentially a "speeding up" of "adjustments" would seem to be a limiting concept of "perfect competition" as realisable only in an indescribable economic universe in which "adjustments" require no time at all, and in which, consequently, everything happens at once.

Still another aspect of the matter is the well-known truth that "constructive cooperation" may promote "more efficient competition". Briefly, I hold that the notions of "perfect competition" and "perfect cooperation" are *identical* concepts: and that both alike are abstractions which cannot be given either concrete content or rational intelligibility until they have been organically related to a valid and integrated economic *philosophy*. In physical science, "the quest of the absolute leads into the

Any systematic investigation of these various considerations inevitably involves, not only the whole field of economic dynamics, but the entire realm of normative economics as well. As here briefly mentioned, they serve to indicate the rigorous scientific accuracy of the Marshallian conception of stationary equilibrium. An economic system is in stationary general equilibrium simply because its "underlying conditions" happen to be such that "the forces making for change" — or "the forces of progress and decay" — are *equal and opposite*.<sup>8</sup>

four-dimensional world". In economic science, the same quest leads into the realm of philosophical values. Cf. below, p. 121, note.

<sup>8</sup> *Memorials of Alfred Marshall*, p. 415. But see below, section iv.

## III

"It obstinately refuses to be conceived as an instantaneous fact."

*A. N. Whitehead.*

Hypothetical stationary equilibrium, if conceived in terms even remotely "realistic," is *not* an *instantaneous* condition (identical from moment to moment), but a round or cycle of activities *occupying Time*—which round or cycle is identically repeated. Those who are tempted to regard a stationary state as identical from instant to instant have only to ponder the indubitable existence of the seasons.<sup>9</sup>

Nor is there any reason why, within each static "cycle," there should be continuous employment of total available resources. To suppose that there is, is to confuse "perfect fluidity" or "perfect mobility" with "equilibrium." There appears to be no reason, for example, why such a society should not "suffer" from seasonal unemployment of labor. So long as the net advantages of irregular occupations outweighed as a whole those of alternative regular occupations open to the individuals concerned; and so long as the costs of whatever inter-seasonal employment was available to them

<sup>9</sup> They should also consider the implications of Marshall's doctrine that in such a state normal and *average* price are identical (*Principles*, pp. 347, 367-8, 372, 810-11). Marshall (p. 367) qualifies his statement that in a stationary state "there would be no distinction between long period and short period normal value" by adding: "at all events if we supposed that in that monotonous world the harvests themselves were uniform." In the absence of "perfect" mobility, "perfect" foresight, "perfect" competition from a stationary state, identical market price fluctuations throughout successive years as a result of a continuing constant degree of imperfect foresight, etc. would not be inconsistent with the continuance (repetition) of stationary equilibrium extended in Time to the extent of a year; and long period normal price would then be (weighted) average in Time as well as in Space.

outweighed for them its advantages on the only terms on which it was available; this seasonal unemployment might repeat itself identically even though, *caeteris paribus*, the seasonally unemployed would "rather" be employed continuously—on unavailable terms. Moreover, "dislocation" of this kind might consist in part only of *potentially removable* "waste," and in part also of conditions which would remain when all the inherent possibilities of the situation (increased knowledge, improved education, keener grasp of "true self-interest") had been exploited to the full extent of the native endowments of the population.

It seems almost impious to suggest that, under still more "realistic" conditions, "the" stationary state might have to occupy a *term* of years; that similar elements of "imperfection" might exist on a larger scale and become explicit at definite points within the "cycle"; and that we might therefore have a stationary state with "cyclical" unemployment of labour and resources. Yet there can be no doubt that, by parity of reasoning, such a situation is consistently conceivable. I shall not here attempt to prove—what I suspect—that it may be more consistent with "realism" than its negative.<sup>10</sup>

<sup>10</sup> As a first approximation, we have only to suppose, for example, that the renewal of certain forms of fixed capital is not perfectly continuous; that the surplus labour discharged from the construction industries contemplated, after their period of maximum activity, takes an appreciable time to become absorbed in its supplementary occupations (if any); and that it is not, on balance, to the interest, real or discerned, of any competent parties to smooth out the irregularities. It is not axiomatic that the entrepreneurs in the construction industries concerned would, or could, enforce perfect smoothness. Their own technical exigencies might favour some irregularity; those of their customers might enforce it; while even the conditions of the labour supply, despite some unemployment, might encourage it. All this is conceivable in a stationary state whose constituent personnel and business units are indestructible and constant. In a state in which individuals were dying and being born at equal rates, and in which the "forces of progress and



The foregoing propositions (in II and III) are preparatory to our next, which is, I believe, crucial to a just and intelligible interpretation of the essence of the Marshallian method. It should be noted, however, that the argument of the ensuing sections is not dependent for its validity on any more thorough-going development than has here been attempted of the proposition that "the" stationary state may consist of "cycles" occupying Time. The main purpose so far has been to remove certain common misunderstandings from our path.

decay" at work in individual firms were "equal and opposite", even more comprehensive forms of "disharmony" and "dislocation" are quite conceivable. While it would be distressingly tedious, I do not think it would be impossible to set up in detail plausible conditions of this kind. The really fantastic element arises in connexion with the dovetailing that is necessary to ensure that the system is really *stationary*, so that it repeats itself identically: and this is equally fantastic in the case of a stationary state that is identical from day to day.

In anticipation of later discussion, it may be added that, if we do not adopt the device of visualising the (purely hypothetical) stationary state as returning to its precise point of initiation after a definite term of years, we leave the way open to baffling and inexhaustible possibilities of *new simultaneous combinations* which may, for aught we have assumed to the contrary, themselves be the conditions of the emergence of so-called "dynamic changes".

## IV

"I could no more write one book about my Statical state, and another about my Dynamical state, than I could write one book about a yacht moving three miles an hour through the water which was running against it, and another about a yacht moving through the still water at 5 miles an hour."

*Alfred Marshall.*

It must next be urged that the notion of constructing a *self-subsistent*, "comprehensive" Statics as a *prelude* to Dynamics, though a pertinacious one in economics, and revived quite recently, is erroneous, and based on a fundamental misunderstanding as to what "statics" really is.<sup>11</sup> The evolution of economic thought has today reached a

<sup>11</sup> See Lionel Robbins, "On a Certain Ambiguity in the Conception of Stationary Equilibrium", *Economic Journal*, June 1930. See especially the passage in which the writer asserts that he would "be prepared to argue, in spite of Marshall himself, that it ("the" stationary state) underlies much of what there is of validity in the results achieved by the 'statical method'"; and, secondly, that "the stationary state itself is superior to the more limited conceptions." But a charitable interpretation of the former statement reveals no opportunity for reckless defiance; while the second assertion, in so far as it is susceptible of any definite interpretation at all, is, as I shall show, definitely false.

That Professor Robbins has failed to grasp the essential significance of Marshall's work is even more clearly shown by his assertion (*loc. cit.*, p. 194) that the essential concern of economic theory to date has been with "the stationary state and static laws". This was not Marshall's view: "This volume is concerned mainly with normal conditions; and these are sometimes described as Statical. But, in the opinion of the present writer, the problem of normal value belongs to economic Dynamics: partly because Statics is really but a branch of Dynamics, and partly because all suggestions as to economic rest, of which the hypothesis of a Stationary state is the chief, are merely provisional, used only to illustrate particular steps in the argument, and to be thrown aside when that is done." *Principles*, p. 366, note.

stage when this issue has become of vital importance, and one on which neither compromise nor good-natured tolerance is any longer possible. It must be clearly and unambiguously laid down that continued aggressive urging of the claims of a segregated and self-subsistent Stationary State, as over against the method which, in Marshall's actual language,<sup>12</sup> is "*not quite accurately* called the statical method," has today become the hallmark of second-rate economic thinking. It is the insignia of pre-Marshallian thought.

The whole of this essay is devoted to the detailed substantiation of this wholesome if harsh assertion. But it will be useful, in this present section, to try to formulate in advance, in generalised language, the methodological *essence* of a fallacy which, when it is tenaciously clung to and "rationalised" by a perverse logic into a whole "system" of thought, threatens economic science itself with collapse into futility. If, therefore, the "metaphysics"-shy reader has difficulty over this preliminary generalised statement, I can only ask him to suspend judgment and return to it later.

It is extremely difficult to expose clearly and briefly the essential confusion of thought involved in supposing that "the" stationary state somehow differs radically from a "statical hypothesis." And the difficulty is immensely increased by the fact that those who suppose there *is* such a difference imagine themselves to possess some occult backing from Mathematics. They seek to express this sanction with the words "complete mutual determination." But the point is that "complete mutual determination" itself results in *stationary* equilibrium only under certain *assumed* conditions as to the specific nature of the whole complex of functions involved. Thus we have then to inquire under what conditions these assumed conditions would be actual: and this

<sup>12</sup> *Principles*, p. 369. Italics mine.

leads either into an "infinite regress" or—what is practically much the same thing—into an exhaustive investigation of the whole realm of dynamics.

It may be useful to approach this matter gradually by way of a well-known passage from Marshall.<sup>18</sup>

The terms Statics and Dynamics [he said] are imported into economics from physics; and some discussions about them among economists have seemed to imply that statics and dynamics are distinct branches of physics. But of course they are not. The modern mathematician is familiar with the notion that dynamics include statics. If he can solve a problem dynamically, he seldom cares to solve it statically also. To get the statical solution from the dynamical, all that is needed is to make the relative velocities of the things under study equal to zero, and thus reduce them to *relative rest*. But the statical solution has claims of its own. It is simpler than the dynamical; it may afford useful preparation and training for the more difficult dynamical solution; and it may be the first step towards a provisional and partial solution in problems so complex that a complete dynamical solution is beyond our attainment.

The term "relative rest" calls for notice: for it plays an important *role* in the so-called stationary state of the economist. 'Absolute rest' is an unmeaning term; statical problems deal with relative rest. This fact is perhaps more familiar than he knows to "the man in the train". . . . Experience . . . has taught him to look out for the disruptive dynamical element that is latent in the apparently peaceful statical problem.

This passage brings out clearly (a) the fact that a "stationary" equilibrium, in the most general sense of the term, is itself a particular case of the interaction of *dynamic* forces; (b) the *relativity* of the concepts of "motion" and "rest"; and (c) the consequent fact that a *system* of forces can, as a *whole*, be strictly described as "stationary" or "moving"

<sup>18</sup> *Memorials*, p. 312.

only when conceived as part of some larger, more comprehensive system, relatively to some element in which the whole sub-system is "stationary" or "moving".

Now it would seem to follow that, strictly speaking, within any "closed", *comprehensive* system, the various relative "movements", just because they are relative, must necessarily, so to say, cancel out."<sup>14</sup> Hence the suggestion naturally presents itself that we should seek the "precise" significance of the distinction (increasingly popular in recent years) between a *general* "moving" and a *general* "stationary" equilibrium in terms of absolute constancy and absolute increase respectively.<sup>15</sup> That the notion of a constant aggregate magnitude has been vaguely prominent in attempts to conceive a "precise statics" will hardly be denied. And Professor F. H. Knight<sup>16</sup> has recently spoken of Marshall's "refusal" to "separate sharply, productive changes under static conditions, i.e. changes compensated by inverse changes

<sup>14</sup> For example, even if population and national dividend were both (I ignore possible difficulties about the latter) increasing absolutely, but at different rates, the "movement" of the first relatively to the second would be "cancelled" by the opposite "movement" of the second relatively to the first. The ancients puzzled over a Universe which, while—like the economist's Stationary State—"full of movement", could not itself "move". Eddington—*The Expanding Universe*, pp. 125-130—after remarking that "expansion is a relative term", insists that "even if our standards are held responsible for the expanding of the universe, they cannot be held responsible for its bursting". This is the end-result of real "one-way" *development*. I hope, therefore, that I shall not seem to have fallen, on the title-page, into mere fallacious mis-comparison; and that the venturesome analogy will be found to have broad philosophical justification. See section xii.

<sup>15</sup> The terms "rest" and "motion" are relative concepts. But "*Numer* (of discrete individuals) is absolute." Eddington, *The Nature of the Physical World*, p. 23.

<sup>16</sup> *Statik und Dynamik: Zur Frage der mechanischen Analogie in den wirtschaftswissenschaften*, *Zeitschrift für Nationalökonomie*, August 1930, p. 16, n. 2.

in other industries, from changes which affect the social aggregate." From this standpoint, we might be tempted to say that the "social aggregate" was increasing absolutely either when population and dividend were both exhibiting absolute increase, or when, if either was diminishing absolutely at a certain rate, the other was increasing absolutely at a rate sufficient on balance to swell the absolute "aggregate". And we might then add that the "social aggregate" would be a constant either when both population and dividend remained absolutely constant, or when an absolute increase in either was exactly balanced by an absolute decrease in the other.

To any such procedure it would presumably be objected that we had no right to combine such "incommensurables" to form an "aggregate". But, if that be so, what significance are we now to attach to the term "general"? Are we now free to apply it to the constancy of some *one* component in the economy, provided that component be taken in its full extension throughout the entire economy? If the effect on "numbers" of a steady expansion in the volume of trade under the influence of improved technique is precisely offset by a steady rise in the standard of life, so that total population remains constant, is this one possible type of "stationary general" equilibrium? In that case there is no one unique case of "general" stationary equilibrium.

It is also open to us, however, in trying to "define" stationary "general" equilibrium, to divert our main emphasis from the notion of absolute constancy—the result of "equal and opposite" forces—to the notion of "relative rest." We can then hold "general" stationary equilibrium to be a condition in which *all* the constituent absolute quantities in the economy are either absolutely constant, or are varying together proportionately—so that they one and all fail to "move" *relatively* to one another. This is in fact

what Marshall, when conceiving "a Stationary state," did with regard to individual *industries*. But it is now to be noted (a) that this conception is *combined* by him with that of simultaneous "opposite movements" in the magnitudes of the constituent *firms* of these individual industries, with no fanatical attempt to carry through the application of the concept of "relative rest" to every conceivable constituent "element" in the economy; (b) that this particular combination was clearly selected by him for pragmatic reasons: it provided the "simplest form" of "dynamical" fluctuation about a centre (or centres) of "rest"; and (c) that the concept is "provisional", and merely a "fiction": to be used simply to "illustrate a particular step in the argument", and "to be thrown aside when that is done." If, for any reason, it became useful to select, as the "unit" or "element" which remained in a position of "rest" relatively to other "units", a *group* of industries, whose constituent industries then underwent "equal and opposite" changes relatively to one another, I cannot think Marshall would have repudiated this on principle, or on grounds of "pure methodology".

We may therefore avoid any further verbal manipulation of these abstract concepts, and pass to what is the really essential point of this Section: *all* such stationary equilibria—partial or "general"—are of necessity merely illustrative—mere "static hypotheses". The truth of this assertion is not impaired by recognition that, *if* stationary "general" equilibrium—of one kind or another—*actually came into existence*, it would do so by virtue of the fact that the "forces of progress and decay" (relevant to its definition as "stationary") had somehow come to be "equal and opposite;" so that the situation would be one particular (static) kind of "complete mutual determination"—imperfectly describable by analogy in terms of Marshall's illustration of "the balls in the bowl." But to embark on the task of *exhaustively* de-

termining the conditions under which such a stationary general equilibrium "would" supervene is *identical* with embarking on the task of ascertaining whether, in fact, it is *going to* supervene.<sup>17</sup> Hasty critics of Marshall, misled by their desire for greater precision and comprehensiveness in the study of "the closed system" into supposing that the first step towards this goal is the preliminary construction of a comprehensive and self-subsistent "Statics," and therefore into making facile psycho-analytic charges of "temperamental aversion" from the "heroic abstraction" of "the" stationary state,<sup>18</sup> urgently require to realise that Marshall was, to use a phrase of his own, "loose with system" in this matter; and, as we shall see, remarkably well endowed with the wisdom of the serpent.

<sup>17</sup> Since we are dealing with a *closed system*, then, if we refuse to stoop to the device of arbitrarily "holding" any of its elements constant, we have to determine, not merely the conditions making for equilibrium with regard to each in turn on certain *hypotheses* with regard to the others, but the conditions under which the entire closed system *will* achieve stationary equilibrium. The task thus becomes identical in economics with quasi-physical speculations about the "running down" of the Universe. It will be obvious that, unless the Universe is actually going to run down, and unless we can in advance obtain such perfect knowledge of its entire structure and implications as to foresee the inevitable, it is inherently impossible to set up the "conditions" under which it "would" run down *except* by interpolating *somewhere* an "arbitrary statical hypothesis".

<sup>18</sup> Robbins, *loc. cit.*, and *Economic Journal*, September 1928, p. 395. Cf. Marshall, *Principles*, Appendix D, on the *appropriate* use of "heroic abstractions".



## V

"I cannot conceive of any Static state, which resembles the real world closely enough to form a subject of profitable study, and in which the notion of change is set aside even for an instant."

*Alfred Marshall.*

I have now to advance a proposition the defence of which, and the development of the implications of which, will occupy the entire remainder of this essay. This proposition is nowhere advanced by Marshall in these very words: moreover, I believe that its systematic, comprehensive and consistent elaboration reveals the one fundamental analytical error in the structure of the *Principles*. Nevertheless, Marshall has himself quite unambiguously asserted it in substance, not once but many times; and, unless I am completely mistaken, it is the clue to the mighty underlying scheme of thought on which the whole structure of the *Principles* is based. This proposition is as follows.

It is naive and inadequate to try to *segregate* "static adjustments" from "dynamic changes": in the last resort, this distinction is *purely relative to the standpoint and assumptions of the scientific "observer."* It has *no meaning* except in terms of the "frame of reference" which, in the employment of the method "not quite accurately called the statical method," the economist has *temporarily* selected as "the centre" of the economic universe.<sup>10</sup>

The frantic attempts of much recent economic dialectic to effect a rigorous dichotomy between a *timeless* "statics"

<sup>10</sup> "By that method we fix our minds on some central point: we suppose it for the time to be reduced to a *stationary* state; and we then study in relation to it the forces that affect the things by which it is surrounded, and any tendency there may be to equilibrium of these forces". *Principles*, p. 369. "And see especially Preface to the Eighth Edition.

and a "dynamics" which deals (in some manner never clearly specified) with the passage of this two-dimensional abstraction through Time is in striking and instructive contrast with the recent momentous developments in physics which have resulted from a rigorous investigation of the conception of a Space-Time Continuum.<sup>20</sup> To suggest that an economic "space-time" (price-quantity-time) continuum—essentially similar as regards the scientific attitude of mind involved, though of course not necessarily parallel in detail—

<sup>20</sup> A word of warning is here in place against objections to "arguing from analogy". The carrying over of *concrete images* from one science to another in what may be called the Spencerian manner is admittedly dangerous, and, though often helpful, requires the check of a fine and just discrimination. But the present argument is not, in strict logic, one from analogy at all. It is today generally recognised that the tendency of modern pure physical mechanics is to eliminate from its purview all "metaphysical" entities, and to deal only with the static and dynamic relationships of *quantities* that result from the nature and interaction of such entities. It seeks, in short, to become simply a *kinetics* or motion study of the processes of the physical world. Hence its final philosophical message seems to be: "*Something unknown is doing we don't know what*" (Eddington, *op. cit.*, p. 291). In this signification the term "mechanics" is independent of the ultimate nature of the phenomena with which it deals. It is therefore highly inappropriate, and inimical to scientific self-respect, that economists should continue to regard the terms "economic statics", "economic dynamics", "economic mechanics" as "mere analogies." (Schumpeter blandly confesses that the terms "statics" and "dynamics" are, in his own usage, "misnomers"; and even suggests that "it would, perhaps, be best to drop the terms altogether." *Economic Journal*, September, 1928, p. 374, n.). The term "economic mechanics" possesses coordinate rank with the term "physical mechanics"; and simply implies, as we shall see, the kinetics of concrete organic growth and adaptation. The same is true of general categories within the field of "mechanics" (I refer not only to "statics" and "dynamics", but also to "friction", "inertia" and so on) in so far as these also are taken to mean simply certain quantitative aspects of kinetic interaction in Space and Time.

It may be added here that similar considerations apply to certain so-called "biological analogies". "Biology" is the science of "living organisms". Cf. below, p. 36, note.

underlies the Marshallian analysis and requires a similar systematic explication and development, is not to propose some esoteric "mixing up" of economic "statics" with economic "dynamics." "Your protest," says Eddington,<sup>21</sup> "in the name of commonsense against a mixing of time and space is a feeling which I desire to encourage. Time and space ought to be separated. The current representation of the enduring world as a three-dimensional space leaping from instant to instant through time is an *unsuccessful* attempt to separate them." I propose to show that attempts in economics to eliminate Time from "statics," and to visualise the enduring economic world as a two-dimensional abstraction of timeless demand and supply curves "leaping from instant to instant through time," involves an essentially similar inherent confusion. It seems to me that only failure to perceive that this proposition is really implicit in the Marshallian analysis can account for the remarkable fact that Marshallian critics should at once protest that the structure of the *Principles* as a whole is merely "statical" or "quasi-statical" and at the same time take exception to the Marshallian long period supply curves as "dynamic"—yet without doing anything effective themselves to clear up the mystery.<sup>22</sup>

<sup>21</sup> *Op. cit.*, p. 37.

<sup>22</sup> Schumpeter ("The Instability of Capitalism", *Economic Journal*, September, 1928, especially p. 368, n.) misquotes and distorts what he calls Marshall's "protests against the limitations of the static apparatus" by omitting the essential phrase "in which the notion of change is set aside even for an instant" from his rendering of Marshall's letter to J. B. Clark. This phrase contra-distinguishes the type of "static state" which does *not* "form a subject of profitable study" from the type which *may*: one in which the dynamic forces at work are equal and opposite—as distinct from one which cannot be otherwise than "static" because all possible causes of change have been *eliminated* by hypothesis. At the same time Schumpeter objects to the long period normal supply curve as including "dynamic" elements, and protests that "there is only one theoretic supply curve; and it slopes upwards in all cases."

This matter of the valid interpretation of the Marshallian demand and supply curves is of vital and central importance to an understanding of the difficulties associated with the notion of a "moving general equilibrium;" and to this task of interpretation I therefore now turn.

That the long period supply curve (whether positively or negatively inclined) is *not* a "historical" or "statistical" or "empirical" curve is immediately evident from the fact that Marshall defines it as representing *hypothetical* conditions which (given time) *would* emerge—in the absence of "substantive new inventions," which are an indubitable part of the *actual* historical sequence.<sup>23</sup> But the futility of branding the long period supply curve as "historical"—or even as *inappropriately* "dynamic"—is still more conclusively evidenced by the fact that it is in a very real, and most significant, sense *static*: it represents a series of positions of *general*, as distinct from "partial," *equilibrium*. This is really the crux of the whole matter; yet its lucid exposition is a matter of extreme difficulty. An attempt can best be made by beginning with a crucial passage from the *Principles*.<sup>24</sup>

Of course the periods required to adapt the several factors of production to the demand may be very different; the number of skilled compositors, for instance, cannot be increased nearly

<sup>23</sup> *Principles*, p. 460. Schumpeter (*loc. cit.*, p. 378, n.) objects that this principle of theoretical segregation "cuts up a homogeneous phenomenon"—in the last resort "new inventions" themselves are a form of "external economy". I know no evidence to support the suggestion that Marshall (of all economists) regarded "new inventions" as merely sporadic, and out of all relation to their milieu. I suggest that his principle of segregation here was conceived by him as essentially *practical* in character, and in terms of what might be foreseen by a competent observer. Consider his comments, *loc. cit.*, pp. 460-461.

<sup>24</sup> P. 379, n. Italics mine.

as fast as the supply of type and printing-presses. And this cause alone would prevent any rigid division being made between long and short periods. *But in fact a theoretically perfect long period* must give time enough to enable not only the factors of production of the commodity to be adjusted to the demand, but also the factors of production of those factors of production to be adjusted and so on; and this, *when carried to its logical consequences, will be found to involve the supposition of a stationary state of industry.*<sup>25</sup>

The meaning of this can I think be expressed as follows. A long period curve depicts a series of alternative long-period responses of supply conditions to alternative postulated changes in normal demand. *Each* of these long-period responses represents, "in strict theory," the functional relationship—of quantity supplied and price—which, on each hypothesis regarding the initial stimulus from demand, *would* come to prevail *after all the readjustments throughout the entire system were completed*: for this is, by definition, the rigorous meaning of the term "long period." Thus, although in a "stationary state," into which no disturbances were introduced, there might be no *overt* distinction between the long and the short period, nevertheless the demand and supply curves whose points of intersection gave

<sup>25</sup> He adds: "Some such assumption is indeed unconsciously implied in many popular renderings of Ricardo's theory of value, if not in his own versions of it; and *it is to this cause more than any other* that we must attribute *that simplicity and sharpness of outline*, from which the economic doctrines in fashion in the first half of this century derived some of their seductive charm, *as well as most of whatever tendency they may have to lead to false practical conclusions.*" Italics mine. For reasons which I shall elaborate, this is to my mind perhaps the clearest indication in the whole of the *Principles* that Marshall, in his customary handling of the "long period", is being "loose with system". His long period supply curve, as customarily drawn and discussed, is *designedly* "theoretically imperfect": because, in a dynamic economy, a "theoretically perfect" long period is a *contradiction in terms*.

the normal (and actual average) prices stably prevailing, would of necessity be equivalent to *long period* curves—since they would in fact be combined in and express a state of *general* equilibrium.

Once this fundamental point is clearly grasped, it becomes evident what deplorable confusion of mind is involved, and what a calamitous retrograde step in the very elements of economic analysis is proposed, when it is suggested that we should regard short period (or, worse, “instantaneous”) curves, which are really simply *partial* equilibrium curves, as *the* true “theoretical” or “static” curves, and disown the long-period curve, which is really the *basic* “static” curve, as inappropriately “dynamic.”<sup>26</sup>

Now there is no reason whatever why some of the long period supply curves implicit in a stationary state should

<sup>26</sup> A closely related confusion of mind is apparent in the frequently expressed notion that the “Neo-Classical” apparatus of demand and supply curves makes inappropriate use of the device of *ceteris paribus*, and that this supposed clumsy inadequacy is disposed of by the methods of the “Mathematical School”, with its technical methods for handling “complete mutual interdependence” of functions. It is apparently supposed that the employment of *ceteris paribus* in the definition of even the long period Marshallian curve involves the assumption that the different functional relationships of price and quantity which it depicts are theoretically determined on the assumption that no “shifts” occur in any of the demand or supply curves for other commodities in the system. But it is quite clear that this cannot be the case; since the comprehensive readjustment throughout the entire system which must, by definition, occur in connexion with any “long period” adjustment, involves, among other things, *consequent* “shifts” in the positions of other curves—except under *really* stable “static” conditions of a very particular kind (see below, pp. 67-68). What is “impounded in *ceteris paribus*” is the class of “substantive” (as distinct from consequent) “shifts” in the positions of the various curves. The excluded changes are thus in fact identical with those which are equally excluded from a mathematical formulation of the conditions of general stationary equilibrium. As Marshall liked to point out, the device of *ceteris paribus*, properly employed, is inseparable from, because it is identical with, scientific analysis itself.

The problem of “substantive changes” is discussed below.

not be negatively inclined from their point of intersection with the demand curve. All that is necessary to "stable" equilibrium, as this term is commonly employed with regard to the mode of intersection of the curves, is that the corresponding demand curves should decline even more steeply. What *can* be argued is that, if the curves could be drawn "throughout their whole length," such declining supply curves must, on the assumption that the economy as a whole remains "stationary" throughout the successive increases in the scale of output of the commodities in question, rise again *before long*—so that a "theoretically perfect" declining long period supply curve should be drawn with an upward bend not far below the point of intersection. For, so long as the system as a whole (and therefore the total available resources) *remained* "stationary," a *continuing* increase in the supply of any commodity must, for reasons that need hardly be elaborated here, *fairly quickly* encounter increasing costs. But I can see no reason whatever to justify the extreme and quite unproved assumption that, in a continuing stationary general equilibrium, increasing returns could not operate *at all* in response to a shift in demand.

But this apparent gain in precision over the Marshallian curve<sup>27</sup> is little more than apparent. If, in such a stationary

<sup>27</sup> Marshall makes no mention of an ultimate upward bend in his declining long period supply curve. Indeed, he suggests (Appendix H, p. 807, n.) that "stable" equilibrium is ultimately to be looked for, under conditions of increasing returns, in the fact that the supply curve must ultimately fall less steeply than the demand curve. This is strictly true: a "stable" intersection of the curves does not require any upward bend. But his failure to point out the need for the latter when the economy *as a closed whole* is assumed to continue stationary (in accordance with his own definition of a "theoretically perfect" long period) suggests that, in drawing his long period curve for practical purposes, he did not habitually contemplate his own ultimately rigorous definition of "the long period". His reason for this has already been indicated, and is further discussed below.

state, there "spontaneously" appeared an increase in the demand for a commodity subject to decreasing costs (with, necessarily, a corresponding decline in demand for other commodities), *under what conditions* would this "stationary" equilibrium *continue* to be "stationary" *as a whole?*<sup>28</sup> If the structure contained two commodities only, with identical declining supply curves of uniform elasticity, then a given shift in demand from one commodity to the other would reduce the supply of the first by just as much as it increased the supply of the second; and the system might then be said to continue in stationary equilibrium *in the proper Marshallian sense of that term which implies that the dynamic forces at work are equal and opposite.*<sup>29</sup>

<sup>28</sup> It may be suggested that this involves a distinct extension of the concept of "equal and opposite" change beyond any meaning contemplated by Marshall himself; since it involves the idea of equal and opposite shifts in the *long period* normal equilibrium prices of different commodities: whereas Marshall habitually spoke of long period normal price as constant in a stationary state. It may therefore be well to state here (what will become more apparent later) that the suggestion in the text is properly to be regarded as an expository device which, if ill-treated, may easily lead to confusion for which I must disclaim responsibility. Any difficulty in this regard must arise out of misunderstanding of the function of the (quite customary) hypothesis of an initial "spontaneous" change. Strictly, so-called "dynamic" changes within the organic process are not to be regarded as "spontaneous", but as conditioned by the process itself. In a really stationary state long period normal price would be constant if we selected the appropriate unit of time (see III *supra*) within which to average price fluctuations.

<sup>29</sup> I have reason to fear at this point the objection that I am illegitimately assuming the "reversibility" of a long period curve. Of course I am not. Unless the point of intersection on a declining long period supply curve occurs at the precise point at which the decline begins, the curve itself contains the assumption that a restriction in the scale of the industry will cause *some* increase in costs. I know of no reason for assuming that this is everywhere or even usually impossible on material grounds. "Irreversibility" simply means that, if the initial equilibrium condition is disturbed (by an increase in demand) the adjustment response on the long period supply curve will itself involve developments



Much more complicated combinations on the same principle could be hypothetically constructed. *But they would be mere hypotheses.* It is equally open to us to suppose that demand is withdrawn from a commodity subject to *increasing* costs, and diverted to one subject to decreasing costs; in which case the new "stationary" equilibrium, when (or rather *if*) it emerged, might be expected (in the absence of counteracting assumptions regarding the disutility of effort) to contain a larger volume of production than before—and quite conceivably a larger population. But again the structure and its implications can be indefinitely complicated, and we have a system which, while each intersecting pair of

which require a "re-drawing" of the curve. Schumpeter (*Economic Journal, loc. cit.*, pp. 368-9, n.) claims that Marshall's insistence on the "irreversibility" of the long period supply curve amounts to virtual acceptance of his (Schumpeter's) criticism of the theoretical validity of the long period curve. But this is a complete misapprehension. "Irreversibility" has, directly, nothing whatever to do with the theoretical definition of the curve. It turns on a question of fact—whether the entire closed economy is or is not a *truly* "stable" stationary general equilibrium in the Marshallian sense that the dynamic forces which operate within it produce changes which are equal and opposite, and, in addition, whether these changes, after they have occurred, are in fact capable of being completely reversed. In a *dynamic* economy *all* curves of both demand and supply are "irreversible". See Marshall, *Principles*, p. 808: "This is true whether the production of the commodity obeys the law of diminishing or increasing return; but it is of special importance in the latter case, because the fact that the production does obey this law *proves* that its increase leads to great improvements in organization." Italics mine. It is true that Marshall's exposition does not always illustrate what has been called "the modern journalistic craving for immediate intelligibility"; but reflection shows that his meaning here is both profound and accurate. In my view, however, it requires, as will more fully appear below, bold extension into a universal generalisation applicable to all the curves of a dynamic economy. (Even in a truly "stable" stationary general equilibrium of the kind contemplated in the text, the changes need not be completely *reversible*—which merely amounts to saying that, under certain assumptions of *attempted* "reverse" action, the system would no longer remain stationary. See below, p. 70, n.)

its constituent demand and supply curves is in "stable" equilibrium, is yet itself *as a whole* only in "stable" stationary general equilibrium so long as nothing happens to disturb it! Once it is disturbed, it turns out to have concealed (precariously imprisoned, as it were) within itself an "expanding" economic universe. Any prediction as to its ultimate return to stationary general equilibrium is (except on the hypothesis of a fantastic degree of knowledge of its hidden "implications") quite impossible. *For the process, once under way, involves a continuous series of "shifts" in the "positions" of the long-period curves themselves. It is for this reason that, in such a dynamic economy, a "theoretically perfect" long period is a contradiction in terms.*<sup>20</sup>

In terms of this conception of a dynamic deterministic continuum, Marshall's term "substantive" changes, if employed to characterise *either* shifts in the long-period curve under consideration *or* those shifts in other long-period curves which are *not* merely incidental to the long-period "response" under consideration,<sup>21</sup> is preferable to "spontaneous" (employed by Pigou and others); because the term "spontaneous" suggests *causeless* "creative evolution" of a kind inherently unamenable to scientific analysis; whereas the term "substantive" merely makes the correct suggestion that the changes to which it is attached are to be regarded as *outside the chain of consequences contemplated in the hypothesis under consideration*. But the term "substantive *new inventions*," which Marshall employs to characterise those changes which are assumed absent when a given long period curve is drawn,<sup>22</sup> invites an undue restriction of conception of the *range* of factors which may

<sup>20</sup> Cf. Marshall, p. 379, n., and, for a particularly vivid picture of the resulting complications as a whole, pp. 346-7.

<sup>21</sup> See above, p. 26, n. Also below, pp. 58-59; and sec. xi.

<sup>22</sup> *Principles*, p. 460.

occasion "substantive" shifts. As will presently appear, these shifts are in the last resort *the product of the whole process of evolution.*

To this difficult conception it will be more convenient to return later. It should already be fairly clear, however, that, in such a continuous dynamic process, the distinction between "dynamic change" and "static adjustment" is necessarily a purely relative one. "Static adjustments" in one part of the system may be the *inducements* to "dynamic change" in another part—which dynamic change is thus itself an "adjustment". It involves not merely oversimplification, but actual distortion, of the real nature of the problem to take stationary equilibrium as a datum which requires no explanation (being already "explained" by so-called "static theory"), and then to cast round for various radically- and qualitatively-distinct forms of "dynamic change"—which must then of necessity be conceived as intrusions from "without"—and investigate the supposed process by which stationary general equilibrium is "re-established." The conditions of stationary general equilibrium do not exist except on the assumption of a complicated and miraculous set of special functional interrelationships among the various curves. If the various "elasticities" of demand and supply are in fact *not* of this remarkable and miraculous kind, then the various curves *already* contain within themselves the implications of an *indefinite* "dynamic" development. No intelligent person would assert that this fact invalidates the scientific method "not quite accurately called the statical method:" what it *does* invalidate is the naive picture of the economic process as one of continual "static adjustment" towards a discrete series of stationary *general* equilibria which continually replace one another in Time under the mysterious influence of a separate set of qualitatively distinct phenomena—"dynamic

changes." What it does expose is the half-baked claim on behalf of the "superiority" of a comprehensive and self-subsistent "statics" regarded as prior to and independent of "dynamics"; and the spuriousness of the charge that Marshall's profound and subtle handling of this difficult and fundamental intellectual problem involves the "shirking" of analytical rigour. In such a dynamic economy, the conditions represented by any given long period supply curve not only are never completely realised, but are inherently incapable of "theoretically perfect" formulation, because the very process of realisation itself alters the conditions to be realised.<sup>28</sup>

The general outlines of what I believe to be a valid interpretation of Marshall's profound and rigorously scientific conception of "the relation of statics to dynamics" should now be fairly clear. Before proceeding to scrutinise a little more closely some of the problems involved in his conception of "substantive" change, it will now be necessary, first, to make a provisional inquiry into the general problem of the relation of "mechanics" (statics and dynamics) to what I may perhaps be forgiven for calling "organics."

<sup>28</sup> Concrete illustrations might be multiplied indefinitely within the limits of our present knowledge. A rise in wages, for example, as a result of increased demand for labour, either in general or in a particular occupation, may, through the influence of unaccustomed affluence in inducing a rise in standard of life and in economic efficiency, alter the normal supply schedule for the labour before "adjustment" brings about "restoration" of equilibrium on the "old" supply curve. (Cf. Marshall, pp. 807-809). This *conception* permeates the whole of Marshall's work. For an important discussion of the same principle, in which illuminating significance is attached to Adam Smith's concept of "division of labour", regarded as a *diffused* form of the organic principle in economic evolution, see Allyn A. Young, "Increasing Returns and Economic Progress", *Economic Journal*, December, 1928.

The general concept of diffusion in its relation to "substantive" changes is discussed in sections viii and xi, below.

## VI

"The modern economic organism is vertebrate."

*Alfred Marshall.*

It is not always easy to tell whether those who try to segregate two distinct and separate categories of economic phenomena are endeavouring to do so in terms of a false dichotomy between "statics" and "dynamics"; or in terms of another dichotomy, also false, between "mechanics" and the "evolutionary" or "historical."

Discussions among economists of these two basic methodological problems have lately become wrapped in obscurities which are rendered the more profound (a) by a frequent tendency to regard the two distinctions as identical; (b) by an apparent inability, even when the latter alone is seemingly in the forefront of the discussion, to decide just what the writer himself means by it. It seems possible to unravel at least five leading concepts: the concepts of "dynamic" (or "organic"?) change as radically distinct from mere "static adjustment" in that it is (a) *disruptive* of equilibrium; (b) *discontinuous* in its operation; (c) *spontaneous* in its origin—as distinct from being "mechanically caused"; (d) "*qualitative*" in the sense of effecting changes in the nature of the *structure* of the economy; (e) "*qualitative*" in the sense of involving, on the part of individuals, high intelligence and "*creative*" mental activity as opposed to mere "automatic" or "mechanical response". By way of corollary, it is frequently implied that "static adjustments" are capable of *prediction*—or, if not of positive prediction, at least of hypothetical prediction, in the sense of formulation in terms of "exact law": while "dynamic changes" intrinsically are not. And it seems to be almost universally

assumed that all these distinctions correspond to the graphical distinction between a "static" curve and a shift in the curve.

So far as I know, no systematic attempt is ever made to ascertain whether all these concepts are merely various aspects of one fundamental line of cleavage in economic phenomena; or whether some of them, at least, are not mutually incompatible as simultaneous bases for a single sweeping dichotomy. It would run counter to the mode of development which I have chosen for this complicated discussion, to take up each of these concepts in turn and discuss them individually and in their relations to each of the others. Each one of these distinctions is treated at an appropriate place in the ensuing discussion. At this point, for the purposes of a preliminary inquiry into the relationship of "mechanics" to "organics," it will be convenient to give specific attention only to the concepts of *spontaneity* and *creative emergence*.<sup>84</sup>

Even if it were possible to classify the "responses" of economic agents into two classes, the one involving much intelligence, invention, foresight and so on, and the other little, these classes would still not correspond<sup>85</sup> to the distinction between "adjustments" on a so-called "static" curve and "dynamic shifts" in the curves. The "creative" ingenuity, foresight, enterprise, and so on, implicitly *em-*

<sup>84</sup> (a) The notion of disruption should already be suspect as a result of the foregoing discussion of a dynamic *continuum*. It derives its apparent importance from the false segregation of a supposedly comprehensive "statics" from "dynamics". See also pp. 135-137, below.

(b) On discontinuity, see below, sec. vii, especially p. 142, n.

(d) On qualitative structural changes, see sec. xi.

On the question of prediction, see p. 143 and sec. xi generally.

On the distinction between "elasticity" and "shift", see, further, secs. vii and ix.

<sup>85</sup> As Schumpeter, for example, appears to suppose.

*bodied and assumed* in a so-called "static" supply curve for a particular kind of labor, for example, may greatly exceed in scope and quality the corresponding characteristics exercised by the "entrepreneur" who inaugurates for the first time a new automobile service based on the "drive yourself" principle.<sup>85</sup> Conversely, many "innovations" of this and more elaborate types can be "predicted," under given conditions, by a well-informed and competent person, with every whit as much scientific precision as regards probability, nature, extent and rate of "emergence," as can the nature, extent and speed of the "elasticity response" of a certain kind of labor to a change in its demand price.<sup>87</sup>

Every element in the dynamic (or organic) continuum must be regarded as either (a) *conditioned* in its "emergence" by the complex of remaining elements—whether "atomistically" or otherwise;<sup>88</sup> (b) a response to the stimulus of something acting from *outside* the system; or (c) *unconditioned* "emergent" or "creative" evolution. Where the last alternative is introduced, science ends; since it is the function of science to seek the "necessary" conditions of such emergences, and it is the essence of scientific progress to extend the scope of such "explanations," in terms of "conditions," to phenomena previously regarded as unaccountable "innovations." From a scientific standpoint, therefore, we are necessarily confined to alternatives (a) and (b).

Now from a standpoint which regards all "economic" *change* as, by definition, the result of "interference" from "outside," economic science is, by definition, a theory of stationary equilibrium, and can never by any conceivable

<sup>85</sup> Cf. Schumpeter, *loc. cit.*

<sup>87</sup> It might be better to put the matter the other way round, and to say that the latter are as unpredictable as the former. See below, sec. xi.

<sup>88</sup> See below, secs. vii and viii.

means become anything other than the presentation of the supposed functional inter-relationships of the elements of such a static system, together with a discussion of the repercussions produced in that system by external interference. Such a conception of "economic theory" reduces the science to futility; for it excludes from the supposed equilibrium all the real dynamic forces which alone give the concept of equilibrium meaning and content, and which alone give intelligibility to the very conception of "curves"—or *implicit* functional inter-relationships. It is also inherently self-contradictory, because, as we have seen, the assumption of an inherent *stationary* equilibrium, to which the system tends to return when disturbed, only holds on the hypothesis of a miraculous special set of inter-relationships among the various "elasticities" of the long period, or general equilibrium, curves.

The problem of distinguishing *true external* changes from those "implicit" in (and, as time proceeds, "emerging" out of) the system itself, still remains, however; and is one of considerable difficulty. It is capable of solution only in terms of the Marshallian conception of the economic system as an *organism*.<sup>39</sup> The difficulty of the problem of dis-

<sup>39</sup> The fear which this term inspires in some thinkers has been eloquently voiced by the late H. J. Davenport. (*Economics of Enterprise*, pp. 387-394, n.). It is here employed, not as a "biological", or any other kind of "analogy", but in the philosophical sense, as a convenient term to describe *any* living, self-developing system. Most of Professor Davenport's objections to the term "social organism" seem to me either irrelevant or to apply to "biological" organisms as well; and all of them might—on his own admission that we "do not know what life is"—apply to at least *some* "biological" organisms. If we are to accept Webster as our authority for the use of terms, Davenport's suggestion that we substitute "organisation" amounts merely to the substitution of (in this "universe of discourse") a synonym. And, if we permit appeal to individual *feeling* regarding the implications of a word, the present writer seems entitled to object that the word "organisation" savours of a discredited, artificial, "contractual" theory of human society.



tinguishing satisfactorily between an organism and its environment turns basically on the fact that what is *relevant* in the environment depends in part on the structure and nature of the organism itself, and that interaction is mutual. But this difficulty can be circumvented by distinguishing (relevant) *changes* in the environment which are *due to* the activity of the organism, and (relevant) *changes* in the environment which are *independent of* such activity.<sup>40</sup> The latter may, for our purposes, be dismissed, after noticing one troublesome complication. If it be held that "economics" is not, in strict methodology, coextensive with "sociology," then the economic system is not identical with the social organism; but is one *aspect* of it. We should therefore, in a more comprehensive discussion, have to examine this view,

It is doubtless open to question whether any philosophical discussion of the distinction between "mechanism" and "organism" has ever reached satisfactory finality; but the old-fashioned reductive technique which degrades the "organic" to the "mechanical" is definitely moribund, and present indications point rather in the opposite direction. The aspect of the problem with which we are here primarily concerned is resolved in the Marshallian conception of the mechanics of organic growth. See below, pp. 136-137.

In my view, the concept of "organism" is open to some criticism, from an ultimate philosophical standpoint, as being itself associated on another level with attempts at false "reduction" of the more complex to the less complex. The economic organism is a *society*; and the progress of accurate thought requires rigorous philosophical analysis of the concept of "society". From this standpoint, it is more important, and more accurate, to say that organisms are societies, than it is to say that societies are organisms. Cf., e. g., A. N. Whitehead, *Adventures of Ideas*, chaps. xi-xv.

<sup>40</sup> We may thus contrast depletion of natural resources, through the activity of the economic organism, with earthquakes, or with seasonal crop fluctuations in so far as these are due to the weather.

A frequent confusing use of terms makes it desirable to add that independent environmental factors are "economic" factors in the same sense that independent environmental factors in biological evolution are "biological" factors. That is, they are facts pertaining to economic and biological science respectively.

and the question whether it involves the possibility that the "environment" of the "economic organism" may include not only physical, but also "social" and "political," conditions, which may undergo (relevant) *independent* change.

Our present concern, however, is simply with the fact that the economic organism is itself "organic." It presents the problem of indefinite "emergent" change, even in the absence of independent environmental change, and so challenges scientific analysis of, and formulation of the conditions of, such change.<sup>41</sup> The apparatus of Marshallian demand and supply curves, conceived, as already outlined, as containing within their own "elasticities" the implications of indefinite emergent change in the system as a whole, may thus at once be seen to be, in essence, *the abstract mechanics of organic economic evolution*.

Now it is in my opinion of the very highest importance to grasp the fact, and the implications of the fact, that this abstract apparatus, besides being one which, because abstract, does not itself describe the organic process in its full concreteness, is also of such a character that its own abstract functional interrelationships are intrinsically incapable of determination except through a progressive exploration of the realistic sociological, technological and psychological factors whose concrete processes it symbolically depicts. It may seem at first sight that this is a statement which nobody would dream of denying: yet it is by implication denied, either entirely or almost entirely, by those who seek to effect a cleancut segregation of economics from psychology and the other social and technical sciences. The idea seems

<sup>41</sup> While the phenomenon of "emergence" is frequently offered as the ultimate philosophical *differentia* of "organism" from "mechanism", the above should be understood, not in this sense, but merely as a provisional statement of fact. The problem of the relationship of organism to mechanism—or rather those aspects of this problem which concern us here—is further discussed in sec. x.

to be that, after an initial and final taking over of an elementary modicum of alleged psychological or other facts from these neighboring sciences, the economist can thereafter proceed with his task in magnificent isolation. And this, it would seem, is possible because he has thereby equipped himself with a final and valid determination of at least the *general forms* of the demand and supply functions which he thereafter devotes himself to manipulating.

I shall accordingly endeavour to show that it is quite impossible to seek to escape the indefinite and progressive inter-penetration of the "boundaries" of economics into the "territories" of all the neighboring social sciences by alleging that the general forms of the demand and supply functions are ascertainable without such exhaustive investigations. For in this way it will be possible most conveniently and fruitfully to develop further the implications of the suggestion, already made, that the graphical distinction between elasticity and shift in curves is a purely relative and flexible one; and thus to explicate more fully the provisional conception, already outlined, of the Marshallian method as a dynamic mechanics of organic evolution.<sup>42</sup>

<sup>42</sup> Cf. Marshall, Preface to the Eighth Edition, p. xv.

## VII

"Fragmentary statical hypotheses are used as temporary auxiliaries to dynamical—or rather biological—conceptions: but the central idea of economics, even when its Foundations alone are under discussion, must be that of living force and movement."

*Alfred Marshall.*

The notion that the distinction between elasticity and shift, as applied to graphical theoretical curves, is relative and flexible appears clearly in Marshall's analysis of Supply. He points out that his discussion relating to the long period supply curve (itself, as we have seen above, a hypothetical and therefore theoretical curve) might equally well have been developed in terms of a discussion of a (hypothetical) *series of shifts* in a *short* period curve,<sup>43</sup> with whose temporal succession of short period equilibrium points it is indeed *practically* equivalent.<sup>44</sup> And he notes that, if we were able to develop such a discussion in terms of the various time intervals required for the various adjustments depicted along the length of a single long period supply curve, this would constitute a fundamental advance in our methods of analysis.<sup>45</sup>

The long period supply curve is thus, as Marshall points out, not adequately depicted when drawn in two dimensions representing price and quantity alone: it requires to be drawn with reference to three axes, and with a *direction* in the Time dimension—the more extensive adjustments in the scale of the industry normally requiring more time to occur than the smaller adjustments.

<sup>43</sup> *Principles*, p. 463, n.

<sup>44</sup> *Loc. cit.*, pp. 809-810. See below, sec. ix.

<sup>45</sup> *Loc. cit.*, p. 809.

It is not surprising that those who are committed in advance to the notion of a timeless, instantaneous "statics" should fail to perceive and to follow up the implications of this conception. The theoretical structure, in terms of which they habitually think, lacks, as it were, *an entire dimension*. Failing to realise that this long-period curve (when "theoretically perfect") is itself the basic *static* curve (corresponding to the "complete mutual determination" of "mathematical statics"), and that an adequate skeleton picture of a *dynamic* economy therefore requires the still further conception of the "movement" of the *long-period* curve in Time, they are unable effectively to transcend in thought the inadequate notion of "dynamics" as consisting of the "movement" in Time of "*instantaneous*" curves which are for them the only "*true theoretical*" curves. For this view, they seek support in the distressing phenomenon of the three allegedly "Empty Boxes" (now, however, being progressively filled<sup>46</sup>) involved in the conception of Normal Supply; and also in pointing to the sense of a baffling a-symmetry between Demand analysis and Supply analysis which undoubtedly accompanies acceptance of Marshall's long period supply analysis. They then try to abolish their perplexities at a single blow by proclaiming that "there is only one theoretic supply curve; and it slopes upwards in all cases." In this way the difficulties of the long-period curve are comfortably dismissed as manufactured difficulties arising out of Alfred Marshall's lack of rigorous theoretical precision!

But we have already seen that this delightfully convenient refuge is an intellectual mirage—the lack of theoretical rigour is on the other side. And in this connexion it is

<sup>46</sup> See, especially, the extremely important work of Mr. G. F. Shove, which has appeared from time to time in the *Economic Journal*: particularly for March, 1930, and March, 1933.

extremely instructive to note that this false prescription for the attainment of theoretical precision is accompanied by conscientious (if naive) admission that it does not really provide such precision: for perhaps (it is conceded in passing) there is no such thing as a "true theoretical" *supply* curve at all!<sup>47</sup>

<sup>47</sup> See especially Schumpeter, *Economic Journal*, September, 1928, *loc. cit.*, p. 367.

This ultimate scepticism appears to arise from a feeling that even a "short period" supply curve cannot be given the supposedly essential qualities of rigorous "instantaneity" which adherents of a timeless Statics demand, and which they believe to be present unalloyed in the kind of theoretical demand curve which they regard as the perfect type of "theoretical" curve. This is further discussed below. But it may here be pointed out that a "theoretical curve" is simply a diagrammatical representation of a series of hypothetical propositions or conditional sentences, of the type "If A, then B." The elimination of Time has nothing whatever to do with the theoretical rigour of such theoretical propositions, which, in the Marshallian analysis, relate to the theoretical segregation of forces operating in an economic *price-quantity-time continuum*.

It is true that the conception of a "curve" also carries with it the implication of (an unspecified degree of) "continuity". But the path from "continuity" to "discontinuity" is itself, in a very real sense, continuous. To suggest that the concept of "the margin" is an instrument which can be used only in connexion with "infinitely small" gradations of change is to display a basic failure to grasp the essential significance of the Marshallian method. It might almost be said that the fundamental significance of Marshall's work lay in the flexible application of the marginal concept to a wide variety of *forms* of "continuity": "The margin, which must be studied in reference to long periods and enduring results, differs in character as well as in extent from that which must be studied in reference to short periods and passing fluctuations." (*Principles*, Preface to Eighth Edition, p. xvi). "Smallness" is itself a relative concept: and it is extremely probable that, on thorough investigation, the notion of discontinuous *quanta* would be found to be essential to *all* "periods" and *all* "margins".

It is extremely instructive to note that Schumpeter seeks to dichotomise his "statics" from his "dynamics" on the basis of *both* of two distinct criteria, which are, on his own showing, mutually irreconcilable; and that, when they conflict, he shamelessly abandons the criterion of "dis-

But if it is not surprising that those who reject the Marshallian supply analysis have failed to explore its general implications for the structure of theory as a whole, it is at least curious that those who have continued to accept, defend and even develop it have so largely failed to explore and expose its fundamental and universal application. For the confusions and futilities that have repeatedly dogged the footsteps of adventurers in search of *symmetry*<sup>48</sup> between supply analysis and demand analysis are due, I believe, to the fact that the true solution lies in the opposite direction from that in which it has been repeatedly and fruitlessly sought. It is not Marshall's supply curves that are theoretical monstrosities: *it is his long period normal demand curves*. The only valid criticism of Marshall on this whole fundamental matter is that he was not Marshallian enough: he did not extend (with, of course, appropriate flexibility) his own conception—of the essential relativity of the distinction between “elasticity” and “shift”—*to both demand and supply curves for all periods*. Hence the very noticeable lack of firmness in his handling of demand analysis.<sup>49</sup>

The *explanation* can only lie in the extraordinary dominance of “creative emergence” for that of “creative continuity” for that of “creative emergence”. Cf. *loc. cit.*, p. 378, n.: “In the case of important invention, change in data is great; in the case of unimportant invention it is small. But this is all, and the *nature* of the process and of the special mechanism set in motion is always the same.”

It is the thesis of this essay that the qualitative, or organic, or emergent element characterises (in varying degrees) *all* economic phenomena; and that no such “special mechanism” exists. “Normal action falls into the background” (Marshall, *loc. cit.*, p. xiv) only in the case of events which defy reduction to “law”; these do not fall into any radically distinct “qualitative” category; and the pragmatic boundary line which segregates them shifts continually with the progress of scientific knowledge.

<sup>48</sup> I refer, of course, to logical or conceptual symmetry: there are naturally no *a priori* grounds for assuming *material* symmetry.

<sup>49</sup> The most significant passages are: pp. 93-94, 99, 109-111, 455-6, 462, 465.

nance exercised over the science from its infancy by the "psychological principle" of "diminishing utility." The *error* lies in assuming unquestioningly that a valid and useful theoretical (hypothetical or conditional) demand curve must necessarily be a "translation"<sup>50</sup> of this principle "into terms of price."

"There is," says Marshall,<sup>51</sup> ". . . an implicit condition in this law which should be made clear. It is that *we do not suppose time to be allowed for any alteration* in the character or tastes of the man himself. . . . *If we take a man as he is*, the marginal utility of a thing to him diminishes steadily with every increase in his supply of it." Now if with this passage we compare another and closely succeeding one—"For *time is required* to enable a rise in the price of a commodity to exert *its full influence* on consumption"<sup>52</sup>—we must be struck immediately by the presence of at least an apparent inconsistency. But it is precisely at this point that it is necessary to proceed with the utmost caution. For it is possible to reply that the inconsistency is apparent only; that the "time" required for consumption to respond to a price change is merely "historical" or "clock" time, whereas the "time" that is eliminated in the definition of the "theoretical" demand curve is *functional* or "operational" time.<sup>53</sup>

<sup>50</sup> Marshall, p. 94.

<sup>51</sup> P. 94. Italics mine.

<sup>52</sup> P. 110.

<sup>53</sup> For an able, if somewhat provocative, discussion of the place of these two concepts in Marshall's work, see Redvers Opie, "Marshall's Time Analysis," in *The Economic Journal*, June 1931. Mr. Opie's discussion serves admirably to disclose some of the misunderstandings involved in Professor Robbins' critical comments upon Marshall's "statical method"; but he seems to me to have been led to make some assertions of an extreme and over-simplified character which threaten to land him in confusions of a directly opposite sort from those which he claims to find in Marshall. It is true that, having asserted roundly that "the



Whether such a reply is sufficiently conclusive in the case of the short period demand curve to permit us to continue to regard it as simply a "translation" of the law of diminishing utility into terms of price, I shall consider in the next section. For the moment, the essential point to notice is that it can have no validity whatever with reference to the *long period* normal demand curve. For the *functional* definition of "the long period" imposes no theoretical obligation upon us to place a straitjacket on the internal psychological processes of the consumer. On the contrary, it definitely forbids us to do so. A theoretically perfect long (supply) period, it will be recalled, "must give time enough to enable not only the factors of production of the com-

fundamental idea" of the distinction between the long and the short period "is a very simple classificatory device" designed to segregate the *class* of forces which are respectively permitted to operate, he goes on to concede that such functional classifications may themselves "shade into one another". But he adds: "but the shading has nothing to do with the continuity of time in Nature" (p. 200). It is just here, I think, that error is bound to creep in. What *really* "troubled" Marshall (I shall not assert that it *never* "led him into confusion") was that the shading has to do *both* with the functional *and* with the temporal shading of "periods". It is only by reference to what seems to me to be Mr. Opie's one-sided concentration on the "operational" aspect that I can explain what I believe to be his own unsatisfactory comments (pp. 203-4, n.) on the long period curve. For reasons already given, I do not think it was "clearly open" to Marshall to "admit" that "'curves' built out of shifts in curves may" (does Mr. Opie mean must?) "be purely historical records." A theoretically perfect long period curve admirably illustrates the complexities arising out of the concurrence of both temporal and functional "shading": for, while from a functional point of view the concept is homogeneous and precise, from the temporal standpoint the amount of clock time required varies for every point on the curve. I believe Marshall quite definitely assumed *two* things as "normal": (a) that "long period" adjustments require more clock time to take place in than do "short period" adjustments; and (b) that more extensive long period adjustments on a given curve require more clock time to take place in than do less extensive long period adjustments on the same curve. See below, sec. ix.

modity to be adjusted to the demand, but also the factors of production of those factors of production to be adjusted and so on"—throughout the entire system, until stationary general equilibrium once more supervenes. There is here no question of a "long period" during which, by definition, no changes are permitted to occur in the internal structure of supply. For this would be patently ridiculous. Not only do we permit all those internal changes, in the structure of the industry supplying the commodity, which are *consequent upon* the initial stimulus of the increase in normal demand: we also allow—because we are logically compelled to allow—whatever consequent readjustments are required in the supply and demand conditions for all the other commodities in the economy.

Now it is *this* concept of the long period, I submit, which should logically, symmetrically and naturally be applied also to the case of demand. A "theoretically perfect" long demand period is one which "gives time enough" for—or, if the reader prefer, is so functionally defined as to permit—all those readjustments (throughout the entire demand and supply structure for all commodities) which are consequent upon, and necessary to, the complete adaptation, in a closed system, of the demand for a specific commodity to a change in its normal supply price. A theoretically perfect long period demand curve must show the various alternative *general equilibrium* conditions of demand that would ultimately supervene in response to various alternative initial changes in normal supply conditions.

Is there, then, any *a priori* reason why the demand for all commodities should, *in the long period*, conform to the Law of Diminishing Utility? I submit that there is no such reason.<sup>54</sup> If in fact it does, the matter is nevertheless one

<sup>54</sup> It is true that Marshall wrote: "Those demands which show high elasticity in the long run, show a high elasticity almost at once; so that,

calling for much more elaborate determination than the mere inappropriate evocation of the "psychological principle" of "diminishing utility," whose truth is dependent upon the assumption that "we do not suppose time to be allowed for any alteration in the character or tastes of the man himself." For we now no longer *start* with a psychological dogma imposing a rigid and contradictory paralysis upon the hypothetical mental history of the consumer. We postulate simply a hypothetically isolated change in the price (that is, in the supply) of a single commodity, and then inquire what the effect upon demand will be, *given time to allow all the implications* of the existing situation to work themselves out. We do not (in the illegitimate sense of that ambiguous expression) "take a man as he is." We do not arbitrarily and absolutely exclude "*any* alteration in (his) character or tastes." We take the man as he *potentially* "is"; and, while excluding *substantive* changes in his character or tastes, *permit*, as we logically must, such changes in his character and tastes as are *consequent upon, or are evoked by*, the initial disturbance of price the reaction of "demand" to which we are engaged in studying. There is therefore no *a priori logical* reason, arising out of our definition of a "true theoretical" demand curve, for excluding at the outset the possibility that some commodities may, under certain conditions, be subject to the long-run Law of Increasing Utility.

subject to a few exceptions, we may speak of the demand for a commodity as being of high or low elasticity without specifying how far we are looking ahead" (*Principles*, p. 456). This is possibly one of those occasions on which Marshall confused "operational" with clock time. At any rate, I submit that this statement is meaningless on the assumption that we obtain "the one universal law of demand" by "translating" the psychological principle of diminishing utility "into terms of price" (for in the case of a demand curve so defined there can be no question of "the long run"); and incorrect if we obtain our long period theoretical demand curves in the manner here described.

Neither is there any mathematical or graphical basis for such exclusion. Even if we assume once more (as in the case of supply) that the various long period demand and supply elasticities are miraculously dovetailed so that the system as a whole is a truly stable equilibrium, which does remain "stationary" throughout the "dynamic" functioning of equal and opposite forces, there is still no self-evident *a priori* reason why some given long period demand adjustment throughout the whole system—in response to some "spontaneous" increase in supply—should not involve such reorganisation of the habits and preferences of consumers as to bring about an ultimately larger total consumption of the particular commodity in question at a higher price. Just as, in the case of supply, an industry may (in a stationary state) be in stable equilibrium and yet conceal implicit conditions of decreasing cost which would become explicit in response to a permanent increase in demand for its product; so, in the case of demand, some particular consumers' want *may* (in a stationary state) be in stable equilibrium and yet conceal implicit conditions of *increasing utility* which would become explicit *in response* to a permanent increase in supply. There is nothing inconceivable about this even if we again make the assumption of a miraculous dovetailing of elasticities involving real stability of the stationary equilibrium as a whole. All that is necessary is that the positively inclined long period demand curve should intersect a more steeply rising normal supply curve.<sup>68</sup>

It seems justifiable, therefore, to assert provisionally, as it were, the existence of *the same Empty Boxes* in the case of Normal Demand as in that of Normal Supply. Can the empty *demand* boxes be provided with contents? Can it be demonstrated that long period demand for some commodi-

<sup>68</sup> Again we should note that if the normal demand curve is to be "theoretically perfect" it presumably cannot rise for long.

ties actually does conform to the Law of Increasing Utility? To embark on such a demonstration in any great detail would be to transcend the essentially methodological confines of the present discussion. I think it may fairly be urged, however, that with the resolution of the methodological difficulty more than half the battle has been won, and that, for purposes of general discussion at least, the burden of proof is upon the critic to show by realistic psychological analysis that in no case can a cheapening in the supply of any commodity (in a stationary general equilibrium) produce such repercussions in the system of habits and preferences of consumers as to lead ultimately to the consumption of a larger quantity of that commodity at a higher price than before.

The history of discussion concerning "increasing returns" does not encourage one to go much further than this within the limits of the present analysis. Nevertheless, it may be pointed out that, to prove the universal negative proposition, it would be necessary for the critic to demonstrate the existence of some peculiarity with regard to the formation and modification of systems of wants which would make the functioning of a Principle of Increasing Utility forever and inherently impossible.<sup>56</sup> I can think of no such peculiarity. On the contrary, it seems to me reasonable to suggest the presence, in the case of demand for some commodities, of two subsidiary factors "corresponding" in certain respects

<sup>56</sup> It seems to me of some importance to emphasise that the discussion of this section cannot be accurately described as a defence of Edgeworth's concept of "negative elasticity" against Marshall's refusal to entertain this concept. Marshall was clearly well aware of the purely mathematical possibilities of "negative elasticity" (see *Memorials*, p. 441). He repudiated the concept on *realistic* grounds, and as needlessly confusing to "the ordinary mind". Hence what is important in the present argument is the claim that, on realistic grounds of the very type which Marshall himself regarded as of the first importance, the concept of long period "increasing utility" is essential.

to the factors of internal and external economies in the case of supply. It is difficult to characterise them satisfactorily; but since they also should be "blanket" concepts, or further "empty boxes" whose filling requires concrete psychological and sociological investigation,<sup>57</sup> they may perhaps be designated the factors of internal and external *emerging benefits*. Internal emerging benefits would result from more extended consumption by a given consumer leading to an increase in the desiredness of the commodity to him, quite apart from any increase in consumption by other individual consumers or any increase in the number of consumers. External emerging benefits, while also, of course, accruing to individuals, would be such as could nevertheless only so accrue in virtue of the expansion of *aggregate* consumption, whether by other previous consumers or through an increase in the numbers of consumers.<sup>58</sup>

<sup>57</sup> Just as the filling of the empty boxes of internal and external economies requires concrete sociological and technological study.

<sup>58</sup> For example, if the production and consumption of automobiles is in equilibrium in a given community on a small scale, an increase in supply at a lower price may *induce*, not merely a "reversible" elasticity response from consumers, but an irreversible (or imperfectly reversible) modification of their want systems. Those who formerly kept one car may now keep two, while many who formerly kept none may now keep one. Any of these consumers considered in isolation may, *in consequence of the stimulus of the initial price decline*, so modify his system of preferences that even if price were to return to a higher level than before (through failure of further supply economies to keep pace with the long period demand adjustment), he would maintain an increased consumption. And this habit modification might be more pronounced and less reversible in the case of each consumer *if the modification of all reinforced the modification of each*: the emergent benefits of a new mode of income expenditure to the individual may be in part dependent on the discovery of like benefits by one's friends.

The reader will misjudge me if he supposes I am trying to force an unnatural parallel with supply. There is no parallel *in detail*. We do not need, for example, to discuss in the case of demand, whether conditions of increasing utility do not involve monopoly of consumption by

There is one difficult matter which must be briefly treated at this point. As the reader will now have clearly in mind, we have been discussing, in connexion with the long period Law of Increasing Utility, strictly *hypothetical* conditions, which are therefore capable of depiction by a "true" theoretical long period curve. These conditions are, moreover, as we have seen, quite conceivable in a really stable stationary general equilibrium, in which the "forces of progress and decay" are equal and opposite—the operation of increasing utility in the case of one commodity (or more) being exactly counterbalanced by the operation of diminishing utility in the case of other commodities: just as, in the case of supply,

a single consumer. This bugbear does not even raise its head (unless, indeed, those who yield to it in supply analysis are prepared to contend that, "under truly competitive conditions", all consumers would be excluded from the market except the one member of the community to whom the marginal utility of money was lowest). The applicability of the concepts of "internal" and "external" alike to economies and emerging benefits rests on the deeper fact of the organic, mutually interpenetrating character of all basic social processes; and reveals the profundity of Marshall's methods of analysis.

It may be mentioned that a more intensive discussion would need to take account of repercussions *through Supply* on collateral elements in changing systems of Joint Demand. And it would need to undertake a detailed analysis of the *factors* making for long period conditions of increasing utility in the demand for some commodities. It might be found, for example, that *caeteris paribus* a commodity would be more likely to be subject to increasing desiredness if its purchase absorbed a smaller fraction of total income than an automobile. Generally speaking, the operation of the law is clearly intimately dependent upon the capacity of the commodity, once a supply price decline stimulates its consumption, to uncover "new" wants and to effect diffused organic modifications in related old ones. On both these grounds, the telephone, the radio and electric power would seem to be subject over a considerable range of price and time to the law of increasing utility. I cannot here discuss the complications that arise in seeking to formulate correctly the bearing on this law of utilisation of such commodities as aids to the production of others. In my opinion, in discussions of this kind, "demand" means "final" demand from "consumers" of *all* kinds.

the operation of decreasing cost in the case of one commodity (or more) will be, under really stable stationary conditions, precisely counterbalanced by the operation of increasing cost elsewhere. A long period demand curve which rises *for a time* beyond its point of "stable" intersection with a still more steeply rising supply curve is therefore not only a "true" theoretical curve, but it *may* also be theoretically "perfect".

We have also seen, however, that the Marshallian long period supply curve *need* not be theoretically "perfect": that, in fact, in a "dynamic" economy undergoing internal organic development, it *cannot* be. In short, it is quite possible for the *supply* conditions implicit in an economy to be such that, once its dynamic or organic expansion is under way, the general level of unit costs *as a whole* may decline indefinitely. Is any corresponding phenomenon either possible or conceivable on the side of demand?

Now, on certain "static" psychological assumptions, it would of course be possible to declare that a general rise in the "marginal utilities" of things in general—so far from constituting "economic progress" of a kind comparable with that usually assumed to accompany a general fall in the "costs" of representative firms under the operation of the principle of decreasing cost—is itself indubitable evidence of (is in fact identical with) the *impoverishment* of the community through increased scarcity; and therefore involves a shrinkage in "total utility." But it is at least quite clear that this kind of "static" psychological assumption eliminates in advance the very thing whose possibility and conceivability we are here discussing. Increased scarcity of this kind is clearly the mere resultant of occurrences originating on the side of supply. What we wish to know is whether "autonomous" changes on the side of demand are possible or conceivable, which are in any valid way



comparable to organic developments on the side of supply involving *general* reductions in "cost".

To render this problem even moderately intelligible, several preliminary distinctions are essential. First, it will be desirable not to discuss the matter in terms of *money* "demand price" or *money* "expenses of production": in view of the prevalent differences of opinion as to what constitutes "monetary neutrality" in a matter of this kind, it is better at this point to avoid the "money" aspect of the problem altogether. But, secondly, neither are we here concerned either with *psychic* "real cost" or with *psychic* "satisfaction". On the side of supply, we have to distinguish between "effort" (expenditure of human physical energy) and "the disutility of effort" (real cost). On the side of demand, we have to distinguish between "desiredness" (or *Wanting*) and the "utility" derived from the "satisfaction" of "desire".

By the "economies" which result in a closed economy from a general extension in the operation of the principle of "decreasing cost" is meant, therefore, an increase in the aggregate volume of physical return to a given expenditure of human effort.<sup>59</sup> Now it is of course obvious that there is not, and cannot be, any exact counterpart of this phenomenon on the side of demand: for it is essentially a phenomenon of technical production. But this does not mean that there is *no* phenomenon on the side of demand which is an "autonomous" cause of absolute organic expansion of demand.

Is the absolute "volume" or intensity of Wanting (for

<sup>59</sup> I avoid the apparently alternative mode of statement—"a diminution in the amount of human effort required per unit of output"—because, in order to segregate for a closed economy the forces of absolute expansion implicit in, say, a transfer of demand from an industry subject to increasing costs to one subject to decreasing costs, it is necessary to assume that the total quantity of effort expended remains constant.

things in general) a constant or a variable? Can the Wanting of an individual be conceived at all as an absolute magnitude; and, if so, is it always and everywhere the same, or may it expand and contract? I venture to think that there is meaning in the notion of "total" Wanting; that it is (or can be made to be) neither more nor less definite than the notion of total *effort*; and that both alike are variables capable of absolute expansion and contraction.

There has been in economics much loose talk about the "insatiability" and the "infinite expansibility" of human wants "in general". This notion is the pernicious twin of that other questionable concept—the "infinite" productivity of capital (in sufficiently large quantities); and is probably much the more dangerous of the two. Putting on one side the psycho-pathological problem of infinite greed, it should be clear that, under stable stationary conditions of the simplest kind, in which no changes either in methods of production or in wants are taking place, the individual will have a *finite system* of wants, in equilibrium with his system of activities, and a constant volume of expenditure of energy per unit of time.<sup>90</sup> Now the irruption of progressive dynamic *supply* conditions may bring about indefinite modification in, and possibly enrichment of, the internal structure of his demand system, as regards both the quality and variety of the objects demanded. Yet the individual may, despite the shock of these innovations to his former peaceful system of preferences, steadfastly refuse to exert a greater quantity of effort than before, effecting all organic modifications in his wants system within the rigid limits thus self-imposed. On the other hand, he may be led to increase, perhaps progressively, the amount of physical effort he is willing to exert. But to this there must obviously be an upper limit.

<sup>90</sup> The *appropriate* unit of time being one complete "static cycle". See above, sec. iv.

I think that, for our immediate purpose, it is proper to regard the total amount of physical energy which the individual is willing to exert, in order to satisfy his "wants" in general, as his total effective *demand*.<sup>61</sup> It is in terms of this conception that we can validly formulate the long period

<sup>61</sup> The incorrigibly "reductive" type of monist who would "reduce" *cost* to *utility* should note, in the above procedure, what will probably look to him suspiciously like reversing the process and reducing utility to cost. But in fact neither of these invalid procedures is here in question. Economists are (or were) accustomed to point out that "supply is demand, and demand is supply". Apart from the applicability of this truth to the actual goods and services themselves, it is, I hold, the element of *effort* which is identical in both "demand" and "supply". But this identity does not imply *reduction* of either to the other. The duality of *psychic* "costs" and "satisfactions" cannot be resolved in this way; still less can it be resolved by "reduction", because one is not the mere "negative" of the other: *both* are *positive* psychic experiences.

This sheds light on the absurd claim that Wieser and his successors transcended the supposed primitive "dualism" of Marshall's scissors in an elegant monism. What really happened was that Marshall started from the abstract formalistic monism of elementary "statics"; avoided the false one-sided emphasis on "demand" which, in terms of its own static abstractions, it had no right to make; opened up the underlying psychological level of reality from very contact with which the static formalist shrinks; displayed the presence of an active dialectic of opposing psychological principles; and clearly indicated the nature of the concrete reconciliation of this dualism in a dynamic and organic theory. It is a mistake to suppose that such a dialectic involves the assumption that all "production" is "painful" and all "consumption" pleasurable (*cf.* Marshall, *Principles*, pp. 135-136): it is only "at the margin" and "in equilibrium" that "satisfaction" emerges unambiguously on the "demand" side and "real cost" on the "supply" side.

The matter is too complex for adequate discussion here. The incredible cocksureness with which the crude intellectual blunders of reductive monism today parade themselves is only explicable on the assumptions (a) of a complete innocence of any philosophical conception of the relation of Man to Nature; (b) of an inherent lack, in abstract static formalism, of the courage of its own neurotic convictions; and (c) of a sublime imperviousness to the significance of the evolutionary and organic concepts in connexion with which Marshall found it essential to emphasise "activities".

Law of Increasing Desiredness (what I have hitherto called the Law of Increasing Utility). It will of course be apparent that the operation of this law with respect to a specific commodity is independent of absolute expansion of total effective demand: desire for such a commodity will, under stable stationary conditions, simply absorb at the expense of other commodities an increasing *proportion* of the constant total amount of effort expended. But it is now also evident that this need not *necessarily* happen; and that the actual long-run demand functions for *all* commodities *may* be such as to constitute, on the side of demand, an "autonomous," explosive force making for organic growth.

The further problem of the relation of this analysis to problems of *psychic* "real cost" and "satisfaction" really belongs to another discussion: that of Maximum Net Social Satisfaction through Time. But readers who feel strongly on the matter of the increasing "tension" of modern life may be tempted at this point to try an adaptation of Marshall's inferences from the Law of Decreasing Cost, and to propose a complementary policy of placing sales taxes on commodities subject to the Law of Increasing Utility and promoting by governmental action of various kinds the consumption of commodities subject to the long-run Law of Diminishing Utility! It is therefore only fair to warn them that the subject is a treacherous one. This will be immediately apparent if the reader will reflect that increasing general "tension" (manifested in increasing aggregate expenditure of effort) does not result from the mere atomistic operation of the Principle of Increasing Desiredness in the case of one or even more commodities; since, as we have seen, such *specific* instances of the law are compatible with a situation in which the individual refuses to increase his *aggregate* expenditure of effort. Furthermore, even if increased aggregate expenditure of effort does occur, it may be the conse-

quence, not of diffused social-psychological factors predisposing towards a general increase in *tension*, but<sup>63</sup> to the diffused influence of the higher real income involved in the "supply" changes—mediating through such factors as improved health and vigour—in inducing a greater capacity for enjoyment (of work as well as of consumption) and a lower marginal disutility of a given aggregate of effort.<sup>64</sup>

Now, whether the aggregate expansion of Wanting be due to a decline in the marginal disutility of a given aggregate of effort, or whether it be due to an increased willingness to suffer more intense marginal disutility of effort,—in *either* case, the form of any particular "demand function" is itself a "function" of the *whole structure and functioning* of the economy: not only in a "static" sense, but from an organic standpoint. Or—to put the matter somewhat more clearly—

<sup>63</sup> I owe the suggestion to Professor Mitchell.

<sup>64</sup> I think there can be no doubt that this distinction is a meaningful one, corresponding to two quite distinct and recognizable kinds of change of psychic state. It is fashionable nowadays, among those who wish to evade all consideration of the "metaphysical" difficulties connected with the basic concepts of the science, to talk about "the relativity of economic quantities". But it is through resolution of its own basic metaphysical confusions that any science achieves its greatest advances. As already suggested (*supra*, p. 9, n.), this "relativity" is properly a challenge to undertake the search for an underlying "absolute" which, when discovered and formulated, may radically transform our conceptions of the "relative quantities" themselves. It seems to me that the immediate significance, for normative economics, of the above distinction, lies in its implication that two economic processes, radically distinct as regards their inner psychological significance, might (of course within a limited range) "objectify" themselves identically so far as the purely quantitative characteristics of their abstract mechanics were concerned. It would seem to follow that the really serious problems of normative economic theory involve something deeper than, and qualitatively distinct from, rather facile conceptions of mere manipulative mechanics.

It may be added that the very supply changes which tend to reduce the marginal disutility of a given aggregate of effort, are by no means incapable of simultaneously inducing increased expenditure of effort.

we err if we visualise a "closed" economy (whether "Crusoe" or social) as consisting of a mechanical plurality of "given" *self-subsistent* demand and supply "curves" (or "functions") which, in their mechanical interrelationships or interactions, together explain and exhaust the whole—regarded as a "causal" *resultant* of this "machinery". Rather must we regard the specific demand and supply functions for particular commodities as *constituted* by, and *derivatives* from, the whole economy (whether Crusoe or social) regarded as an organic unity.<sup>66</sup> That this is not "mere mysticism" (in the vulgar, abusive misuse of that term) should be apparent from the consideration that, if aggregate Wanting undergoes absolute expansion, it is unintelligible to speak of this expansion as *resulting* from the fact that the various specific demand functions *are* such that, in their mechanical interaction, they *produce* this expansion. So far as real causation is concerned, the reverse is the truth: it is the functional character of the system as an organic whole which determines and dictates the specific "elasticities". And the difference is not merely a verbal one. For the various specific functions are not crude data—brute facts of experience: they are abstractions derived from analysis. It may be said that they are admitted by everyone to be "interdependent". But the point is that the structural character of this "interdependence" as a comprehensive whole cannot be *obtained* by any process of mechanical combination of the "elements". It can be obtained only from an understanding of the diffused, functional organic principle; which itself, however, aids analysis by enabling us to make at least some headway

<sup>66</sup> This may seem more obvious in the case of a personal, than in that of a social, economy: but the progressive discovery of its applications to, and significance for, the latter is one of the greatest tasks confronting twentieth-century economics. "The Mecca of the economist lies in economic biology rather than in economic dynamics."

towards determining the conditions under which specific functions will assume one or other of the various forms abstractly possible.

Naive, mechanical pseudo-“rationalism”<sup>66</sup> has sought to evade the task of coming to grips with the organic principle by trying to segregate “static adjustments” from “dynamic changes”: or, to use a fashionable jargon, “endogenous” from “exogenous” changes. The former are conceived of as “within”, the latter as “without”, what is called “the chain of economic causation”.<sup>67</sup> Of this, something has already been said above;<sup>68</sup> and something further will be said in a later section. It is sufficient at this point to emphasise (or to re-emphasise another aspect of something that has already been stressed) that even the attempted circumscription of “economics” to “statics” does not permit successful evasion of the organic principle. For if we assume conditions such that the aggregate expenditure of effort remains *constant*, this “static” situation, no less than a “dynamic” one, requires explication in terms of a diffused organic principle. We cannot “explain” this stationary organic balance as being the “result” of a miraculous mathematical combination of “functions”. We must rather seek to explain the mathematical peculiarities of the system of functions as resulting from (or rather as the quantitative expression of) certain organic characteristics of the “economy” as a systematic unity.<sup>69</sup>

<sup>66</sup> What Professor Morris Cohen calls, I think, “crypto-rationalism”. See his *Reason and Nature*, *passim*.

<sup>67</sup> Cf. Lionel Robbins, *An Essay on the Nature and Significance of Economic Science*, p. 116.

<sup>68</sup> Section VI.

<sup>69</sup> The application of the phrase “within the chain of economic causation” to “statics” shows that the static formalist has mistaken logical or mathematical deductive necessity (in terms of certain arbitrary statical assumptions) for efficient causation. Cf. Eddington, *loc. cit.*, p. 238. The

A mechanical economic atomism which would exclude organic conceptions from economic "science" is thus ultimately as impotent in the field of "statics" as it is in the field of "dynamics". Its attitude is based on false reification of "given" curves and functions which themselves can have no determinate meaning or existence except when defined with reference to diffused organic concepts. It rests, in short, on the fallacious metaphysical doctrine of "external relations".

The foregoing discussion should serve to illustrate, at least in a very general way, one broad aspect of the universal truth that, in the economics of a closed system, *the whole is "prior" to its parts*. It will be obvious that more detailed development and exemplification of this truth would involve embarking on the colossal and ambitious task of actual concrete development of the science of "economic biology". Such a task transcends the modest methodological scope of the present essay. Some further ramifications of the general principle involved will be developed in the ensuing section; and, at a still later stage, some representative illustrations will be offered of the more general applications of the same principle to sciences other than economics.

The phenomenon of the "Empty Boxes" having been discovered to be present in the case of demand as well as in that of supply, it becomes evident that even the *general forms* of demand and supply functions (for long periods at least) cannot be obtained by any brief, elementary and final excursion into the realms of "psychology" or "sociology" or "technology". Both demand and supply (long period) functions may assume *any* of the abstract logical (or mathe-

static economic formalist has in fact "not even begun to find the controlling laws" of the economic system.



mathematical) possibilities which are *a priori* available. Only concrete study can ascertain which abstract possibility actually applies in a given instance; and this involves the progressive development of further "laws" of the "dynamics" (or rather "organics") of consumption as well as of production. It is therefore in the nature of the case inherently impossible (and therefore, in the proper sense of the term, *illogical*) to seek to place *a priori* pseudo-"logical" methodological *limits* to the invasion of "economic theory" into the realms of psychology, sociology, technology, and so on.

In the light of these considerations, we are enabled to assess at their true value the pretensions of that school of thought which inhabits an even more tenuous and rarefied intellectual atmosphere than do those who confess at least an *initial* obligation to "psychology": I refer to those who proclaim no obligation at all.<sup>10</sup> Lacking the courage of their own suicidal convictions, these zealots sometimes aver that their demand functions, though "independent" of "psychology", are derived from "experience": that the "things which psychology studies" are "given data" for the economist. Coming as it does from the self-constituted arch-enemies of "empiricism", this pretence is inherently unintelligible. As these pure formalists themselves vehemently proclaim on other convenient occasions, no one ever has "observed", or ever *can* "observe" (or even obtain by statistical analysis) a "true" demand function. Since these functions are thus not crude empirical data, they are derivations from analysis, and from *psychological* analysis. The mere fact that the economist, in his own limited psychological investigations to obtain his functions, stops short of, or evades the technical language of, "hedonism", does not make his results "independent" of

<sup>10</sup> For an able recent exposition of the tenets of economic formalism, see Lionel Robbins, *op. cit.* See especially, in the present connexion, pp. 83-86.

"hedonism". An elementary economic psychology of this kind is simply not competent to decide whether its results imply, or must ultimately be integrated with, "psychological hedonism". They may, or they may not: that is a much deeper psychological problem. The elementary economic psychologist may not, indeed, need to *proclaim* himself a "hedonist"; but, conversely, he has no right whatever to assert that his elementary psychological conclusions do *not* ultimately imply "hedonism". The uneasy desire to make this assertion is itself evidence that he has already gone beyond his own avowedly limited sphere, and has had the impertinence to reject "psychological hedonism" as psychologically invalid. All that such an elementary economic psychologist has any right to do is to assure the professional psychologist that he has not himself obtained his functions by means of *a priori* deductions from a dogmatic "hedonism", but has reached them with the aid of a little unofficial "psychologising" of his own, and to ask that they therefore be scrutinised by official psychology in this spirit, and not summarily dismissed without careful examination as necessarily erroneous corollaries of a "hedonistic" dogma that is no longer regarded as valid by psychologists themselves.

It is therefore *ambiguous* to assert that the psychological analyses of the elementary economic psychologist are "independent" of "hedonism" or of any other "school" of psychological doctrine. This can only mean either (a) that at most the results of embryonic economic-psychological analysis have not actually been reached by specific use of the doctrines of such a school, or (b) that the economist has actually pushed his psychological researches far enough to entitle him to take a definite stand on the question of hedonism, and to assert that his position does not imply this doctrine. But in either case it in no way follows that the results obtained by the amateur economic psychologist are finally and eternally

valid. Whether "hedonistic" or not, they are at least results of psychological analysis; and, as such, are necessarily subject to scrutiny and possible emendation by more elaborate psychological investigation. To deny this is to embrace with both arms the Fallacy of Dogmatic Finality.<sup>21</sup> The intellectual strategy of those who would evade this consequently inescapable "dependence" on "psychology" by retreat into formal abstraction is the strategy of neurotic evasion; nowadays so familiar to alienists in cases of dementia praecox. "Economic theory" can neither (a) be made "independent" of psychology (or of other neighbouring sciences, such as sociology, technology, etc.), nor yet (b) escape with some initial "obligation" which it thereafter shamefacedly and most immorally tries to forget. The progressive development of "economic theory" is the progressive development of the *interrelations* of economics with its fellows. The corpus of the sciences is, in the strictest sense of the term, a *society*: not a collection of hermetically-sealed atoms which must avoid mutual contact in the interests of individual self-preservation and "independence".

The discussion of the foregoing section is therefore properly to be regarded as, from one point of view, an adventure in Economic Psychology. Though it is an excursion into this field of only the most elementary and general kind, it has nevertheless served to show that the conception of one universal law of demand—the law of diminishing utility—is psychologically invalid. The deeprooted and persistent belief in the impregnability of this "law" as yielding the one and only "true theoretical" demand curve derives from the fact that this conception has been obtained from a combination of (a) the static assumption of a fixed available total outlay (or fixed total expenditure of effort); (b) the "static" psychological conception of "diminishing marginal utility";

<sup>21</sup> A. N. Whitehead, *loc. cit.*, *passim*.

and (c) the conception of rationality, with its corollary of conscious systematisation of preferences. By whittling away the "hedonistic" associations of (c) and refusing to commit itself on the question as to just *what it is* that the individual systematises; by arguing that full and self-conscious "rationality" need not be omnipresent in order that the principle of "diminishing utility" (or diminishing "vendibility") should operate; and by placing increased emphasis on the "static" assumption; modern economics has succeeded in making out a plausible case for a "law of diminishing utility" stated in terms, not of "hedonism", but of a kind of analytical "behaviourism". Regarded merely as a defence against rather crude charges of dogmatic "hedonism", this position has much to commend it. But it is merely defensive; it involves, as we have seen, a fallacious conception of "statics", and it can provide no final resting place for a virile and progressive science. More careful analysis shows that its real weakness is not "hedonism", but a disintegrating mechanical atomism, which vitiates the very pseudo-"statics" which it seeks to make its last line of defence. For we have seen that, even in the case of a valid Marshallian Statics, knit and integrated by means of the concept of organic unity, the "principle of diminishing utility" need not universally operate. When it is further realised that "the" stationary state is itself a mere arbitrary "statical hypothesis", and that so-called "static adjustments" may themselves *give rise* to "dynamic changes", we shall no longer bow to the illusion that material limitations relevant to the real world can be somehow or other excogitated out of a formal void. Once the crumbling ruins of "the" stationary state are tidily removed, it becomes startlingly apparent that the possibilities of pure formalism are indeed boundless, and that the pure formalist in economics has delivered himself into the eternal bondage of an absolute freedom which makes all choice impossible.

## VIII

"At that time I did not understand the nature of the idea of *precision* in economic generalisations."  
*Professor Lionel Robbins.*

The controlling purpose of this unavoidably complicated discussion is to reach a general understanding of the meaning of the notion of a "moving general economic equilibrium", with the ulterior object of relating this concept, in a later study, to the conception of "maximum net social satisfaction through Time". An adequate analysis of the problems raised by the Marshallian conception of short period (*partial equilibrium*) demand and supply curves would therefore transcend our present objective. But some further discussion must be offered at this point, confined in its purpose to further elucidating the conception of the relativity of the distinction between elasticity and shift.

The *irrelevance* of the psychological principle of diminishing utility to the formulation of *long* period laws of demand should by this time be apparent. It may now be questioned whether this "law" is an appropriate or even possible basis for the "precise" definition of *short* period theoretical demand curves.

The difficulty raised by the dual functional and temporal characteristics of economic "periods" has already been noticed; and must be recalled here. As has already been noted, once these two aspects of an economic "period" become apparent, there is danger that we may be led to suppose them capable of segregation for separate analytical treatment; whereas, in fact, the clock-time required for the various adjustments itself depends on the character and scope of the forces that are "permitted" to operate. But, while the temporal element must thus be reckoned with in any attempt to

construct a full-fledged theoretical dynamics, it may of course be provisionally overlooked while performing the limited task of simply *defining* a "period" in terms of the range or character of the forces it releases. Our problem therefore really is: is it appropriate and useful, and is it even *intelligible*, to define a "short demand period" as one during which there is *no* "alteration in the character or tastes of the man himself"?

It is of paramount importance to realise that the basic difficulty here is again *psychological*. For it may seem that the assumption itself is an obvious one; and that, even if, following the procedure already adopted for the long period demand curve, we now wish to define a "short" demand period as one in which *some* changes in "character or tastes" *are* permitted;<sup>12</sup> still, this does not preclude the simultaneous validity of a theoretical demand curve, (conceived as the "shortest" of all short period curves) which does exclude *all* changes in "character or tastes."

It may be admitted at once that this argument is in its outward verbal appearance, deceptively consistent with the spirit of the present discussion. But the real difficulty is to decide precisely what substantial *meaning* underlies the apparently transparent expression "no alteration in character or tastes".

In the broad sense in which Marshall was content to employ his distinctions, there is, of course, no great difficulty, because we all know (or think we know) more or less what is meant. But the difficulty becomes fundamental when we seek to keep pace with the determined modern attempt to achieve *precision* in the dynamic analysis of a closed system.

<sup>12</sup> So long as they are not "substantive", but simply "responses" to the supply price change; and so long as, like the short period supply curve, they are distinguished from the long period curve by involving something less than complete readjustment throughout the whole system.

X: a  
G3  
14092

It is in connexion with this attempt that clear recognition of the basically psychological character of the difficulty is vital.

For it is apparently widely believed that "precision" can be given to this notion of a so-called "static" curve without any elaborate resort to psychology: it seems so easy and obvious to postulate "absence of change".

Now, for the kind of "static theory" which regards the "instantaneous" demand curve as its first and last line of intellectual defence, the distinction between "change" and "absence of change" in "character or tastes" is *identified* with the distinction between shifts in these curves and mere elasticity adjustments thereon. But such a procedure places this type of "static theorist" on the horns of a curious dilemma. For if we assume—as we must for this purpose—that the total money income of consumers is given and constant, then, if the elasticity of demand for a particular commodity happens to be *anything other than unity*, any response to any change in the price of this commodity, by altering the total amount of money expended thereon, affects to some extent the outlays of the consumers on their other articles of consumption, and so *of necessity* shifts the demand curves for at least some of these other commodities. In terms of the "static" theorist's avowed method of *defining* "changes in character or tastes", therefore, consumers have, in the very process of making their "static" response, *changed* their "characters and tastes": for they have "shifted" their demand curves for certain commodities. It follows that the demand curve, adjustment along which produces such consequences, cannot in strict rigour *be* a "true theoretical" curve from the standpoint of the "static theorist". Once again it is the pseudo-precisians who are "loose".

I suppose it may be assumed that the "static theorist" (however ruthlessly prepared he may be, in his fanatical quest for "precision", to force "economic science" to tread

a methodological pathway constructed in the form of a diminishing spiral, with its centre in Nirvana) would scarcely be prepared to confine his "static laws" to a society in which, by hypothesis, the elasticity of demand (and supply) for each and every commodity is unity. But even if he were, it would not help him: for it is a sufficient answer that any such "definition" of "absence of change in character or tastes" is purely arbitrary and abstract, and inherently meaningless. We cannot avoid "psychology", and at the same time pretend to a rigorous definition of the treacherous phrase "absence of change in character or tastes". A very simple illustration will suffice to show this.

Suppose I have only two articles of consumption, which I consume in a given ratio while their relative prices are given. If, now, these relative prices change, so does my consumption ratio. Under such circumstances, what conceivable meaning is attachable to the statement that there has been "no change in my character or tastes"? Suppose that, in the initial situation, I expend exactly *half* my total effort on each of these articles. Must I say that an "absence of change in my character or tastes" implies that, when their relative prices change, I *still* expend precisely half my total effort on each, though now receiving different quantities of each in return? Put in this way, the notion may appear plausible to some; but it is in fact mere illusion. Suppose, instead, that my "demand schedules" for the two commodities *are at the outset* such that to the change in relative prices I "automatically respond" by devoting three-fifths of my total effort (which remains a constant aggregate) to the acquisition of one, and two-fifths to the acquisition of the other; *and* that, nevertheless, my "character and tastes" continue to be such that, if the original price ratio were restored, I should then "automatically respond" by reverting to my original half-and-half distribution of effort. If I am at the



outset so constituted, and continue throughout to be so constituted, that I am prepared, in the face of repetitions of this reversing stimulus, to repeat identically this completely reversible response, then surely there is no intelligible sense in which I can be said to undergo any "change in character or tastes". In short, the only conceivable method of attaching a rational and consistent meaning to the conception of "absence of change in character or tastes" is to interpret it as applying to a stable, "stationary" and *completely reversible*, closed psychic *whole*.

All this, it may be said, is but a needless elaboration, by elementary methods, of a conception already thoroughly familiar to those acquainted with the general equilibrium theory of the "Mathematical School". No doubt. But I am not sure that the *implications* of this conception, in its relation to the Marshallian analysis, and to the preceding sections of this essay, are thus thoroughly familiar.

In the first place, this "instantaneous static" curve, which we have supposed offered to us as the "shortest" of all "short" period curves, turns out to be, when rigorously and consistently defined, a *long period curve*—and a "theoretically perfect" one. It is the counterpart, on the side of demand, of the long period Marshallian supply curve which Schumpeter—and various alleged adherents of the "Mathematical School"—would have us discard in favour of the short period supply curve (if only this could be made "short" enough to correspond rigorously with the one true theoretical demand curve!). It is not a "partial" equilibrium curve at all; but a *general* equilibrium curve: so that, if a *short* period or *partial* equilibrium demand curve is obtainable at all, it must clearly be obtained by other methods than assuming "no change in the character or tastes of the man himself", and then seeking to give this phrase perfect "precision." Paradoxically, a "short period" demand

curve which allows *some* "change in the character or tastes of the man himself" is "shorter" than an "instantaneous" (or "long period") one which *does* not!

In the second place, this "really static" curve, which allows *no* change in "character or tastes", *may* not conform to the Law of Diminishing Utility: for, being a general equilibrium or long period curve, it *may*, as we have seen, conform to the Law of Increasing Utility—even in a truly stable stationary equilibrium.<sup>13</sup>

In the third place, having gone so far, why should we not go a step further still, and point out that there seems to be no *a priori* reason why a general equilibrium or long period demand response should not involve *an increase in the aggregate expenditure of effort, and yet remain completely reversible*.<sup>14</sup> We should thus have,—given the continued application of the reversing stimulus—an alternately expanding and contracting psychic universe. It would not be "stationary" if we chose units of time less than the duration of one such "cycle"; but there is nothing to hinder selection of a more appropriate time unit. I see no reason whatever why, if modern mathematical physicists are to be free to indulge themselves with enticing variations of this kind, puritanical economic formalists of the "Austrian" persuasion should be pitilessly debarred from similar alleviations in the more prosaic and pedestrian cultivation of the Dismal Science.

<sup>13</sup> This may be questioned; so I should point out that by "may" I mean "for aught we yet know to the contrary". Marshall, as we have seen, emphasized the "irreversibility" of the declining long period supply curve. But he did so on *material* grounds: that the scale of an industry once having been enlarged in response to an increase in demand, mere contraction of the latter to its previous extent would not *in fact* restore the *status quo ante* on the side of supply. No abstract formalism could yield this result. Lacking corresponding *psychological analysis*, we therefore cannot yet say that there is any inherent reason why we should not suppose a total psychic response, involving the Law of Increasing Utility in the case of one commodity, to be "completely reversible".

<sup>14</sup> The preceding footnote, *mutatis mutandis*, is applicable here also.

Finally, and in the light of these considerations, let us consider the situation when the "instantaneous static"—or, as I prefer to call it, the long period<sup>75</sup>—demand curve is "theoretically imperfect"; and depicts "adjustments" or "responses" which, as a matter of cold brute fact, are *not* "reversible". It should surely now be clear that this question of "reversibility" is one of material fact, and not one of formal definition of the functional character of the curve

<sup>75</sup> If man were omniscient, and so completely conscious of all his own potentialities, then perhaps the long demand period would have to be conceived as instantaneous or timeless. But in fact he is not; and he therefore requires Time to discover these potentialities under the influence of various stimuli.

There is a kind of inverse aspect of this fact, which may have considerable significance for Economic Psychology. It does seem possible to conceive of an extremely short period (almost "instantaneous") demand curve, for a particular commodity, to be understood as somehow excluding by *definition* the implication of consequential readjustments throughout any appreciable part of the total field of preferences. (The extreme case of complete absence of relevance to any other elements in the preference system would seem to be, if it is strictly conceivable at all, synonymous with sheer unregulated impulse). But, if this is so, then satisfactory "definition" is a much more difficult task than superficially appears. And such a curve would *not* assist any attempted rehabilitation of a comprehensive, exclusionist and necessitarian "statics". On the contrary, its probable function would be to serve as a conceptual element in a complex theory of psychic "indeterminacy" and of the progressive emergence of new preference systems. It would be quite impossible, I fancy, to give rational intelligibility to the notion of the *simultaneous* psychological coexistence of a whole set of such curves; and their form might have to be regarded as, in individual instances, purely indeterminate. The matter could only be satisfactorily handled, if at all, as a subordinate element in a broad metaphysical theory capable of synthesizing the "tyranny" of unregulated impulse with the "freedom" of rational choice. Cf. Whitehead, *op. cit.*, p. 255: "The causal independence of contemporary occasions is the ground for the freedom within the Universe. The novelties which face the contemporary world are solved in isolation by the contemporary occasions. There is complete contemporary freedom. It is not true that whatever happens is immediately a condition laid upon everything else." But this is only a starting-point for such a theory. Cf. also *ibid.*, p. 249.

itself, using the term "functional" to refer to the *range* of forces that are "permitted" to operate in response to the initial stimulus. For the *range* of free forces is, as it were, identical whether the adjustment proves to be "reversible" or not. We simply do not know, until we actually reverse the original stimulus, whether the response is reversible. If in fact it is not, we cannot by any conceivable method of "scientific abstraction" *segregate* the irreversible from the reversible *aspect* of the response. To seek to do so is to try to divide something which, in its very nature, is inherently indivisible. It is to make a self-contradictory "assumption" of reversibility simultaneously with the "assumption" of conditions which, as a matter of brute fact, are *irreversible*. We cannot "impound" the "changes in character or tastes" in *caeteris paribus*, because they are themselves an integral element in the response. The only intelligible *precise* definition of "absence of change in character or tastes" is complete reversibility of the whole long period or general equilibrium adjustment. If this general adjustment is in fact irreversible, then general equilibrium adjustment itself involves "change in character and tastes". But since, in *both* cases, the whole general adjustment *must be permitted*, there is nothing "detachable", as it were, to "impound".

So far, therefore, we have made no headway whatever towards discovering a satisfactory psychological principle in terms of which to define a useful and appropriate "short" demand period. Not only that, but we have been compelled to reject absolutely, for this role, the "instantaneous static" curve defined with precision in terms of "absence of change in character or tastes"—even though the "static theorist" should abate his demands to the extent of begging us simply to accept this curve simultaneously with any *other* "short" period demand curves that might turn out to be useful. And the reason for this rejection is that the plea is meaningless.

In asking ourselves whether there can be such a thing as a partial equilibrium demand curve, and, if so, on what basis it should be defined, we should, as in the case of the long period, turn for light and leading to the Marshallian analysis of Supply. The short supply period does not depend for its definition (any more than the long supply period) on any unintelligible assumption of *complete* internal static immobility in the particular industry. Neither does it depend on any apparently rigid but really elusive assumption of general stationary equilibrium throughout the whole closed economy. It very sensibly avoids *both* these tantalisingly interpenetrating "opposites", and postulates instead (by an application of the device "not quite accurately called the statical method") the provisional holding of one set of factors (the "scale" of the industry) constant during the free operation of another set. It does not confuse the issue by asking at the outset for final precise and dogmatic determination whether the forces which are permitted to operate are "reversible" or "irreversible".

The principle of segregation of the "free" from the "impounded" forces is not (and could not be) derived *a priori* and *in vacuo*. It is a material distinction, obtained from observation of, and reflection upon, the technical structure of the industry, the psychology of business men, and the sociological structure of industry as a whole. It turns in fact in a general way, upon the broad distinction between "overhead" and "direct" costs, between "supplementary" and "prime" costs, between "fixed" and "working" capital. This is the *basis* of the distinction: its *development* requires resort to a more concrete, more subtle, and more profound investigation into the actual structural and functional character of organic growth in the field of Supply.

Speaking broadly (and anything more would here be impossible), I think we must look for the *basis* of distinction

between a general and a partial equilibrium demand curve in the distinction between fairly quickly extended application of a given commodity to "already existing" uses, in response to an initial fall in the supply price; and the slower, more gradual emergence of "new" uses which supervenes when the lower supply price continues long enough to work out its "full effects" upon the consumers' systems of wants and preferences. The general law of *short period* demand is (at least provisionally) the Law of Diminishing Utility. In the case of *long period* demand, either the Law of Diminishing Utility or the Law of Increasing Utility may operate.

So powerful, and so chronic in some quarters, is the craving to draw "broad artificial lines of distinction where Nature has made none", that several brief warnings (involving partial recapitulation) must here be given ere we pass on to the next step in the march of our general argument.

First, the distinction between extension of "existing" uses and emergence of "new" uses cannot by any exercise of perverted ingenuity be transformed into a Great Wall of China. I do not merely mean that, if the price of wheat falls suddenly and sufficiently, it may be quickly given to domestic fowls, whereas at the higher price it was not; so that this, though a short period response, might be termed a "new" use. It must be not merely conceded, but affirmed, that the distinction between the two periods here turns in part on the *degree* of reversibility, in part on the *extent* to which the "new" use (in this sense) was already present to consciousness, in part on its emergence independently of any *fundamental* changes in the general structure of habits and preferences; and so on. But none of these things can be bloated into the central and sacred dogma of some "school" of economic "thought". The Principle of Continuity is too subtle for that.

Thus, though the supposed alteration in the diet of domes-

tic fowls may be supposed to be one which would not be maintained in the face of a reverse price movement of equal magnitude and suddenness, this does *not* mean that the attitude of the owners of the fowls in this matter of feeding has undergone *no* incidental irreversible modification; or that it is one of the essential elements in our very definition of the "short period" that it *must* not. Other illustrations of short period demand responses might well exhibit higher degrees of *quick* incidental irreversible modification; and, conversely, some illustrations of *long* period demand responses might plausibly show a fairly high degree of reversibility. It is therefore not essential to the definition of short period demand responses that they must *involve* "no change in the character or tastes of the consumer"—a phrase the *precise* meaning of which we have already seen to be capable of explication only in terms of a theoretically perfect *long* period.

Again, it is no part of the definition of the short period demand response that it must occur *without any* repercussions on demand schedules for other commodities. This could only be so if all short period demand curves were required to possess unit elasticity.

All such attempts to effect rigid *mechanical* partitioning of "periods" in terms of such categories as the above are mere evidences of intellectual depravity. They confuse mechanical separation with functional distinction of aspects of a unitary organic process. This becomes even more apparent, if possible, than our already over-long discussion should have made it, when we further reflect that in real life we are not concerned with the occurrence of isolated catastrophes in some unsubstantial "static state". We are concerned with the continuous, everchanging, living process of organic growth and development. Changes in *normal* supply price do not "normally" come like thunderbolts,<sup>76</sup> and short

<sup>76</sup> So widespread is the misunderstanding of the relation of "normal"

period and long period adaptation takes place both simultaneously and continuously, in response to stimuli which themselves occur, not as "discontinuous" catastrophes, but as living processes of organic growth. This is true both of changes in normal demand acting as stimuli to long period adjustments in supply, and of changes in normal supply conditions acting as stimuli to long period adjustments in demand.<sup>77</sup>

processes to "cyclical fluctuations", that it requires today some courage to utter this truth: one is apt to be accused of being unaware of the depression. No one single access of illumination would do so much for economic science today as a widespread realization that the theory of normal value, both "long" and "short", and the study of "fluctuations in the volume of business activity" *occupy two distinct universes of discourse*. It is the most pernicious economic error now prevalent to hold that the theory of normal value "explains equilibrium, but tells us nothing about disequilibrium", which must therefore be separately accounted for in terms of dynamic disturbances of "equilibrium". If the present work should do anything to dissipate this false and unintelligible juxtaposition, it will not have been written in vain.

Unfortunately, there is no simple and short way of saying what *happens* to "long period normal" prices and forces during a "cycle". But (a) a *general* fall in money prices does not produce specific "normal" responses of the kind here under discussion; (b) while relative values are frequently severely "dislocated", this is not generally *regarded* as "normal" or permanent (even when we allow for the lengths to which "errors of pessimism" can sometimes go); and (c) "long period normal" *responses* still take time.

The patient thinker is continually being called upon to apologize abjectly for the complexities of a subject-matter for whose nature he is in no way personally responsible. See also next note.

<sup>77</sup> So great is the danger of misunderstanding here that I must add something. First, it is quite true that changes in the "price" at which commodities are offered may occur quite suddenly, and may sometimes be spectacular in their extent; and these changes may be due to technical innovations which quickly *affect* "normal" supply price. But the process of *complete* long-period adjustment of *supply* to these changed conditions does not, and *cannot* occur quickly.

Secondly, all curves for all "periods" must be conceived as having reference to a particular time; or the notion that they shift through Time would be meaningless. In the quest of "precision" we may there-



Some readers are bound to feel that, having first examined and rejected, for the role of "short" demand period, the *precise* notion of "absence of change in character or tastes", I have then blandly proceeded to substitute and advocate "precisely" the same conception *in a loose form*. This is not at all a bad description of what I have done—and done deliberately—in trying to follow, to the best of my ability, in the classic footsteps of Alfred Marshall. I have earnestly

fore seek to visualize a long-period curve as having reference to an instant of Time, and therefore as shifting from instant to instant. We could then argue that this series of shifts in the long-period curve may be either "fast" or "slow". Even if this were the case, it would not alter the fact that the actual complete adaptation of the system itself towards these ideal "norms" would nevertheless require time, and a *long* time. The result of this would be that moment to moment shifts in the position of the long-period curve would, individually, elicit very little response from the actual economic organism. In consequence of reflections along these lines, the seeker after "precision" may very easily conjure up a vision of a world in which the fluctuations from moment to moment through Time of *long* period equilibrium supply price are *wider* and more extensive than the actual fluctuations of actual prices (on the analogy of a donkey whose zigzag pursuit of a moving carrot exhibits narrower deviations from a straight line than does the zigzag progress of his nimbler and more speedy seducer). But such a notion would, I am convinced, be an illusion. Being (I say it quite without pride) entirely innocent of mathematics, I am totally incapable of developing the problem quantitatively; but I suspect that Alfred Marshall spoke with strict mathematical accuracy when he spoke of *slow* secular changes in long period supply and demand prices and conditions, and intended this to apply, not, as we are nowadays so frequently assured, to a "quasi-static" world in which "business cycles" and other "catastrophes" are unknown, but to the actual world in which we live. We have already discovered that there is an inherent *conceptual* difficulty in the way of giving precision to the notion of the movement of long-period normal price in time; in that, for a dynamic economy, a theoretically perfect long period is itself a contradiction in terms. If mathematics can circumvent this difficulty for *broad* theoretical purposes, then I suspect that, however unsatisfactory this may seem to the pseudo-precisian, the "instant" to which a given long-period curve must be conceived to be *relevant* must itself be a "unit" which is a considerable period of "clock-time".

and conscientiously and painstakingly tried to be "loose with system".<sup>78</sup> The quest of "precision" leads inexorably and inevitably to *the Whole*. Hence, in the first instance, the application of precision to the definition of "absence of change in character or tastes" leads us willy-nilly to the "theoretically perfect" *long* period—and the *short* period has eluded us altogether. To begin with, therefore, we must either compromise with Precision or lose the immense practical advantages of the short period concept. But this is not all. With relentless courage, we now seek to draw even nearer to the God of Precision, who still somehow partially eludes our embraces. We discover—not without some passing vertigo—that the self-subsistent and comprehensive *static whole* is in its turn an unsubstantial part-truth, infected with "looseness" in its very essence. If we still sternly refuse all "compromise", all paltering with the sacredness of Precise Truth, the long period itself must now be thrown into the limbo along with the already abandoned short period: and we are left gasping weakly in the void—praying pitifully for the advent of the Economic Einstein whose single Magic Equation, embracing in one mighty sweep the whole universe of the Economic Space-Time Continuum, shall one day deliver us from all our troubles and perplexities.

I wonder if it will. It may instead, like Schelling's Absolute, prove a "night in which all cows are black". For the

<sup>78</sup> Any more satisfactory definition of a short demand period, in more precise terms than I have here achieved, would seem to depend upon the ability of Economic Psychology to discover some *structural* principle of differentiation "corresponding" to that between fixed and working capital on the side of Supply (which is likewise not perfectly clearcut). I have not been able to satisfy myself as to how this may best be done for Demand. I am disposed to think that the proper line of attack on the problem is through the conception of *sub-systems* of wants—by way of development and elaboration of the Marshallian concepts of Joint and Composite Demand.

inescapable present, we have, at any rate, the great Economic "Anglican Compromise" of Alfred Marshall. This is no mere "refuge". A solid and broadbased structure, it yet leaves the way wide open for the next major advance in the evolution of Classical truth. It is no easy step; and it will not be made quickly. But if the inheritors of the Classical Tradition in this generation attempt it with courage, patience and integrity, they may yet be able to say, without shame or sentimentality—"one step enough for me". If, on the other hand, they sell their birthright for a mess of pottage—running blindly after strange Non-Conformist gods whose devious and "reductive" Böhm-Bawerkian pseudo-logic holds out delusive promises of a "static" shortcut to final mystic union with Precision—this step will not be taken in our generation: and the chaotic sweep of world events will quickly, tragically and completely discredit a bogus science which believes the path to "exact truth" and to the hardly-won mantle of "authority" to lie through the disseveration of "form" from "substance".

## IX

"In fact we are here verging on the high theme of economic progress; and here therefore it is especially needful to remember that economic problems are imperfectly presented when they are treated as problems of statical equilibrium, and not of organic growth. For though the statical treatment alone can give us definiteness and precision of thought, and is therefore a necessary introduction to a more philosophic treatment of society as an organism; it is yet only an introduction."

*Alfred Marshall.*

Having now, I trust, taught Precision to know its proper place, we can at once proceed to direct presentation of the general relativity theory of the distinction between elasticity and shift. We shall find that the element of "clock-time", so far from having "nothing to do" with the matter, is integrally bound up in it.

As already in part noted,<sup>19</sup> Marshall, in discussing long and short supply periods, seems clearly to have had before his mind a number of considerations, which may be listed as follows:

(a) Both the functional and the temporal aspects of economic periods "shade imperceptibly" into each other.

(b) The clock time of a given "period" varies from one industry to another.

(c) The clock time of a given "period" varies for the same industry from time to time under changing circumstances.

(d) The clock times of the various *alternative* responses to alternative initial stimuli (i. e. to alternative hypothetical initial shifts in the normal demand curve), which are depicted on a given long period supply curve for a given commodity under given conditions, *vary also*; and vary, on the whole, directly with the magnitude of the required response—large adjustments in

<sup>19</sup> *Supra*, p. 45, n.

the scale of the industry tending, generally speaking, to require a longer period of clock time for their consummation than smaller responses.<sup>80</sup>

(e) The clock time of a *long* period response to an initial stimulus is *always* longer than the clock time of the *short* period response to the same stimulus.

(f) Since the purpose in view is analysis of the actual dynamic, or rather organic, world in which we live, and not of some unsubstantial "static state", not only the short period, but also the long period, should be habitually regarded as "theoretically imperfect", and as dealing with "responses" which are inherently irreversible.

It follows, from (d) above, that the long period curve, at any rate, has *direction* in Time.<sup>81</sup> And it is this fact which

<sup>80</sup> I do not assert that Marshall regarded this as a universal and *necessary* phenomenon; nor do I venture to assert that it is. This statement must be accepted for the moment as a convenient and legitimate first approximation. To attempt to analyse with "precision" its degree of validity "in a stationary state" would lead into an endless and unprofitable maze. What I believe to be its *practical* justification will presently appear. And in the two ensuing sections considerations will be adduced which at least suggest that it has profound and universal theoretical significance.

<sup>81</sup> It is not sufficient, and may be positively confusing, merely to say that it is a "curved surface" in Time. It is quite possible to conceive of it as a *line* in three dimensions. This line may then be developed into a surface "edge on" to the time dimension for the purpose merely of indicating that the various quantities are (as they of course must be) quantities produced per unit of time. It may be that, from a purely mathematical standpoint, this is superfluous, owing to the conception of *rate*. But notice of it has the possible advantage—at least for non-mathematical thinkers—of calling attention to the problem of the "appropriate unit" (*supra*, p. 77, n.). In other words, when we are thinking on the long period plane, we "ignore passing fluctuations": normal price in a stationary state would be the *average level* of price, if that state contained market, seasonal or cyclical fluctuations.

So the point is that, regarded as a *line*, this curve has direction in Time. If it could be located at a particular instant in actual historical Time (the "instant" to which the particular series of potential con-

explains why this curve may be looked at in two different ways: (a) as showing the various *alternative* responses of long period normal supply to *alternative initial* stimuli (alternative initial shifts in the long period demand curve); (b) as showing the hypothetical <sup>82</sup> *series* of "substantive shifts", in the *short* period supply curve, that would *successively* occur through Time under the influence of a gradual increase in normal demand. To put the same thing in somewhat different terms: the long period curve is the basic "static" curve, because it shows *alternate* positions of *general equilibrium*; but it is also an instrument of "dynamic" analysis, because it represents theoretical analysis of the conditions determining the nature of the actual historical sequence of shifts of the short period curve.

The full enormity of the intellectual position of "static theorists" who would deny "theoretical" status to the long period curve is thus apparent. A "theoretical curve" is simply the diagrammatic representation of a series of hypothetical ("theoretical") propositions. Hence, to deny "theoretical" status to the long period curve is to deny in advance the very possibility of scientific *analysis* of the crude totality of conditions producing the actual historical sequence of shifts in the short period curve. The irony of the situation resides in the fact that this denial of "theoretical" status to the long period curve—made in the name of a "precise"

ditions it depicted were relevant—*cf. supra*, p. 76, n.); and if it were then intersected by the corresponding relevant normal demand curve; both arms of both curves would be directed *forward* in Time from the point of intersection. As has previously been pointed out (p. 29, n.), to draw a curve "leftwards" from its point of intersection in a two-dimensional diagram has nothing to do with the question of reversibility.

<sup>82</sup> Hypothetical, because it shows the series of short period shifts that *would* occur (in response to increases in demand) on the assumption that "substantive new inventions" or other "substantive" changes are "impounded in *caeteris paribus*".

and comprehensive "statics"—itself stultifies all efforts to achieve any such "statics" by throwing out the one theoretical curve that can give meaning and organic cohesion to the concept of *general* equilibrium. The basis of the curious confusion of thought involved is, as we have seen, the uncritical assumption that, for some unstated reason, a "precise" hypothetical ("theoretical") proposition (or series of propositions) can only be formulated if the element of Time is first of all ruthlessly *excluded*: whereas the real truth is that a higher order of "precision" reveals the fact that "precise" statement of a theoretical proposition *demands* specific *inclusion* of the Time element, and formulation of the theoretical proposition in the form of a statement about a "tendency" in a price-quantity-time *continuum*.

We are now, however, in a position to go even further than this. For the analysis of the preceding section has shown that, if we are to be really "precise", the term "static" cannot with complete accuracy be attributed to *any* curve—merely in virtue of its "definition" in terms of the *range* of (responsive) forces that are "permitted" to operate. For a "static" condition is strictly a quality, not of a particular curve, in virtue of the terms of its definition, but of the economy *as an organic whole*. Even a long period curve is really "static" only by virtue of a unique *system of interrelationships* with all the other curves in the economy—a system of interrelationships which depends on the real nature of the economy itself. If the economy is in fact not a stable stationary one, the long period curve is not "theoretically perfect". It is not, in other words, fully "static". Conversely, any attempt to make the "short period" "shorter" and "shorter" until we achieve "instantaneity" has the embarrassing consequence of bringing us back "full circle" to the "theoretically perfect" *long* period! We were therefore forced to "compromise"; and to "define"

the short period as one during which *some* internal readjustment is permitted to occur, but not complete readjustment throughout the entire system. It follows, methodologically speaking, that there is not, in the abstract, one unique short period, and one only: there may be as many "short" periods (of varying "lengths") as are found to be functionally convenient and significant after realistic analysis of the actual forces of supply and demand. And since *none* of these "short" periods can be whittled down to "static instantaneity", either functionally or temporally speaking, it follows, further, that a short period curve, as well as a long period curve, will have direction in Time.<sup>82a</sup>

From a general, abstract methodological standpoint, therefore, we have to conceive of the abstract graphical apparatus of economic theory as consisting essentially of theoretical demand and supply curves for a continuous series of whatever number of "periods" may prove, on realistic analysis, to have functional significance. Each successive "period" is "longer" than the one preceding it, and involves the functional "release" of a more comprehensive range of responsive forces. Each may be employed in dynamic theoretical analysis to depict a hypothetical series of "substantive shifts", under definable conditions, in the "position" of the curve for the next "shortest" period. Each has direction in Time.

Realistic economic analysis to date has done nothing (owing to the paralysing influence of the "Law of Diminishing Utility") to investigate the question of the existence of functionally significant *demand* periods of various "lengths". This is a task for realistic investigation by

<sup>82a</sup> There are no static curves—only (possible) static *systems* of curves. The reader who dislikes this way of putting the matter may substitute the statement that all curves are static curves—in terms of the method "not quite accurately called the statical method."



Economic Psychology. On the supply side, we owe to Alfred Marshall the one main functional distinction of a single "short" period. It is for modern Economic Sociology ("Institutional Economics") to determine whether other significant supply periods exist, and, if so, to differentiate them. It would seem to be quite possible, if not actually probable, that the number and nature of such significant periods may undergo secular modification with the "institutional" evolution of the economy in History.<sup>88</sup>

<sup>88</sup>In order fully to make good our escape from the Fallacy of Dogmatic Finality, development of the foregoing elastic conception of "periods" requires to be methodologically integrated with another, but intimately related, conception also disclosing unsuspected complexities in the full structure of our theoretical apparatus. Mr. G. F. Shove (*Economic Journal*, March 1933) has shown that it is inadequate to conceive, even in a given instance, of only *one* long period supply or cost curve, the quantities of which are regarded simply as "functions of price". The shape of a "given" supply curve depends upon assumptions regarding not only demand *price*, but also the "structure" of demand. "For example, if the demand for retailers' services is concentrated closely about a particular shopping centre, a firm may supply all its customers from a single establishment, whereas if the demand were more widely spread it would operate through several branches: its cost might be different in the two cases even though its volume of sales were the same."

I can here only barely suggest that, by parity of reasoning, the shapes of long period *demand* curves may depend, not merely on a simple functional relationship to assumed changes in supply *price*; but likewise on various realistic "structural" assumptions as to the manner in which these supply price changes are supposed to occur: so that, of two alternative "autonomous" supply changes, involving identical alterations in supply *price*, the one may elicit a larger, the other a smaller, long period demand response. And this not in a sense to be accounted for as implying "merely" differing *collateral* "cost" elements to the consumer, "utility" being taken as "constant"; but in a sense involving *different* changes in the "utility" itself. (These two things may indeed be in some ultimate sense identical; but not in any sense appertaining to an atomistic "statics" which knows no long period demand curve).

If, for example, a given reduction in supply price is effected by improved technique, including better labour conditions, the long period de-

As has been repeatedly pointed out, the general emphasis of this study is methodological rather than realistic. The foregoing conception of the graphical apparatus is put forward, therefore, as a valid general methodological first approximation: not as a piece of sacred dogma. The general conception I am urging gives rise to a multitude of more specific problems, the majority of which cannot even be mentioned here. They require long and careful study. There is, however, one general question which may conceivably cause difficulty to some readers with respect to the basic conception itself; and a few words should therefore be devoted to it.

A little reflection should suffice to show that there is no meaning in raising a doubt as to whether a "shorter" period is really temporally shorter than the next "longer" one. But there may be meaning in inquiring whether we are justified in universalising the principle derived from the long period supply curve: the assumption with respect to either a supply or a demand curve for *any* given period, that the progressively more *extensive* responses (to a larger initial stimulus) depicted on that curve require progressively longer periods of clock time to materialise. As already pointed out,<sup>84</sup> this is not advanced as a necessary and universal truth:

mand response of an enlightened consuming public may differ markedly from its long period response to an identical supply price reduction effected by taking advantage of an opportunity for "sweating". Professor Mitchell has again come to the rescue here. If any reader should be disposed to think this example either trivial or "forced", I would reply (a) that he is perhaps underestimating the possibilities, and even present actualities, of organised "economic chivalry"; and (b) that the crucial methodological significance of the general concept lies in its revelation of the possibilities of *real freedom* (not the mere irrational "caprice" behind the "instantaneous curve") underlying the deductive "necessities" of the graphical apparatus as applicable to a real economy in Time. See below, section xi.

<sup>84</sup> *Supra*, p. 81, n.

but I think it legitimate and important to claim for it a very general practical justification, at least as a first approximation. It may be objected, for example—though the objection could hardly be otherwise than very loosely developed—, that the clock-time differences of the various general equilibrium responses on a given long period demand curve may be (to say the least) much less marked than those on a long period supply curve; and it has even been suggested to me that the clock-time in the case of the long period demand curve might actually vary *inversely* with the strength of the initial stimulus from the supply side, on the ground that a stronger stimulus may act “more promptly” on the system of wants and preferences. It is in no sense necessary, in the interests of my general thesis, to cast doubt on any validity there may be in this suggestion. But it seems to me important to urge that, if offered as a contrary dogma, it rests on misunderstanding. We have first to rule out of consideration (in a manner familiar to psychologists) any stimuli which are too slight (or fleeting) to excite “long period” responses at all: these are simply irrelevant. Of those which do excite long period adjustments, it does not in any way follow that the more violent, by exciting more “prompt”, or more “rapid”, or more violent *initial* disturbances in the system of habits and preferences, thereby lead to *complete restoration* of a new general equilibrium of habits and preferences at the end of a shorter period of clock-time than in the case of milder stimuli. The response may be more “prompt” or more “rapid”: but there may be more of it.<sup>85</sup>

<sup>85</sup> I do not think anything can be made of this question, in this form, because it can only be formulated precisely (if at all) on the assumption of stationary equilibrium. In a dynamic economy there may be *no* “end” to the “responses”. Hence the importance of pointing out that what may seem to be my own “dogma” in this matter is not a dogma at all, but merely a practical device, which I believe to have broad justification, but which is easily capable of modification. However, see below, p. 136, n.

So far as I can see, therefore, the main function of considerations of this kind is to call attention to the fact that the intrinsic organic or structural character of the response may vary *qualitatively* with different intensities in the initial stimulus. This consideration might merit further study if we had in fact to do with discontinuous intrusions, at different times, of alternative sudden stimuli of different magnitudes into an identical situation in a continuing stationary general equilibrium.<sup>88</sup> But in so far as we are concerned "normally" with adjustments which take place in response to changes which are *themselves gradual* through Time, and which are themselves in part responses to the very changes which we are here regarding merely as responses to them, the appropriateness of the general principle here provisionally adopted seems unquestionable. If we were concerned with comparing two complete sets of long period responses, in a stationary state, to two alternative initial stimuli—the one a small shift in the normal supply curve, the other a sudden, violent, and very large one—we might be concerned to discuss whether the two complete sets of responses did not exhibit radical organic and qualitative differences. But if we are concerned to consider the effects of a gradual and progressive shift in the supply curve upon normal demand, then clearly we have not to consider a number of alternative sets of responses, but rather a gradual and progressive response. Under such conditions, the long period demand curve assumes that at first sight baffling duality of aspect (which we noted in the long period supply curve) which renders it capable of being conceived both as representing a series of mutually exclusive alternatives, and as "fairly" representing this series as a progressive one in which the alternative "long period" equilibrium points become a *series*

<sup>88</sup> It might be of some importance also, for its suggestiveness, in a more intensive realistic study than this.

of "short period" equilibrium points each of which is a milestone on the road to its successors.<sup>87</sup>

For these reasons I do not think the possibility—that the various alternative adjustments, to alternative initial demand or supply changes, depicted by a given long period supply or demand curve, might require much more nearly equal time periods to work themselves out completely in a truly stable stationary state—involves any essential modification in the general conceptual scheme here outlined. In any case, it remains true that the distinction between "elasticity" and "shift" in "theoretical" curves is purely relative to the standpoint and assumptions of the "scientific observer": that is, to the particular assumptions upon which a given "theoretical" curve is drawn.

It should now be more fully evident than it was when the proposition was first advanced and provisionally defended in an earlier section, that the *essence* of the Marshallian method consists in carving up the economic "space-time" continuum by the device of drawing conditional or "theoretical" curves (variously defined in terms of broad *functional* distinctions of organic structure) in different "directions" through that continuum. It should be evident, too, that this device of (more or less) "fragmentary statical hypotheses" (whether for short periods or for long) was adopted, not in order to "shirk" the (deceptive) rigours of "the" stationary state; but because, in default of the

<sup>87</sup>In my opinion, this particular aspect of the Marshallian analysis illustrates perhaps more forcibly than any other the impressive (in fact, almost ludicrous) manner in which Marshall completely outdistanced all other "schools" of contemporary "theory"; many of which, to this day, have completely failed, so far as I can see, to grasp the deeper implications of Marshall's work. As peculiarly illustrative of the difficulties that arise from lack of understanding of the Marshallian long period, see Mr. Piero Sraffa's objections to Mr. D. H. Robertson's subtle "defence" of "the representative firm". *Economic Journal*, March, 1930, p. 93.

possibility of a comprehensive quantitative dynamics "in the physical sense", this very fragmentariness in the use of the method "not quite accurately called the statical method" is, because of its *flexibility*, and its capacity for presenting the same elements now as "dynamic changes" and now as "static adjustments", the best means of attaining comprehensiveness of treatment and of vision in the analysis of a continuous process of organic growth and change.<sup>88</sup>

<sup>88</sup> "By that method we fix our minds on some central point: we suppose it for the time to be reduced to a *stationary* state; and we then study in relation to it the forces that affect the things by which it is surrounded, and any tendency there may be to equilibrium of these forces. A number of these partial studies may lead the way towards a solution of problems too difficult to be grasped at one effort." *Principles*, p. 369.

With this, *contrast* the following: "We abstract from various causes of change, and conceive the remaining economic quantities as stationary, and inquire as to their mutual relations." Robbins, *Economic Journal*, June, 1930, p. 194. Professor Robbins, who, as already noted, imagines that "the stationary state and static laws have been the main subject of investigation" in economics so far, makes docile and admiring exception for "the sublime conceptions of the mathematical economists, in which all the quantities contemplated move together in orderly change." *Ibid.* In 1898 Marshall, who had dwelt long amid these sublimities, wrote: "... when a force moves a thing on which it acts, it thereby changes the force which that thing afterwards exercises. The attraction of the Earth alters the movements of Venus, and thus alters the force which Venus exerts on the Earth: which again alters the movement of the Earth, and therefore the attraction which the Earth exerts on Venus; and so on in endless but ever-diminishing reciprocal influences. Meanwhile both planets disturb slightly the Sun, whose attraction is their chief controller; and all the other planets have a part in the play. For such complications as these arithmetic is useless: they need the strength and delicacy of vast and subtle mathematical engines working out large volumes full of mathematical formulae and figures. But these engines cannot be applied to economics." (Reprinted in *Memorials*, *loc. cit.*). Cf. my article, "Equilibrium Economics and Business Cycle Theory: A Commentary", Q. J. E., November, 1930, *passim*. It seems evident to a non-mathematical economist that to take these "engines" in their generic form, as it were, and to pin to their various parts labels inscribed with technical economic terms and symbols, is not to *apply* them to economics in Marshall's sense of the word. It lies beyond both my province and my competence to

It should be added that the essential relativity of the distinction between "elasticity" and "shift" of curve holds not only within the rigid boundaries of "the" stationary state, but is relevant also to a dynamic or organic system. That is to say, to speak (paradoxically) in terms of the "theoretically perfect" long period curve, we may again distinguish between the actual historical shifts of this curve through secular Time in a dynamic economy, and the hypothetical series of shifts it *would* undergo if some definable part of the whole dynamic continuum were "impounded in caeteris paribus": which hypothetical series of shifts could then be graphically depicted by means of a "theoretical" curve. The problem of getting a grip on this conception with our analytical machinery has obvious difficulties, which cannot be discussed here. The important point to notice is that the fact that Marshall did not attempt it in this way does not mean that his analysis is "after all, quasi-static"—in any other sense than he himself affirmed. For, in the first place, the "theoretically imperfect" long period curve itself overleaps the evasive boundaries of "the" stationary state; and, in the second place, the analysis of the conditions determining the shifts in any one long period curve is obtained, by the Marshallian method, through the application of the short period long period analysis to the other constituent curves in a "closed" system which is, as it were, "moving" forward "under its own steam".

discuss how far and how fruitfully these "engines" may nevertheless be employed in the discovery of important economic principles of a "qualitative" kind. But it is at least apparent that in this sense the structure of Marshall's *Principles* is the creation of a "mathematical economist". Cf. Edgeworth, in *Memorials of Alfred Marshall*, p. 66.

## X

"The point to remember is that the fact that each individual occasion is transcended by the creative urge, belongs to the essential constitution of each such occasion. It is not an accident which is irrelevant to the completed constitution of any such occasion."

*A. N. Whitehead.*

The general methodological viewpoint which it has been the purpose of this essay to expound is now complete in its essential outlines. It is, as I hold, simply a systematic explication of the underlying methodology of Alfred Marshall's *Principles*—corrected in one important respect (the foundations of Demand analysis) to make it internally consistent with itself, coherently *Marshallian*: and embodying one or two broad suggestions for further development with the progress of reflection and of realistic analysis. I cannot think it is seriously open to question that the scheme of thought I have presented *is*, at least in spirit and in general outline, authentically *Marshallian*:

The Mecca of the economist lies in economic biology rather than in economic dynamics. But biological conceptions are more complex than those of mechanics; a volume on Foundations must therefore give a relatively large place to mechanical analogies; and frequent use is made of the term "equilibrium", which suggests something of statical analogy. This fact, combined with the predominant attention paid in the present volume to the normal conditions of life in the modern age, has suggested the notion that its central idea is "statical", rather than "dynamical". But in fact it is concerned throughout with the forces that cause movement: and its keynote is that of dynamics, rather than statics. . . . The main concern of economics is . . . with human beings who are impelled, for good and evil, to change and progress. Fragmentary statical hypotheses are used as tem-



porary auxiliaries to dynamical—or rather biological—conceptions: but the central idea of economics, even when its Foundations alone are under discussion, must be that of living force and movement.<sup>89</sup>

Moreover, “the notion of continuity with regard to development,” wrote Marshall,<sup>90</sup> “is common to all modern schools of economic thought, whether the chief influences acting on them are those of biology, as represented by the writings of Herbert Spencer; or of history and philosophy, as represented by Hegel’s *Philosophy of History*, and by more recent ethico-historical studies on the Continent and elsewhere.”<sup>91</sup> It would therefore be only natural to expect

<sup>89</sup> *Principles*, Preface to the Eighth Edition, pp. xiv-xv.

<sup>90</sup> *Ibid.*, Preface to the First Edition, p. ix. Italics mine.

<sup>91</sup> He adds: “These two kinds of influences have affected, more than any other, the *substance* of the views expressed in the present book; but their *form* has been most affected by mathematical conceptions of continuity.” These quotations alone are sufficient to reveal the misunderstandings of the inner significance of the *Principles* that lie behind glib asides concerning “Marshall’s curious predilection for biological analogies”. Marshall’s own conception of the *place* of “mechanism” in an *inherently* “biological” science he has indicated elsewhere as follows: “The growing prominence of what has been called the biological view of the science has tended to throw the notions of economic law and measurement into the background; as though such notions were too hard and rigid to be applied to the living and ever-changing economic organism. But biology itself teaches us that the vertebrate organisms are the most highly developed. The modern economic organism is vertebrate; and the science which deals with it should not be invertebrate. It should have that delicacy and sensitiveness of touch which are required for enabling it to adapt itself closely to the real phenomena of the world; but none the less must it have a firm backbone of careful reasoning and analysis.” *Principles*, p. 769.

I have tried to show, in the foregoing discussion, how the development of economic “mechanics” and the development of “economic biology” inevitably go hand-in-hand: how a growing adequacy and relevance in the structure of abstract economic mechanics are the *spoils* of progressive invasion by economic science of the territories of social psychology, sociology, industrial technology, etc. Nothing could more devastatingly reveal

that at least the broad philosophic implications of the metho-

the *defensive* attitude of an exclusionist "static" and "mechanical" conception of the science than its stand in this matter. Professor Robbins (*op. cit.*, p. 65) confesses to "a sense almost of shame at the incredible banalities of much of the so-called theory of production" expounded in the works of nineteenth century economists. But his remedy is to slam the door of the Austrian Sanctuary against "the insidious effect of a procedure which opens the door to the intrusions of amateur technology into discussions which should be purely economic." It is thus truly an ironic Nemesis which then ensures that, in striving by means of "analytical definition" to seal up the contents of his "purely economic" vacuum against "technological" contamination, he has himself to risk (pp. 33-35) a hurried sally into the field of "amateur technology"—comparing decidedly unfavourably with those more leisurely excursions into the same field which bring the blush of shame to his cheek when he encounters them in Book IV of the *Principles*. This curiously instructive phenomenon is discussed at some length in my "Nature and Significance of Economic Science" in *Recent Discussion*, Q. J. E., May 1933—where it is also shown how the same tactics of fastidious withdrawal lure an unsuspecting "non-metaphysical" exclusionist Positivism into attempting to build for itself the same Retreat on the treacherous sands of a fallacious *metaphysic*. It is indeed profoundly and tragically revealing that it never even occurs to this school of thought to consider whether its own function in the twentieth century may be, not to recoil in embarrassment from the admitted inadequacies in the labours of its predecessors, but itself to try, modestly but self-respectingly, to remedy these inadequacies by more *concerted and systematic* expeditions into "the borderlands of economics"—in which "ambiguous regions" (no less than in the borderlands of physics and chemistry) the raw materials essential to the continued vitality and constructive development of legitimate science are to be found: and which it is therefore a decadent act of shameful surrender to abandon passively to the tender mercies of "the charlatan and the quack". The salvation of Economic Science in the twentieth century lies in an enlightened and democratic "economic imperialism", which invades the territories of its neighbours, not to enslave them or to swallow them up, but to aid and enrich them and promote their autonomous growth in the very process of aiding and enriching itself.

Under such circumstances, occasional armed conflict among the sciences is inevitable. Such conflicts must be conducted according to the rules of civilised warfare; and it is the duty of each science to subordinate its strategy, as best it knows how, to the ultimate goal of the harmonious unification of knowledge. Mistakes and injustices are bound to occur from time to time; but the "science" which cannot maintain its integrity

dology I have outlined would command immediate general assent from the various contemporary "schools" of economic thought, here and abroad—at least in the absence of any startling and revolutionary developments, either in philosophical methodology or in the basic structure of economic science itself, since Marshall wrote.

Have we experienced any epoch-making innovations of this kind? Certainly as far as the science itself is concerned, if Mr. J. M. Keynes is to be accepted as a trustworthy authority on the matter, we have not. "Marshall's *Principles of Economics*," wrote Mr. Keynes recently,<sup>92</sup> "was published forty years ago, and most of the thinking in it was done more than fifty years ago. For thirty years after its publication the progress of economic theory was very slight. By 1920 Marshall's theory of economic equilibrium had been absorbed but not materially improved."

It is therefore a contemporary phenomenon of extraordinary interest and significance that there is in fact no assurance today either that the methodological scheme of thought I have outlined will obtain immediate general assent, or that Marshall's own statements of his own basic conception of his own work are now generally acceptable to economists—even when these economists are professedly following in the Marshallian tradition. For, while it would of course be mere alarmism to regard the recent Austrian invasion of England as in itself actually perilous to the continued integrity and vitality of the Classical Tradition in the original home of that tradition, it is at least disquieting to observe unmistakable signs of surrender on the part of modern inheritors of the genuine Classical Tradition themselves. Thus Mr. Keynes writes: <sup>93</sup>

and vitality in such an environment deserves to perish. And, for my science, a cowardly isolationist pacifism which cries peace! peace! when there is no peace is the stigma of intellectual disintegration and decay.

<sup>92</sup> *A Treatise on Money*, vol. ii, p. 406.

<sup>93</sup> *Ibid.*, loc. cit.

Unfortunately Marshall, in his anxiety to push economic theory on to the point where it regains contact with the real world, was a little disposed sometimes to camouflage the essentially static character of his equilibrium theory with many wise and penetrating *obiter dicta* on dynamical problems. The distinction between the long and the short period is the first step towards the theory of a moving system. But now at last we are, I think, on the eve of a new step forward, which, if it is made successfully, will enormously increase the applicability of theory to practice;—namely, an advance to an understanding of the detailed behaviour of an economic system which is not in static equilibrium.

This characterisation of Marshall's work, I am sure it will now be agreed, would obviously not have been acceptable to Marshall himself; and, if the foregoing discussion is valid, it involves serious caricature of the true significance of the *Principles*. Nor will it be wise, I think, to treat objections to Mr. Keynes' language as mere methodological pedantry; or to explain that language away as an excusable, even though hasty and inaccurate, means of emphasizing the importance of the "new step" which economic science is today undoubtedly struggling to take. "I do not see how one who looks backward through smoked glasses can look forward with open and clear eyes." <sup>64</sup>

There is (wrote Marshall) <sup>65</sup> a fairly close analogy between the earlier stages of economic reasoning and the devices of physical statics. But is there an equally serviceable analogy between the later stages of economic reasoning and the methods of physical dynamics? I think not. I think that in the later stages of economics better analogies are to be got from biology than from physics.

<sup>64</sup> Allyn A. Young, *Quarterly Journal of Economics*, Feb., 1925, pp. 170-171.

<sup>65</sup> *Memorials*, p. 317. Italics mine.

Of course a new class of considerations as, for example, of *money, credit, international trade*, may be introduced after some others have been carried a long way; and in the first handling of new matter there may be a temporary reversion to physical analogies. *But that will pass*; and, when the new matter is ready to be worked up with the old in an advanced stage, the method will become ever more remote from the physical and more akin to the biological.

I do not infer, from this utterance of the most profound and massive mind that has thus far devoted itself to the problems of economic science, that exploration of monetary "dynamics" is a thing to be avoided. But I do think it at least suggests a doubt as to the substantiality of Mr. Keynes' hopes that the first pioneer attempts to grapple with the "dynamics" of credit offer us a prospect of supersession of the allegedly "essentially static" Marshallian analysis by a full-fledged "economic dynamics"—"not only in monetary theory",<sup>96</sup> but throughout the whole field of economic analysis. And I do think that careful reflection concerning its possible implications may serve to throw some much-needed light upon the extraordinary controversial confusion into which the subject of monetary theory has recently been plunged, by suggesting the possibility that a thoroughly rational, comprehensive and intelligible integration of "economic" concepts on the "mechanical" level may be inherently and intrinsically unattainable—even in "monetary theory".

It lies beyond my present purpose to pursue further at this time these possibly vital applications of Marshallian methodology.<sup>97</sup> The purpose of these preliminary remarks

<sup>96</sup> *Treatise, loc. cit.*, p. 406.

<sup>97</sup> This belongs to a subsequent study. See *supra*, Preface, p. vii. I can here only barely indicate one of my own main reasons for believing that the "mechanical" category in monetary theory is, for purposes of

is simply to suggest, as vividly as possible, that it is not safe,

"exact science" (what Professor Robbins calls "precision") intrinsically inadequate. The thing Mr. Keynes calls "the English taint" when it appears in index number theory (namely, the conception of *the* general price level) wears, as we have seen (*supra*, section iv), another national costume as *the* "stationary state"; and a very little reflection should serve to convince the reader that—somewhat more subtly disguised, perhaps—it is present, too, in the notion of a dynamic process devoid of the "infliction" of "forced saving" upon individuals or groups. (In one aspect, therefore, the "taint" is better described as economic Newtonianism; in another, as sentimental Liberalism.)

If the implications of this fact be unremittingly pursued, it will be found to involve the collapse of the uncritical assumption that there exists *any* "right" quantity of money requisite for the attainment of some supposedly intelligible "moving equilibrium" in Time of a kind which can be "precisely" formulated in "mechanical" or "atomistic" terms. The notion of a meticulously "right" behaviour of the total quantity of money (or of "MV") is an intellectual will-o'-the-wisp of the same order as the notion of an absolute rate of motion of the earth through "the ether". It may seem curious that Dr. Hayek, who has urged so earnestly the total expulsion from "exact science" of such "fictions" and "pseudo-concepts" (to use Professor Robbins' terms) as "price-level" and so on, should have failed to see that not only "Money", but also—and *much more truly*—"monetary neutrality", is a "pseudo-concept" of this kind. But in fact this is only one more aspect of the hydra-headed Nemesis that dogs the footsteps of an atomistic statics. See below, pp. III, n.

Mr. Keynes, on the other hand, super-pragmatist that he is, boldly employs the concept of special "price-levels" with his eyes open: and it further seems to me (though I confess I am still unable to achieve certainty) that, in his strenuous insistence that he is proffering a tattered but suspicious science an entirely "new pair of trousers" (*Economics*, November 1931, p. 390), he is in effect wheedling it back to that "organic" conception of the science the transcendent importance of which in Marshall's work he has neglected to recognise. If this be so, then the increasingly popular sport of attempting to convict Dr. Hayek of intellectual vacuity on the curious ground that his theory is substantially identical with Mr. Keynes' own, stands revealed as a frivolous pastime. For it emerges that the subtle but profound distinction between the Austrian and the Keynesian monetary structures is that the one conceals the death agonies of mechanical individualism, the other the birth throes of a cooperative commonwealth. But it is perhaps permissible to doubt whether a strong ingrained "Liberal" bias has not so far pre-

in attempting to make our own contribution to the evolution of the science, to ignore, or to pass lightly over, the fundamental methodological tenets underlying the most profound synthesis we so far possess; and to indicate, as conclusively as I know how in so brief a space, that, at least as far as recent developments of economic science itself are concerned, present-day tendencies to ignore or repudiate Marshall's, *organic* conception of the science do not rest on the secure ground of any major constructive development since his work was done—but in part upon lack of philosophical knowledge or understanding of the extremely weighty considerations that led him to insist continually on the transcendent importance of the *organic* approach; and in part upon an over-anxiety (no doubt pardonable in these chaotic times) on the part of *present-day economists* “to push economic theory on to the point where it regains contact with the real world”.

If post-Marshallian economics has achieved no fundamental advance which pragmatically, as it were, discredits the “biological” view of the science which Marshall himself held, the only other valid reason why it should today fall into discredit would have to lie in some recent advance in general philosophical methodology, which, becoming known to present-day economists with eyes for the larger movements of thought beyond their own intellectual back-fence, and perceived by them to be beneficently operative in other sciences, would naturally and legitimately have spurred them to go and do likewise—even though they had not yet succeeded in reaping any substantial constructive rewards, but were still merely engaged in the preliminary task of intellectual clarification and consolidation. This, I think, is the utmost

vented Mr. Keynes himself from realising with full clarity the real character of the child he is taking such a conspicuous share in—fathering. Cf. below, p. 165, n.

that would be claimed by even the most aggressive of those contemporary "rebels" who seek "precision" in formal "statics".<sup>98</sup>

The main body of this essay may be regarded as an attempt to discredit this pathological tendency in present-day economics by the direct method (most likely to commend itself to economists): the method, namely, (a) of exhibiting the internal looseness and lack of precision of a mode of thought which claims rigorous formal precision as its chief virtue; and (b) of showing the superiority of the biological, organic or developmental conception of the science not only with respect to precision, but also as to its potentialities for the further constructive development of economic theory itself. It may not be altogether amiss, however, if I now offer the reader, purely by way of auxiliary support to the main economic discussion, a few brief indications of the fact

<sup>98</sup> It is unquestionably a leading thesis of Robbins, *loc. cit.*, *passim*.

The "Austrian" conception of "science" is expounded by Professor Robbins as follows: "Scientific generalisations, if they are to pretend to the status of laws, must be capable of being stated exactly. That does not mean . . . that they must be capable of quantitative exactitude. We do not need to give numerical values to the law of demand to be in a position to use it for deducing important consequences. But we do need to state it in such a way as to make it relate to formal relations which are capable of being *conceived exactly*" (pp. 65-66). This notion is then ruthlessly employed by the Austrian School to *throw out* of "economic science proper" all conceptions whatsoever at whose roots the intellectual scavenger can detect any indication of metaphysical obscurity. It is significant that this activity should include, in connexion with "precise" statement of "the law of demand", the expulsion of "psychology" from "economics".

With this notion of "science", the reader may profitably contrast the following (Whitehead, *op. cit.*, p. 198): "The Certainties of Science are a delusion. They are hedged around with unexplored limitations. Our handling of scientific doctrines is controlled by the diffused metaphysical concepts of our epoch. . . . Whenever some new mode of observational experience is obtained the old doctrines crumble into a fog of inaccuracies."



that the movement in the direction of what may be variously described as static formalism, the exclusion of organic categories from "economic science proper", and an insistence upon exclusive reliance on the concepts of atomistic mechanics, is in direct and striking opposition to the whole drift of modern thought, not only in philosophical methodology, but in the actual practices and methods of a wide variety of special sciences. Systematic treatment of such a topic would of course be a subject in itself; and nothing of the kind is here attempted. Indeed, so overwhelming is the evidence that, for the limited purpose of this digression, anything like elaborate investigation is not necessary.

It should be recalled at this point that the fundamental methodological conceptions which underlie the detailed theoretical argument of the preceding sections are two in number: (a) that the evolutionary or developmental factor in economic phenomena cannot properly be set in unrelated juxtaposition to the merely "mechanical", in such a manner that the science is compartmentalised into "statics", or "economic science proper", on the one hand, and a mysterious world of "exogenous" changes on the other;<sup>99</sup> and (b) that, in order to avoid a kind of superstitious abuse of the instruments of analytical mechanics, these must be regarded, especially from the standpoint of real, efficient economic causation (that is, of *explanation* of economic change, including "adjustment"), as duly *subordinate* to the at once diffused and unitary organic concept of the Whole.<sup>1</sup> Of these two basic

<sup>99</sup> "Statics" being taken to include not only the dissection of "the" stationary state as it conceptually exists in supposed comprehensive static perfection; but also the "Theory of Variations" regarded as a study of "passive adaptations" of a given system in "disequilibrium" towards a final position of "rest".

<sup>1</sup> And also of "partial wholes": that is, of subordinate, but complex, organic structures within the comprehensive Whole. The pragmatic significance, or scientific potency, of this concept—which so invariably

concepts, the second is in one sense the more fundamental—in that, by removing the misapprehension that “dynamic change” is to be conceived as the *result* of the mechanical interaction of specific functions (falsely reified as economic “atoms”),<sup>2</sup> it directs attention to the fact that a “static” as well as a “dynamic” condition is a property of the Whole itself—not a mere resultant of the mechanical interaction of the separate properties of its parts.

In turning to glance at the contemporary situation in other sciences, it is well worth while to note at the outset that the invasion of the “exact” and would-be “exact” sciences by the concept here described as the concept of “organic unity”<sup>3</sup> is no mere novelty of the last decade.<sup>3</sup> In concluding, twenty

irritates the naive “mechanist” into charges of “mysticism”—has been demonstrated by (but is by no means confined to) its ability to rescue us from the confused notion of an “instantaneous static curve”, which, I trust it has been particularly observed, ends no less certainly in atomistic disintegration because its votaries render pious lip-service to “complete mutual determination”.

<sup>2</sup> Cf. *supra*, p. 58. The falsely conceived “atoms” referred to are, of course, curves of aggregate demand and supply for particular commodities; not the individual *persons* who constitute a social economy. Reference has already been made to the fact (*supra*, p. 58, n.) that the concept of a unitary organic whole is by many persons more readily grasped in connexion with the system of preferences of a single person than with reference to the total social structure. Any more profound discussion of the philosophical problem here involved was not essential to the limited purposes of the preceding sections, since aggregate demand curves are simply summations of individual curves (in the sense that the organic social element is allowed for in the discussion of the individual curves themselves). But the larger problem is of profound significance in the study of Maximum Net Social Satisfaction through Time. It is enough here to remark that the *mode* of relationship of the individual to Society cannot be adequately conceived of “mechanically”—he is not an “atom”. See Whitehead, *op. cit.*, *passim*; and *infra*, 109n, 146, and 152-153.

<sup>3</sup> The concept itself (whose full implications are not adequately expressed by the terminology I have thought it necessary to employ throughout this discussion) is of course of vastly greater antiquity than modern science. A. N. Whitehead (*op. cit.*, *passim*) has recently re-emphasised

years ago, his massive survey of European thought in the nineteenth century, John Theodore Merz,<sup>4</sup> after emphasizing the immense difficulties in the way of reaching any generalised understanding of the deeper forces underlying the enormously complicated specialisation of modern philosophical and scientific activities, wrote:

Allowing, however, that there are to be found a few general tendencies in recent thought which distinguish it from that of the preceding age, . . . let us define . . . in a few words what these tendencies are.

First we have what I have termed the "synoptic" tendency of thought, the endeavour to reach a *vue d'ensemble*, a *Gesamtschauung*; and this quite as much when we have to deal with the totality of things as when we confine ourselves to specially selected regions of research. This synoptic view is complementary to, and has succeeded, the combined methods of analysis and synthesis which were introduced into philosophic thought under the influence of the natural and exact sciences in the earlier part of the nineteenth century; and here it is well to note that the latter themselves have, mainly under the influence of Darwinism, gathered renewed strength and vastly extended outlook by similar comprehensive methods.

with great profundity and originality the fact that it constituted the flower of Greek thought. Having developed with devastating and conclusive force the thesis that the mechanistic positivism which dominated nineteenth century scientific thought was conceptually shallow, crude and inadequate by comparison with the philosophical concepts of Plato, he remarks (p. 203): "The final problem is to conceive a complete fact." And "we can only form such a conception in terms of fundamental notions concerning the nature of reality. We are thrown back upon philosophy." Stressing the fact that it would, of course, be "most unscholarly to identify our modern notions" with the "archaic thoughts of Plato"—since "for us everything has a subtle difference"—he maintains that, nevertheless, "for all these differences, human thought is now endeavouring to express analogous elements in the composition of nature".

<sup>4</sup> *History of European Thought in the Nineteenth Century*, vol. iv, Blackwood, 1914. See chapter xii, especially pp. 786-7 and pp. 774-5.

In view of the importance which has been attached in this essay to the inherent dependence of "demand analysis" upon psychology, and upon an "organic" conception of the psychological processes underlying and abstractly expressed in demand "mechanics", it is worth while to quote further:

Secondly, this synoptic view has been nowhere more fruitful than when applied to psychological research. The view introduced by Locke and gradually developed by Berkeley, Hume, and Kant, that all our knowledge of the external world is made up of "ideas", now more correctly termed "presentations", found a forcible but extreme expression in an "Analysis of Sensations", and led through criticism to the recognition that such an analysis (including a subsequent synthesis) neglected to search for the original connection, the "Together" of these elements of cognition in consciousness. The synoptic view is not content with an analysis and synthesis of Sensations, but emphasises the continuum of these sensations or presentations within consciousness, and advances a step further by including in this continuum not only the sensational but also the emotional and volitional elements.

To inquire at all deeply into the progress of psychology during the twenty years since this was written, and into its condition at the present time with respect to the basic metaphysical concept here under discussion would clearly be impracticable in this place. To the zealously "professional" type of "economic theorist", modern psychology is a strange realm of warring "schools" and shifting "fashions", which he views from a discreet distance with mingled feelings in which fear of contamination and a pleasant sense of superiority are the most marked ingredients. Probably even an elaborate discussion would only succeed in convincing him that a fellow economist had tragically succumbed to the lure of one or other of these "schools"—and might therefore be expected thereafter to be a pest to "economic science".

Edgeworth has remarked of Marshall that he achieved "that rare thing—moderation in philosophy". It is possible that one of the greatest aims of the present generation of economists should be to learn to eschew lack of moderation in psychology. (And "excess", of course, here includes the "teetotalism" so dear to the Non-Conformist Conscience). I would therefore emphasise that while, if the substance of the foregoing section is valid, we unquestionably require, for the free development of economic "theory" itself, the cultivation of a field of Economic Psychology which is as indubitably "psychology" as it is "economics"; and while the best results will thus naturally be obtained by workers who are competent "psychologists" as well as "economists"; yet there seems to be no reason why such workers need set up at the very outset as professional Hedonists, Instinct Psychologists, or Behaviorists—in any sense of these overworked terms which implies dogmatic exclusiveness. Apart altogether from the perils inherent in subservience to the Fallacy of Dogmatic Finality, the nature of many of the initial problems, at least, is such that much substantial progress could be made with a minimum of reliance upon the various more or less systematic bodies of doctrine that coexist in modern psychology.<sup>5</sup> In a science in which understanding of its own psychological pre-supposi-

<sup>5</sup> I am aware that this remark runs the risk of being labelled as facile eclecticism. But it does not imply that. From the standpoint of economic science, the problems of economic psychology are necessarily *primarily* dictated by the "needs" of economics (though psychology has it in its power to indicate some very important needs of which economics is as yet almost entirely unconscious). Economists are prone to assuring "outsiders" that all modern schools of Value Theory are "fundamentally" identical. One need not accept this without reservations, and one certainly need not make any similarly sweeping claim with regard to psychological "schools", in order to hold that the latter are not all mutually exclusive. Cf. Woodworth, *Contemporary Schools of Psychology*, especially the last chapter.

tions has not gone far enough to enable it clearly to disentangle the finer distinctions between "hedonism" and mechanistic "atomism";<sup>6</sup> in which the general level of discussion of these pre-suppositions is such that we may at any time encounter without surprise sporadic assertions—based on rumours of a recent popularity of "Instinct Psychology"—that man is no longer a rational animal capable of conscious choice; and in which it is by no means unknown for quite reputable economists to deny that they have any psychological presuppositions at all: in such a science, anything more than this would at present be premature.

After these remarks, it may not be quite so perilous to call attention to the recent widespread prominence achieved in psychology by what is called "gestalt theory".<sup>7</sup> From the standpoint of the view I have taken in this essay of the *status* of abstract demand and supply curves (or functions) in economic science, the significant features (and I think they are also the intrinsically significant features) of Gestalt theory as a means of comprehending mental life are as follows:

✓ (a) Its emphasis on "organised wholes" as "prior" to,

<sup>6</sup> See *supra*, p. 64; which makes it plain that the relation of the former to the latter is that of species to genus.

<sup>7</sup> See Wolfgang Köhler, *Gestalt Psychology*, Horace Liveright, New York, 1929.

"In the German language . . . at least since the time of Goethe, and especially in his own papers on natural science—the noun 'gestalt' has two meanings: besides the connotation of 'shape' or 'form' as a *property* of things, it has the meaning of a concrete individual and characteristic entity, existing as something detached and *having* a shape or form as one of its attributes. Following this tradition, in *gestalttheorie* the word 'gestalt' means any segregated whole, and the consideration of *gestaltqualitäten* has become a more special side of the *gestaltproblem*, the prevailing idea being that the same general type of dynamical process which leads to the formation and segregation of extended wholes will also explain their specific properties." *Op. cit.*, pp. 192-3. The last six words are crucial. Cf. above, pp. 57-60.

and *determinative of*, the *specific* characters of their constituent elements, (b) its insistence on the *intrinsically* "dynamic" character of these "wholes".

This is what I have myself described as the "organic", as opposed to the "mechanical" or "atomistic", conception, in insisting that the real living economic process cannot properly be regarded as the *resultant* of the interactions of the various "elasticities"—which are rather themselves constituted, both individually and in their interrelations, by an *active* "whole".<sup>a</sup>

The concrete application of this can be very simply and briefly illustrated from the field of the psychology of

<sup>a</sup> It is extremely important to *keep* in mind (*cf. supra*, p. 101, n.) that there may be, and are, "wholes" within "the Whole"; and that these "wholes" are capable of surprisingly distinct delimitation. This has immense *practical* significance. Köhler writes: "Protesting against the atomism which had been introduced into the treatment of sensory experience, William James once said that, in the sensory field, local experiences are interwoven with their neighbours in a manner which is beyond the grasp of purely intellectual theory. He seems to think that, even in original sensory experience, there is uniform continuity and that all cuts and boundaries are introduced later on for pragmatic reasons. From the viewpoint of *gestalt* psychology such a statement does not correspond to the facts." Economists who are weighed down, in a crumbling world, by visions of the intolerable complexity of an "atomistic" economic "dynamics" should perceive the analogue and take heart. What is called "Institutionalism" is really this analogue in one of its aspects. But it has others—or the term must be so broadly read as to cover these. It is not always realised, I think, that Marshall's concepts of Joint and Composite Demand and Supply, and so on, fall in the same category. And the concept is capable of rich extension, not only to the study of the *direct* dynamic significance of sub-systems of wants; but also, on the side of Supply, to sub-systems of production whose components are in specially intimate organic relationship, and function in the economy as *direct* dynamical units. For yet another aspect of the same thing, see my discussion of *strategic categories*, Q. J. E. Nov. 1930, *loc. cit.* It is not too much to say that real progress in the new pioneer work in "monetary theory" depends upon ability to select progressively the *right* "wholes".

"sensation". The important points are: (i) that there is *not* "point to point correspondence", in sensory experience, with the elementary "physical" stimuli: the experience is constituted by the *whole* situation presented, in such a way that "physically" identical stimuli assume varying *specific* experiential forms as "elements" in the total experience according to the nature of the "whole" of which they form an element; (ii) that this phenomenon is *immediate*, and is not to be attributed to the superimposed labours of "higher" mental activities working upon original "sensations" which *do* have point to point correspondence with the "physical" stimuli: "sensory organisation is as natural and primitive a fact as any other side of sensory dynamics";\* (iii) that such "wholes", while not themselves "constructs" resulting either from mechanical interaction of sensory "atoms" or from the work of some *extraneous* "higher" mental activity acting upon these "atoms", are themselves also *immediately* "dynamic", in a manner which gives rise to "the theory of direct dynamical determination":

There is no mere sequence of indifferent events, connected indirectly. Each phase of what happens grows out of its predecessors, depending upon their concrete nature.

The reader will of course be keeping clearly in mind that the purpose of this section is simply to glance briefly at the contemporary situation in philosophical methodology and in other special sciences, to see if there is anything there which might afford a clue to the curious present-day revival of "atomism" and "static formalism" in economics. It is

\* Incidentally, it is of much interest to notice the claim made for gestalt theory that it transcends, and synthesises whatever there is of value in, both the old "opposites" of "introspectionism" and "behaviorism"; and that by "introspectionism" is here meant precisely that method which Merz referred to as the method of "analysis and synthesis"—now most significantly labelled "machine theory". See Köhler, Chapter IV.



not to "reason from analogy"; since the economic discussion of preceding sections stands on its own legs.<sup>10</sup> But in view of the conclusions we have reached concerning the true relations between economics and psychology, these striking conceptual parallels with the doctrine I have developed as to the relations of "statics", "dynamics" and "organics" are surely of more than merely curious interest.<sup>11</sup>

At biology we may glance more briefly. Since it is the "biological" view of economic science that is in dispute, no purpose would be served by an extended examination of the methods of biological science itself—at least without an exhausting philosophical examination of concepts, which would here be out of place. But perhaps just because it is the "biological" view of economics, as *opposed* to the "mechanical", that we are here considering, it may arrest the attention of "mechanistic" economists to note that biology itself has in recent years been engaged in sloughing off a moribund *mechanism*—in the sense in which (following a customary philosophical usage) I have employed that

<sup>10</sup> Whitehead remarks, however: "More important even than Occam's doctrine of parsimony—if it be not another aspect of the same—is this doctrine that the scope of a metaphysical principle should not be limited otherwise than by the necessity of its meaning." *Op. cit.*, p. 305.

<sup>11</sup> The gestalt concepts are of course developed with respect to "individual psychology", whereas the economist habitually thinks, in many connexions at least, of society in the large. I have already referred to the fact that many apparent difficulties in this respect result from a false conception of the relation of the individual to society—a problem reserved for discussion elsewhere. But this need not trouble the exponent of static formalism; since he is foremost in insisting, sometimes in curious connexions, that "social phenomena" are "in the last analysis the reflex of individual choice". Cf. Robbins, *op. cit.*, p. 69, n. The real point is that the errors of static formalism, including its inadequate conception of "society", are a direct consequence of its inadequate conception of the *individual*: that is, of the atomistic, mechanical character of the "subjective theory of value" on which its "non-psychological" economics is founded.

word to denote a disintegrative "atomism": and that the conception of "wholes" has recently become prominent in that science also—not as a revival of a sort of "mystical vitalism",<sup>12</sup> but rather as a necessary instrument towards the attainment of rigorous "scientific" comprehension. Thus, under the title "Must Biological Processes be Either Purposive or Mechanistic?", R. F. A. Hoernle says:<sup>13</sup>

The antithesis of mechanism and purpose is out of date. As was shown by the British Association Symposium on the 'Nature of Life', at Cape Town (July 1929), the battleground in biology is shifting. Evidences of this shift are:

(a) The substitution of the concept of the 'whole' for the concept of 'purpose': the battleground is one of mechanism v. holism. Purpose is a psychological concept hard to dissociate from consciousness. Whole is a concept equally applicable to plant, animal, man; to living body and living mind.

(b) The biological protagonists, Hogben for mechanism, Haldane for holism, argued primarily on methodological, not on metaphysical, grounds. . . . It is important to distinguish, on the one side, between mechanism as a methodological point of view and mechanism as a metaphysic (= materialism), and, on the other side, between holism as a methodological point of view and holism as a metaphysic. Taking this distinction for granted, the question asked, viz., 'Must biological processes be either purposive or mechanistic?' should receive the answer, '*Biological processes must be holistically conceived in order to be mechanistically studied.*'

But by 'mechanistically studied' is here meant not 'mechanistically' in the narrower and unsound sense which means purely in physico-chemical terms, but in the wider and sound sense of scientific or experimental determinism.

<sup>12</sup> Cf. Köhler, *op. cit.*, p. 146, on the same bugbear in psychology: "these concepts do not contain a single thought in the direction of vitalism." Italics his.

<sup>13</sup> *Proceedings of the Seventh International Congress of Philosophy*, Oxford University Press, 1931.

The principle of scientific determinism does not limit itself to the concepts of any one science, or to phenomena of any one kind, but leaves science free to establish for each kind of phenomenon its own appropriate concepts and laws.

It will be useful to supplement this admirable and pregnant summary with a quotation from another source<sup>14</sup> which brings out more clearly the crucial notion of an actual *active process* in which systems of "wholes" in dynamic development *produce* successively "higher orders" of "wholes" which, *though strictly and deterministically accountable for, analytically, in terms of the milieu out of which they emerge, nevertheless involve and display continual novelty*:

. . . the theory of emergent evolution, recently advanced by Professor Lloyd Morgan and Dr. S. Alexander . . . maintains that new and distinctive qualities are engendered on successive levels of existence in Space-Time. The reason is that certain collocations or 'constellations' of movements exhibit more advanced characteristics than do any of their separate constituents. Nevertheless, each part of the new complex retains the qualities appropriate to its own level of existence, while it functions in a process belonging as a whole to a higher level. Hence, 'each new type of existence when it emerges is expressible without residue in terms of the lower stage, and therefore indirectly in terms of all lower stages' of evolution.<sup>15</sup>

<sup>14</sup> "Emergent Evolution and Ethics", D. S. Mackay (University of California), in *Proc. of Seventh Int. Con. of Phil.*

<sup>15</sup> Alexander, *Space, Time and Deity*, vol. ii, p. 67.

The reader will note carefully that Alexander's phrase "expressible without residue" means "expressible *analytically* without residue". The "element" of "novelty" which resides in the "whole" is, in other words, *not* itself a true "element", but what I have called a diffused organic characteristic, which can be neither *localised* nor *detached*.

I may take this appropriate occasion to point out that the basic fallacy of the Austrian conception of "monetary neutrality" has a third aspect (for two others see p. 98, n. above) traceable to its methodological incapacity to employ the "biological" concept of "emergence". The

The suggestion of 'emergence', in this exact sense, comes from G. H. Lewes. In attempting to connect the highest mental phenomena with organic and physical conditions in one continuous series, he was led to reject 'the erroneous notion of causation as mere antecedence'. If consciousness is an emergence, rather than a mere resultant, from bodily states, then 'what we call the conditions are just the analytical factors we have detected in the fact.' On the other hand, 'what emerges is the *expression of its conditions*—every effect being the procession of its cause.'<sup>16</sup>

In referring to the doctrines of Lloyd Morgan and Alexander, we have already passed from the special fields of psychology and biology into the realm of "metaphysics", in which the doctrine of "wholes", of "organic emergence", and of "direct dynamical determination" receives generalised statement as a universal principle. Since the economist's interest is mainly directed towards human society, and since the illustrations from gestalt theory referred only to the psychology of sensation, we may fittingly conclude this portion of the discussion by directing attention to Wundt's much earlier formulation of the "law of creative resultants"

"institutional" development of "monetary" forms—all the way from direct barter, through indirect barter, a standard commodity-money, commercial credit, bank-notes and bank checks (perhaps "back again" to the sublimated "barter" of some future "Communist" economy)—is at each stage *analytically* "expressible without residue" in terms of the stage below: so that, in strict methodology, the belief that the novel characteristics that emerge at any stage are analytically detachable is an illusion; and accordingly the notion that "money" *can conceivably depart* from "neutrality" towards the "system" is devoid of rational content. This misconception is not confined to the Austrians; who have merely exhausted it with their customary perverse "rigour".

If, after a little reflection, any reader should find this doctrine continue to irritate, he may safely infer that, despite the preceding discussion, he has not really emancipated himself from the toils of an un-Marshallian, anti-Institutionalist, "mechanistic" economics.

<sup>16</sup> *Problems of Life and Mind*, vol. ii, pp. 411-412.

in psychology; and more particularly (since it pertains specifically to the realm of *Voluntary choice*) to the coordinate law which he formulated in his *Ethics*—the famous “law of the *heterogony of ends*”:

... that manifestations of will, over the whole range of man's free voluntary actions, are always of such a character that the effects of the actions extend more or less widely beyond the original motives of volition, so that *new* motives are originated for future actions, and again, in their turn, produce new effects.<sup>17</sup>

The economist's interest in the contemporary situation in physical science, as it pertains to the topic here under discussion, must of necessity be keen. Not, indeed, that the *absence* from physics, of the concepts we are here urging as vital methodological necessities for economics, would be a conclusive, or (after the foregoing discussion) even a weighty, consideration against this claim. But the *presence* in, and progressive utilisation by, physical science itself, of the “organic” concept—the concept of “diffused”, non-localisable characteristics of “wholes” as essential to the growth of “precision” and comprehensiveness of scientific explanation:—this would surely have an almost dramatic quality which could hardly fail to sweep away the last shreds of *morale* remaining to the exclusionist “mechanistic” economist. What do we find?

There is not far to look. Eddington, writing under the

<sup>17</sup> So cited by Mackay, *loc. cit.* Wundt himself (*Ethics*, vol. i, pp. 330-331) stresses (i) that the several means employed in voluntary action are never wholly congruent with the end-in-view, and (ii) that the extent of the incongruence is dependent (a) upon the length of the time-span required for the attainment of the supposed end, (b) upon the “extent” or generality of this end. There is food here for very deep reflection indeed by economists tempted to excessive self-confidence and equanimity in their attitude towards their own “expert” advisory functions.

significant caption "*The Scientific Reaction from Microscopic Analysis*", says:<sup>18</sup>

From the point of view of philosophy of science the conception associated with entropy must I think be ranked as the great contribution of the nineteenth century to scientific thought. It marked a reaction from the view that everything to which science need pay attention is discovered by a microscopic dissection of objects. It provided an alternative standpoint in which the centre of interest is shifted from the entities reached by the customary analysis (atoms, electrical potentials, etc.) to qualities possessed by the system as a whole, which cannot be split up and located—a little bit here, and a little bit there. The artist desires to convey significances which cannot be told by microscopic detail and accordingly he resorts to impressionist painting. Strangely enough the physicist has found the same necessity; but his impressionist scheme is just as much exact science and even more practical in its application than his microscopic scheme.

"Holism", gestalt theory, "Institutionalism"—and in physical science! The static "mechanist" in economics has been entertaining a Trojan Horse.

Nor is entropy an evanescent "fashionable" novelty of the last decade:

Thanks to clear-sighted pioneers in the last century science became aware that it was missing something of practical importance by following the inventory method of the primary scheme of physics. Entropy became recognised although it was not found in any of the compartments. It was discovered and exalted because it was essential to practical applications of physics, not to satisfy any philosophic hungering. But by it science has been saved from a fatal narrowness. If we had kept entirely ✓ to the inventory method, *there would have been nothing to represent 'becoming' in the physical world* (p. 104).

<sup>18</sup> *The Nature of the Physical World*, p. 103. Italics mine.

Here again, as in the psychological principle of "direct dynamical determination" as a corollary of gestalt theory; as in the methodological "holism" of scientific evolutionary biology; and as in my discussion of the *status* of demand and supply "functions"; the notion of a diffused, non-localisable, but essentially *unitary* principle lies at the very root of "dynamics". It is no accident that economic atomism would incarcerate "economic science proper" in "Statics". And the modern physicist, like the modern psychologist, the modern biologist, the modern economist, must, for his sins, reason patiently with the bedraggled survivors from the shipwreck of pseudo-"rationalism":

You may be inclined to regard my insistence that entropy is something excluded from the inventory of microscopic contents of the world as word-splitting. If you have all the individuals before you, their associations, arrangement and organisation are automatically before you. If you have the stars, you have the constellations. Yes; but if you have the stars, *you do not take the constellations seriously* (p. 106).

The emphasis on the *immediately dynamic* quality of the "holistic" conception of entropy—and on the impossibility of constructing a true "dynamics" without it—is even more striking:

I am afraid the average reader will feel impatient with the long-winded discussion I am about to give concerning the dynamic character of the external world. "What is all the bother about? Why not make at once the hypothesis that 'becoming' is a kind of one-way texture involved fundamentally in the structure of Nature?" . . . This is in fact the kind of idea which I wish to advocate; but the "average reader" has probably not appreciated that before the physicist can admit it, a delicate situation concerning the limits of scientific method and the underlying basis of physical law has to be faced. . . . Whilst the physicist would generally say that the matter of this familiar

table is *really* a curvature of space, and its colour is *really* electromagnetic wave-length, I do not think he would say that the familiar moving on of time is *really* an entropy gradient (pp. 90-95).

Entropy is an appreciation of arrangement and organisation; it is subjective in the same sense that the constellation Orion is subjective. . . . If colour is mind-spinning, so also is entropy a mind-spinning—of the statistician. It has about as much objectivity as a batting average (p. 95).

In . . . other parallelisms we find the objective in the scientific world and the subjective in the familiar world. But in the parallelism between entropy-gradient and "becoming" the subjective and objective seem to have got onto the wrong sides. Surely "becoming" is a reality—or the nearest we can get to a description of reality. . . . Having convinced ourselves that the two things are connected, we must conclude that there is something as yet ungrasped behind the notion of entropy—some mystic interpretation if you like—which is not apparent in the definition by which we introduced it into physics. In short we strive to see that entropy-gradient may *really* be the moving on of time (instead of *vice versa*) . . . I would note that this exceptional appearance of subjective and objective apparently in their wrong worlds gives food for thought. It may prepare us for a view of the scientific world . . . which is much more subjective than that usually held by science (pp. 94-96).

Of those who are tempted in the name of pseudo-rationalism to eject this concept from physics as "subjective", Eddington demands that they show their good faith by "reversing the dynamic quality of time", and

. . . just for a change, give us a picture of the universe passing from the more random to the less random state. . . . If you are a biologist, teach us how from Man and a myriad other primitive forms of life, Nature in the course of ages achieved the sublimely simple structure of the amoeba (p. 91).

But, in real strictness, "unbecoming" is as forbidden as



"becoming" to the atomist, who must have recourse to an *instantaneous* Space-Time Continuum—in which, because there is no "entropy", there can be no entropy-gradient; and therefore no "becoming" or "unbecoming"; and therefore no "direction" in Time; and therefore no *Time*. For the *so-called* "Time dimension" is *then* nothing but a "Space" dimension. We are left with a *four-dimensional* instantaneous "statics", which leaves "the external world without any dynamic quality intrinsic to it."<sup>19</sup>

The more thin-skinned type of professional philosopher may wince over Eddington's method of posing the "subjective-objective" problem; but this method reveals with remarkable clarity, and from an immanent standpoint, the intellectual confusion that lies in wait for atomistic positivism everywhere. The sweeping away of this confusion by the introduction of such intrinsically dynamic and directional "holistic" concepts as "entropy" and "entropy-gradient" is reminiscent of the transcendence by gestalt theory of the "opposition" between machine-theory introspectionism and atomistic behaviorism. The frivolous shadow-sparring in economics between "statistical empiricism" and "precise statics" can likewise be swept away, to make room for serious work, by the introduction into econ-

<sup>19</sup> It is this fantastic Nemesis of pseudo-rationalism (which assumes a myriad forms, and can be instanced from practically every modern science) which long ago led to the so-called "reaction against Reason" —by Bergson and others. Pseudo-rationalism is not the brave guardian of the Citadel of Reason; but the traitor within the gates. For evidence that this jejune "dilemma" is no longer a vital issue in modern philosophy, see Professor Dewey's review of Professor Whitehead's *Adventures of Ideas*, in *The New Republic*, April 19, 1933.

It is interesting to notice Professor Robbins' approving citation (*op. cit.*, p. 98) of "Strigl's expressive phrase" describing "individual valuations and technical facts" as constituting "the *irrational* element in our universe of discourse".

omics also of intrinsically dynamic and directional "holistic" categories.<sup>20</sup>

<sup>20</sup> The reader will note not only that the "holistic" concept of entropy has "statistical" affiliations, but also that, in physics, "it now seems clear that we have not yet got hold of any primary law—that all those laws at one time supposed to be primary are in reality statistical." (*Eddington*, p. 98.) As I have on two former occasions pointed out (*Am. Ec. Rev. Sup.*, March, 1930, p. 37, and *Q. J. E.*, Nov., 1930, pp. 88-87, n.), careful economists have long in effect insisted on the underlying "statistical" character of their "analytical" law of demand—in recognising the "freedom" and possible caprice of the individual. The long period analysis of demand offered above reveals a greater possible scope for real individual freedom, and in a more concrete way. Professor Robbins therefore errs (*op. cit.*, Chapter V) in opposing "analytical" to "statistical" laws: an error which leads him, while proclaiming the eternal and universal validity of the declining demand curve, at the same time to understate the possibility of stability over considerable periods of approximate degrees of elasticity of demand for some commodities. Despite the conceptual difficulties (from which, in the light of this discussion, it would seem that "analytical" concepts are not immune), Marshall obviously believed in the possibility of serious work here. And there seems to be no reason (apart from a false antithesis of "introspectionism" and "behaviorism") why social psychology and "statistics" should not cooperate. What really happens when they do, is that "statistical" methods are employed to check and perhaps increase the "precision" of analytical "laws"—which are at bottom "statistical"! Professor Robbins seems to underestimate, too, the practical utility of such work, being apparently unaware (pp. 99-100) that it is for many large enterprises a financial necessity. And in denying (what would scarcely be affirmed) that some fantastically precise figure extracted from particular data is an (as it were) "empirical" constant for all time, he seems to lose his hold a little on the truth (so dear in other connexions) that all "laws" are strictly hypothetical. Nor will his mere *ipse dixit* suffice to establish the highly questionable universal proposition that "more complex phenomena" ("price fluctuations, cost dispersions, business cycles, and the like") are necessarily less stable in their characteristics (how "defined" we are not told) than single demand functions. It is hardly necessary to remark that in opposing, to Professor Mitchell's remark that "the distribution of the observations around their central tendency is a matter of much theoretical interest", his own assertion (p. 103) that "if there is any significance at all in bringing them together, it must be by way of contrast", Professor Robbins has completely missed the

Before leaving the subject of physical science, I cannot refrain from offering the reader a highly condensed "analogy" to which he may attach as much or as little importance as he pleases. It will be remembered that, in discussing "the" stationary state, I urged

(a) conceptual distinction of a "really stable" stationary general equilibrium from one which is stationary only so long as nothing happens to disturb it;

(b) the view that, in an expanding (or contracting) economy, long period adjustments throughout the entire economy were themselves the motive power of dynamic expansion of the economy as a whole;

(c) that in such an economy a "theoretically perfect" long period is in consequence a contradiction in terms.

In the face of protests from the static precisian that such Marshallian "looseness" is unworthy of "exact science", it is comforting to observe that the modern physicist seems to suffer from at least apparently analogous difficulties. In his later work, *The Expanding Universe*, Eddington tells us

point: and illustrated his own curious predilection for the fallacy of false opposition.

It can scarcely be over-emphasised that effective cooperation between, and scientific integration of, the "empirical" work initiated by Mitchell and the "theoretical" developments initiated by Robertson and Keynes, depends upon the right progressive selection of "real wholes" within the Whole, and in increasingly accurate estimation of the kinds and degrees of real interdependence among them. This may appear a mere empty exhortation to cultivate "right theory": but it is right theory of a kind that cannot be conjured out of the void; and the very generality of the language I have used is designed to call attention to the most intriguing thing about "the breakdown of mechanism"—our inability to set *a priori* limits to the variety and character of significant "wholes". To deny that the functionally significant characteristics of some of these wholes will, in their abstract mathematical expression, most conveniently assume the form of averages and dispersions, would therefore be folly: and so, I believe, would be dogmatic assertions that *all* significant wholes must receive this form of mathematical depiction.

(a) that the original Einstein and de Sitter universes were both static—the former containing “matter but no motion”, the latter “motion but no matter”. But “we have now realised that the changelessness of de Sitter’s universe was a mathematical fiction. Taken literally his formulae described a *completely empty* universe”. With the injection of just the right quantity of “matter”, we get the more substantial but *unstable* static Einstein universe. Alter the quantity of “matter” a little in either direction, and the Universe begins to “expand” or “contract” under its own steam.

(b) that ‘light’ “*could go right round*” the static Einstein universe; but that, in an expanding universe, “light is like a runner on an expanding track with the winning-post receding faster than he can run”.<sup>21</sup>

The reader will notice that I do not provide a physical parallel to the immediately dynamical, self-generative character of the Marshallian “long period”. The limited character of my researches probably places the physicist at an unfair disadvantage. I can only quote the following:

It would seem that the expansion of the universe is another one-way process parallel with the thermodynamical running-down. One cannot help thinking that the two processes are intimately connected; but, if so, the connection has not yet been found (p. 175).

However, there are doubtless grounds for optimism: for, in modern physics, no less than in modern economics, *Institutionalism* is unquestionably still in its infancy.<sup>22</sup>

<sup>21</sup> *Op. cit.*, pp. 62-93 and 104-105.

<sup>22</sup> I cannot refrain, either, from directing the attention of the philosophically-minded to a further curious parallel between the Marshallian long-period curve and the physical phenomenon of “light”. I have already called attention to the confusion of the “instantaneous static curve” with the “theoretically perfect” long period curve; and have further remarked that, *if man were omniscient*, the true long-period curve might conceivably be “instantaneous”. Now the curious behaviour

Sternly repressing the powerful temptation to demonstrate in tedious detail the perfect non-existence (emptiness) of "the" Stationary State in which "perfect Competition" is perfectly ubiquitous;<sup>28</sup> let us now, with the sublime im-

of light, in *uniformly* outdistancing *all* pursuers, whatever their own speeds, naturally suggests to the uninitiated that his youthful belief that light itself requires "time" to travel may be defective, and that it may be *more really* true that it requires no time at all. But I am competently assured that *both* these "attitudes" are efficiently transcended and reconciled in modern general relativity theory.

<sup>28</sup> Reminiscently of Hegel (and, for that matter, of Plato), Eddington remarks: "To my mind *undifferentiated sameness* and *nothingness* cannot be distinguished philosophically. \The realities of physics are *unhomogeneities*, happenings, change. \ Our initial assumption of a homogeneous static medium is no more than a laying out in order of the conceptions to be used in our analytical description of the distinguishable objects and events whose history we are going to relate" (p. 82). While the matter cannot be pursued here, the reader may quite profitably meditate briefly on the questions: (a) 'how far it is true that a little "Monopoly" must be introduced into the economic void in order to have an economic *world*; (b) if a judicious admixture of "competition" and "monopoly" yields a substantial, but unstable, static economic universe; and if the (original creative) injection of a little *more* "competition" (or is it a little more "monopoly"?) into the system set it "moving" under its own steam; whether it follows that we must resign ourselves to the ultimate dramatic bursting of the bubble in the red flare of revolutionary transition to the perfectly "monopolistic" (or is it perfectly "competitive"?) "death" of the Communist World State.

And he may consult Marshall, Letter to J. B. Clark, dated 11. ix. 02, in *Memorials*, p. 414: "... before 1870... I... believed it was possible to have a coherent though abstract doctrine of economics in which competition was the only dominant force... and I now regard that position as untenable *from an abstract as well as from a practical point of view.*" Italics mine.

But why "shirk" these "heroic abstractions"? Unless, indeed, it be on the ground that they are "sheer metaphysics".

To those hard-headed readers who abhor "verbalistic folly" like the foregoing, I ought perhaps to point out that they may find a kindred spirit in Professor Fetter. See his *Masquerade of Monopoly*, especially pp. 347-350. And my "Modern 'Monopoly' as 'The Gentleman Crook'", *Political Science Quarterly*, June, 1933.

pertinence characteristic of the non-mathematical "philosopher", pass on to consider—very briefly and circumspectly, however—the "Queen of the Sciences".<sup>24</sup> For it may well be that the foregoing scandalous disclosures have destroyed the touching faith of the static mechanistic economist in physical science; and that he has already turned for final solace and guidance to Pure Mathematics.

In an epoch remarkable (it is said) for the curious (and surely repulsive?) superstition that God is a Mathematician, it is a little difficult to realise that Mathematics too has known the meaning of persecution, even in quite recent times. And it may at first sight offer hope to the static mechanist to learn that the alleged grounds for this persecution are identical with those urged by his enemies against himself. Thus Huxley once remarked of mathematics that it "is that study which knows nothing of observation, nothing of induction, nothing of experiment, nothing of causation".<sup>25</sup> The really interesting points, however, are (a) the fact that the charge has been warmly repudiated, and (b) the grounds of the repudiation.

Some people have been found to regard all mathematics, after the 47th. proposition of Euclid, as a sort of morbid secretion, to be compared only with the pearl said to be generated in the diseased oyster, or, as I have heard it described, 'une excroissance malade de l'esprit humain' . . . and a very clever writer in a recent magazine article expresses his doubts whether it is, in itself, a more serious pursuit, or more worthy of interesting an intellectual human being, than the study of chess problems or Chinese puzzles. What is it to us, they say, if the three angles

<sup>24</sup> Mathematics, according to Gauss, is "the Queen of the Sciences, and arithmetic the Queen of Mathematics. She frequently condescends to do service for astronomy and other natural sciences, but to her belongs, under all circumstances, the foremost place." Quoted by Merz, *op. cit.*, vol. ii, p. 631.

<sup>25</sup> Merz, *loc. cit.*, p. 630.

of a triangle are equal to two right angles, or if every even number is, or may be, the sum of two primes, or if every equation of an odd degree must have a real root? How dull, stale, flat, and unprofitable are such and such like announcements!

. . . But this is like judging of architecture from being shown some bricks and mortar, or even a quarried stone of a public building, or of painting from the colours mixed on the palette. . . . The World of ideas which it discloses or illuminates, the contemplation of divine beauty and order which it induces, the harmonious connexion of its parts, the infinite hierarchy and absolute evidence of the truths with which it is concerned, these, and such like, are the surest grounds of the title of mathematics to human regard, and would remain unimpeached and unimpaired were the plan of the universe unrolled like a map at our feet, and the mind of man qualified to take in the whole scheme of creation at a glance.<sup>26</sup>

But similar charges have been made (and rebutted, to similar effect, though naturally with less lofty passion) against the humble Aristotelian Syllogism; and, so far, this language might *almost* be utilised by an "Austrian" economist to drown the ribald jeers of the intoxicated servants of the Marxian Dialectic.<sup>27</sup> But this is not all.

We may notice in passing how Merz<sup>28</sup> illustrates the doctrine that "every mathematical instrument, when applied to a novel purpose . . . 'derives as much benefit in its

<sup>26</sup> Professor J. J. Sylvester, Address before the First Section of the British Association at Exeter, 1869.

<sup>27</sup> See the entertaining and bracing work, *The Coming Struggle for Power*, by that engaging if precocious young Revolutionary Communist, Mr. John Strachey. See especially the chapter "Back to the Market?", which concludes: "Even the ghosts of these ideas do not walk. They are laid for ever. For the epoch of human history and the material conditions which alone gave them life have passed away down the irreversible stream of time. Only the least historically minded men on earth, only English economists, could dream of their resurrection."

<sup>28</sup> Vol. ii, chapters xii and xiii. Published in 1903.

development as it confers through being made use of", by showing how Galton, in his pioneer labours "to bring method and order" into the type of questions, biological and social, raised and suggested by the Darwinian theory of evolution, "was . . . able to put novel problems to the mathematician."

To understand this point we must realise the great difference which exists between dealing with a vast number of lifeless and of living units. This difference becomes evident if we consider that in the former case the number of units is unalterable and the units are indestructible; in the latter the elements or units are subject to enormous increase and corresponding destruction, generally with a preponderance of the first. . . . In the vast crowd of gemmules which build up a new organism or regenerate an existing one, we have to deal with a continual influx or creation of new units and a continual extinction and ejection of old or dead ones. Without venturing on any theory as to how this state of things has come about, we may see that the mathematics and statistics of such crowds must be different from those referring to stable, lifeless assemblages.<sup>20</sup>

But this is now, of course, common knowledge; and with Merz himself it is only preparatory to a discussion of further, and—to non-mathematical students of economics—much less familiar affiliations of pure mathematics with "organic" concepts. For my primitive purpose, I merely offer the non-mathematical reader a few brief quotations:

Out of these earlier algebraical and later combined algebraical and geometrical investigations, a novel and very useful point of view has been gradually gained which represents the most general conception of mathematical tactics. This centres in the notion of a group of elements. These elements may be quantities or operations, so that the theory of Groups embraces not

<sup>20</sup> He adds: "We owe it to Professor Karl Pearson to have first grasped clearly and comprehensively the mathematical problem involved, and to have solved it in a manner useful for biological research."



only the doctrines which deal with quantities but also those which deal with arrangements and their possible changes. The older combinatorial analysis dealt mainly with assemblages of a quantity of separate elements, their number, their variety: the modern theory of groups deals rather with the processes and operations by which different arrangements can be transformed one into the other. It is an algebra of operations.<sup>20</sup>

Can it be that the static atomistic formalist in economics is confronted with Evolutionary Institutionalism in the pulpit of the Cathedral of Pure Mathematics? If so, this is indeed the most unkindest cut of all.

It is in this connexion of particular interest to the philosophically-minded economist to note that fundamental methodological controversy can arise even within the science of "pure" mathematics itself; and this interest will certainly not be diminished when he learns of the general nature of the controversy:

I refer to the tendency expressed in its extreme form by the late Professor Kronecker of Berlin, to reduce all mathematical conceptions to the fundamental arithmetical operations with integral numbers, banishing not only all geometrical and dynamical conceptions, such as those of continuity and flow, but also such apparently algebraical notions as those of irrational and complex quantities. This attempt is an outcome of the school of Weierstrass, which has done so much to banish vagueness and introduce precision into modern text-books. (pp. 738-9).

<sup>20</sup> P. 689. Elsewhere (p. 649), Merz speaks of "the radical change which has taken place in recent mathematical thought . . . which can be explained by saying that the science of Magnitude must be preceded by the doctrine of Forms or Relations, and that the science of Magnitude is only a special application of the science of Forms." Discussing the development of this notion, he adds: "In quite recent times Mr. A. N. Whitehead has conceived 'mathematics in the widest signification to be the development of all types of formal, necessary, deductive reasoning', and has given a first instalment of this development in his 'Treatise on Universal Algebra' (vol. i, Cambridge, 1898)."

Concerning this school of mathematical thought, Paul Du Bois-Reymond significantly said :

The separation of the conception of number and of the analytical symbols from the conception of magnitude would reduce analysis to a mere formal and literal skeleton. It would degrade this science, which in truth is a natural science, although it only admits the most general properties of what we perceive into the domain of its researches, ultimately to the rank of a mere play with symbols, wherein arbitrary meanings would be attached to the signs as if they were the figures on the chessboard or on playing cards. However amusing such a play might be, nay, however useful for analytical purposes the solution would be of the problem,—to follow up the rules of the signs which emanated from the conception of magnitude into their last formal consequences,—such a literal mathematics would soon exhaust itself in fruitless efforts ; whereas the science which Gauss called with so much truth the science of magnitude possesses an inexhaustible source of new material in the ever-increasing field of actual perceptions (p. 739).

Even more weighty, if possible, were the words of F. Klein :

Whilst I everywhere demand the fullest logical elaboration, I at the same time emphasise that *pari passu* with it the intuitive representation of the subject should be furthered in every possible manner. Mathematical developments which have their origin in intuition cannot count as a firm possession of science unless they have been reduced to a strict logical form. On the other side, the abstract statement of logical relations cannot satisfy us until their importance for every form of representation has been clearly demonstrated, so that we recognise the manifold connexions in which the logical scheme stands to other departments of knowledge according to the field of application which we select. I compare mathematical science to a tree which stretches its roots ever deeper into the soil, and at the same time expands its branches freely upwards. Are we to consider the

root or the branches as the more important part? The botanist will tell us that the question is wrongly put, and that the life of an organism consists in the interaction of its various parts (p. 740).

Merz concluded his history of nineteenth century mathematics thus :

Most of my readers will no doubt agree with this view. Indeed the perusal of the foregoing chapters must have produced on their minds the conviction that, so far as the advance of science and also of mathematics is concerned, it largely depends upon the introduction of different aspects leading to different courses of reasoning. The unification of all of these into one consistent and uncontradictory scheme, though it remains a pious hope and far-off ideal, has not been the prominent work of the nineteenth century. Rather, wherever it has been attempted, it has had a narrowing effect, and has resulted in a distinct curtailment of the great and increasing resources of Scientific Thought<sup>21</sup> (p. 740).

I lack both the knowledge and the resources to give the reader any indication whatever of the further development of mathematical science; and will merely close with a very recent quotation from one of the greatest of living mathematical thinkers.<sup>22</sup>

There is thus an analogy between the transference of energy from particular occasion to particular occasion in physical nature and the transference of affective tone, with its emotional energy, from one occasion to another in any human personality. The object-to-subject structure of human experience is reproduced in physical nature by this vector relation of particular to particular. It was the defect of the Greek analysis of genera-

<sup>21</sup> What economist will affirm that economic science is a solitary and conspicuous exception?

<sup>22</sup> Whitehead, *op. cit.*, p. 242.

tion that it conceived it in terms of the bare incoming of novel abstract form. This, ancient analysis failed to grasp the real operation of the antecedent particulars imposing themselves on the novel particular in process of creation. Thus the geometry exemplified in fact was disjoined from their account of the generation of fact.<sup>23</sup>

The application of this profound metaphysical (and—evidently—mathematical) truth to the basic problem of economic methodology with which this essay is concerned should now be apparent. If Alfred Marshall believed it to be in the best interests of economic science, in his day and generation, to cloak the “armour of mathematics” in the “garb of literature”;<sup>24</sup> if he carefully avoided the spurious “precision” of “the” stationary state, with its “potty scraps of calculus” and its apparatus of “endogenous” and “exogenous” changes: does it not now seem barely possible that his procedure was dictated by no mere “shirking” or “temperamental aversion”; and that, beneath his circumspect treatment of these mock-heroic abstractions there is to be discerned the conscious (and “steadfast”) purpose of a powerful and profound mind? Shall we any longer entertain as a serious scientific possibility the blasphemous proposal that its “biological analogies” be stripped like excrescences from his noble and vitalizing masterpiece to make vulgar holiday for two-dimensional amateur geometers?

There can be no better place than this in which to notice

<sup>23</sup> My conviction grows that, so long as the static mechanist in pursuit of Precision refuses the experience of a major intellectual Conversion, he will find no sanctuary here. *Not every one that saith unto me, Lord, Lord, shall enter into the kingdom...* and I conjecture that, when the homeless static mechanist makes forlorn but hopeful petition at these austere Portals, he will meet with the implacable reply: *I never knew you. Depart from me, ye that work iniquity.*

<sup>24</sup> Edgeworth, in *Memorials of Alfred Marshall*, loc. cit.

one matter of terminology, which is of great importance, not only to a proper understanding of the discussion of this essay, but in general—if much wasteful confusion of thought among non-mathematical economists is to be avoided. This is in regard to the term “mechanics”. Used in the sense in which I have employed it to speak of the “mechanics” of organic economic growth and change, it indicates merely the abstract *form*, the kinetics, the *mathematics*<sup>55</sup> of these concrete processes. But it is now evident that this does not correspond with the adjective “mechanical” in the sense in which that term is customarily employed to indicate an exclusionist “statics” or a “machine-theory” conception of the “pure form” of concrete organic processes. For the pure form of these processes is not, in this sense, “mechanical”: and modern mathematics itself, it would seem, is “organic.” Of the static mechanist in economics we may therefore safely say that his very geometry is radically inadequate.<sup>56</sup> Present-day non-mathematical economists may therefore naturally look with confident expectation to the modern “high-powered” mathematical economist to play a prominent part in the difficult task of developing a more adequate “economic biology”.

It is the basic thesis of the earlier sections of this essay that, if economic “science” cannot be confined in a “static” straightjacket, neither can it, as has recently been suggested, be made a purely formal science of implications: and for the same reason. For the development of economic “dynamics”

<sup>55</sup> The modern *opposition* of “mathematical” to “mechanical” by physical scientists will be familiar to readers of Sir James Jeans’ *The Mysterious Universe*. See especially pp. 146-148 in the First Edition.

<sup>56</sup> The philosophical purist may indeed object that he is probably not, “in strict methodology”, entitled to the possession of any geometry at all. For we have seen that one of the enterprises of his prototype within mathematics was to endeavour to cast geometrical concepts out of mathematical science as “impure”.

is the progressive penetration by economic science into the "territories" of its neighbours. Only in this way can we escape the dilemma of pure formalism, and select what is *relevant* from among an infinitude of possible abstract systems. Here, again, we can count on the support of the genuine mathematician :

(But a complete existence is not a composition of mathematical formulae, mere formulae. It is a concrete composition of things illustrating formulae. There is an interweaving of qualitative and quantitative elements. For example, when a living body assimilates food, the fact cannot be *merely* that one mathematical formula assimilates another mathematical formula. The fact cannot be merely that the equality of two and three with five assimilates the fact of the equality of thrice three with nine, nor can the number eleven assimilate the number sixteen. Any of these mathematical notions may be illustrated, but the fact is more than the formulae illustrated.

The final problem is to conceive a complete fact. We can only form such a conception in terms of fundamental notions concerning the nature of reality. We are thrown back on philosophy.\*

I make no apology for this excursion into a number of other fields of intellectual activity: nor even for the copious extracts from readily accessible standard works with which I have illustrated it. The justification or otherwise of this procedure turns entirely upon the success I have attained in giving the professional economic reader some inkling of the meaning of the intimate relationships between the development of his own science during the last fifty years and the broad movement of human thought in general during the

\* Whitehead, *op. cit.*, p. 203. Investigation of the far-reaching importance of this for "economic science" belongs to the study of Maximum Net Social Satisfaction through Time. It calls in question the whole structure of a normative economics formulated in terms of "costs" and "satisfactions" *atomistically conceived*.

same period; and in thereby increasing his own ability to distinguish between the spurious and the real within his own specialism. For the static, mechanistic reaction in economic science is no straw man, and there is considerable evidence that it has been gaining in strength in recent years. This may be in part attributable to the fact that Marshall "never explains himself": but it is even more attributable, I fear, to that most disturbing present-day phenomenon—the appalling intellectual provincialism of so much learned economic anti-quarianism. It is at least as true today as in the time of J. S. Mill that "a man is not likely to be a good economist if he is nothing else."

## XI

"The stars are not pulled this way and that by mechanical forces; their's is a free motion. They go on their way, as the ancients said, like the blessed gods."<sup>88</sup>

This sounds particularly foolish even for a philosopher; but I believe that there is a sense in which it is true."

*A. S. Eddington.*

I have purposely reserved for this stage of the discussion a few final explanatory remarks concerning the doctrine of the essential relativity of the distinction between "static adjustment" and "dynamic change". These remarks have reference to one or two possible difficulties or objections, to the thorough-going introduction of this doctrine into economic science, which it is highly desirable to mention specifically because they have already found expression in economic literature in connexion with arguments designed to prove that "scientific" handling of "exogenous" changes is inherently impossible. Discussion of these objections will introduce (with one possible exception<sup>89</sup>) no basic concepts which have not already been introduced, either explicitly or implicitly. But it may none the less be helpful: and a major function of the digression of the preceding section is to make possible a much briefer and more effective handling of these questions than would otherwise have been practicable. Even so, it is evidently quite impossible to anticipate and deal with every possible "case", and to show that it is quite "consistent" with the thorough-going methodological position here advocated. But readers who are impressed with the manner in which the inner necessities of rational

<sup>88</sup> Hegel, *Werke* (1842 Ed.), Bd. 7, Abt. I, p. 97.

<sup>89</sup> See below, pp. 142-148.



and coherent thought itself have forced the development of a conceptually identical point of view in one field of intellectual activity after another, will doubtless be prepared to go to a little trouble to think the matter out more concretely for themselves with regard to whatever particular concrete "cases" present themselves to them.

First, I think, it is a little difficult to overcome the feeling that certain factors, commonly referred to under the heads of (a) "new inventions", and (b) "political" and "social" changes, are in some way "*radically*" distinct from the factors abstractly depicted by the demand and supply curve apparatus: and this feeling is likely to persist or recur, too—despite the discussion of demand analysis in earlier sections—with regard to those more pervasive and striking changes in "fashion" and "taste" that occur so "capriciously" from time to time. Concretely regarded, the field of social change, thus barely indicated, is so wide and complex, and, for that matter, so much of a *terra incognita* to the "economic theorist", that actual examination of it is here impossible. I can only indicate what I hold to be, by the inner necessity of Reason itself, the right *attitude* with which to approach such studies—the attitude dictated by a really thorough-going logical "precision". I shall not venture into amateur sociology!

We have already<sup>40</sup> noticed that the *true* conception of "exogenous" change, from the standpoint of an organic conception of the science, is that of independent environmental change in the sense of changes in the environment of the organism which are not themselves the result of the action of the organism itself upon its environment: and that independent changes in the physical environment of human society can be unambiguously placed in this category. The question as to whether the "economic organism" may also

<sup>40</sup> *Supra*, pp. 36-38.

experience independent environmental changes occurring in *Society* was left moot; and something further must now be added on this point. The non-existence of "exogenous" changes of this kind naturally cannot be demonstrated by a process of exhaustive "induction"; but the belief in their existence can, I think, be very briefly shown to involve logical error in terms of the definition of "economics" accepted even by those who suppose there are such "exogenous" social changes.

It is generally agreed that the "subject-matter" of economics cannot be mechanically segregated by "classificatory" methods from the subject-matter of the other social sciences: the distinction of the various sciences results from their differences of *attitude* towards a common subject-matter.<sup>41</sup> From the standpoint of an "organic" conception of *Society*, therefore, it is inherently illogical to attempt to effect any *clean-cut* segregation of different *aspects* of the organism one from the other.<sup>42</sup> The "economic system" is only one *aspect* of the "social organism"; and the notion of *completely* "independent" technological, political, or social change is therefore inherently illogical.

With regard, first, to "Inventions", it is urged by Schumpeter<sup>43</sup> that what is important — for economics — is not "invention" but *utilisation*. This is of course in a sense true; it is not intended, I think, to provide any basis

<sup>41</sup> Even the Austrians, illogically enough, admit this. See Robbins, Chapter I. They attempt to achieve consistency by trying to segregate the various *attitudes* in watertight compartments.

<sup>42</sup> It is not illogical, however, to distinguish *relatively* "independent" *partial* "wholes" within the social whole—exhibiting, as wholes, *relatively high degrees* of dynamic independence. The pathological element creeps into methodology only when it is sought to give complete rational coherence to these partial wholes in defiance of the measure of interdependence that does exist among them.

<sup>43</sup> *Economic Journal*, *loc. cit.*, p. 378.

for clean-cut segregation of an "independent" "purely economic" process within the wider social whole. This is no place to enter into a history of technological invention itself; but it may be suggested that such a study would reveal (a) that specific technical inventions "normally" occur in a social milieu which is already prepared to receive them—a fact which has frequently been copiously illustrated for the period from the Industrial Revolution downwards; and (b) that many curious instances of *sporadic* "pre-mature" inventions can doubtless also be found; but that, in such cases, they have not been "utilised" and have therefore been "lost" again.<sup>44</sup>

In the matter of "utilisation" itself, regarded as illustrative of a "special mechanism", something has already been said above;<sup>45</sup> and a few additional remarks will be added here. These will, however, be brief, since further development of the subject would anticipate the discussion of Freedom at the close of this section. We have already seen reason for believing that there is perfect psychological "continuity" from the most seemingly "mechanical" ("automatic") "responses" to "stimuli" to the most conspicuous examples of high creative faculty. This means not so much that the latter may be "reduced" to "mere response" as that even the most seemingly "mechanical" of "responses" may conceal a "creative" implication.<sup>46</sup>

<sup>44</sup> It may be remarked that the "truistic" character of this assertion is methodologically identical with the admittedly "truistic" character of statical "necessary laws": the only difference, from an ultimate logical standpoint, being that we are here in a universe of discourse more richly endowed with "dimensions" than the universe of the static formalist. If the truth of this statement is not already apparent, I trust the remainder of this section will make it so.

<sup>45</sup> *Supra*, pp. 34-35, and 42-43, n.

<sup>46</sup> This language may suggest evasion. But the point it is intended to recall to the reader's mind is that (a) while emergent change is not

But it is none the less conversely true that even the most constructively creative activity is, in the strictest sense of the term, a "response". It is an "equilibrium adjustment".

. . . Though a pendulum will generally swing clear backwards and forwards along the same line (said Marshall), yet, if a clock is standing on an inclined ledge, the vibrations of the pendulum may make it slide downwards towards a final catas-

strictly localisable, (b) neither is its absence: stable equilibrium is likewise a characteristic of the whole. It is most important to realise that the position here adopted does not involve denial of the importance of "habit", "custom", "routine": merely an attempt to define with true precision their relation to creative emergence. Cf. Whitehead, *op. cit.*, p. 114: "It is the beginning of wisdom to understand that social life is founded upon routine. . . . So many sociological doctrines, the products of acute intellects, are wrecked by obliviousness to this fundamental sociological truth." But this is not to deny that emergence, whether spectacular or imperceptible, is "holistic".

If we suppose an individual on a fixed income and with a well-defined standard of life, then he is obviously in one sense severely limited as regards the character of the *immediate* responses he is free to make to relative price changes affecting that standard. But we cannot segregate static psychological adjustments here from emergent psychological change. A really static psychic whole would only be possible in a society which was also static. Even in the case of a highly stable, habit-ridden individual, such price changes will force *some* "emergent" changes; and it is precisely in such a case that it will be least possible to appear to trace these changes to the localised stimulus of a single price change. These reflections suggest that, so far from more "violent" stimuli having a *shorter* "long period" response, the long period would in such cases actually be much longer. (*Supra*, pp. 86-88.)

In the converse case of an individual *least* ridden by habit, it may seem that the stimulus to creative emergence will be more consciously "localised". But this notion may be pursued to two widely opposed limiting cases: that of mere undisciplined impulse which reacts capriciously to isolated stimuli; and that of the "creative entrepreneur". But the distinguishing characteristic of the latter is precisely that he thinks and acts in "holistic" terms: both the stimulus and the response are then holistic, precisely as in the case of the victim of routine. It is the range and quality of the "whole" that is different.

It may be added that "invention" itself has become increasingly "institutionalised" in the modern world.

trophe. Mechanical analogies ought, therefore, not to be abandoned hastily on the ground that economic events react upon the conditions by which they were produced; so that future events cannot happen under exactly the same conditions as they did.<sup>47</sup>

It is true that the "equilibrium" towards which "adjustment" is made is not the self-same equilibrium in both cases—that would involve contradiction. But the "force" at work is in each case the same—gravity. Similarly in an evolutionary economic process. All adjustments (or innovations), from the most seemingly "automatic" to the most constructively "creative", are alike manifestations of one universal social force—*pursuit of maximum net advantage*. And the formal theorist who himself employs this *comprehensively abstract* conception, and who refuses as "*extra-economic*" the task of investigating the concrete content of the living psychic processes that lie concealed beneath this abstraction, is by a remorseless logic debarred from drawing "hard and fast lines" of *psychological* distinction between different classes of "economic adjustments" made in the pursuit of "maximum net advantage."<sup>48</sup>

<sup>47</sup> *Memorials*, p. 317.

<sup>48</sup> "One who likes to conceive of all economic processes in terms of tendencies towards an equilibrium," said the late Professor Allyn A. Young ("Increasing Returns and Economic Progress", *Economic Journal*, December, 1928, pp. 534-5), "might even maintain that increasing returns . . . are offset and negated by their costs. . . . This would amount to saying that no real economic progress could come through the operation of forces engendered *within* the economic system—a conclusion repugnant to common sense. To deal with this point thoroughly would take us too far afield. I shall merely observe, first, that the appropriate conception is that of a *moving* equilibrium, and second, that the costs which (under increasing returns) grow less rapidly than the product are not the 'costs' which figure in an 'equilibrium of costs and advantages'".

It will be apparent to the reader that the doctrine in the text—that *all* the elements in an evolutionary economic process are "equilibrium adjustments"—is based on emphasis, and not denial, that "real economic progress" (in the sense obviously intended) can come "through the

Let us turn now to "political" and "social" conditions as possibly capable of yielding "exogenous" changes—in other words, "non-economic" events.

Here again we may at once urge that the notion that such changes can, *in strict methodology*, be regarded as "exogenous", involves a logical contradiction of the view that the subject-matter of economics cannot be segregated by means of "classificatory definition". If the subject-matter of economics is something held in common with the other human sciences, and the distinctions between these sciences are distinctions of *attitude*, then political and social changes are part of the subject-matter of "economics" in so far as they have "economic" aspects. The real reason why the logical contradiction is not immediately apparent resides, once more, in the *static atomism* of the "Austrian" attitude. I can here call attention to only three basic aspects of the problem. I think these should be sufficient for the present purpose.

operation of forces engendered *within* the economic system". But in my view, as will appear in the next section, it is the conception in the text which is in strictness that of a "*moving* equilibrium". The conception, referred to by Professor Young, of a "realising of increasing returns... spread through time in such a way as to secure an equilibrium of costs and advantages", is in my judgment capable of satisfactory treatment only in terms of the concepts appropriate to a *normative* economics. It cannot, I think, be made rationally intelligible in terms of the notion of a discrete series of (timeless) "costs" and "satisfactions" strung like beads on the thread of Time; but must be elucidated in terms of an ultimate philosophical conception of Maximum Net Social Satisfaction through Time. In the sequel to the present study, I hope to develop, by way of immanent criticism of certain basic concepts of present-day normative economics, the thesis that an intelligible *normative* "dynamics" can be constructed only through systematic subordination of the "quantitative" to the "qualitative" category. (See my "Nature and Significance of Economic Science" in *Recent Discussion*, Q. J. E., May, 1933). This, I believe, is a somewhat more rigorous way of indicating the nature of the concept Professor Young doubtless had in mind.

(a) Since the methodology of static atomism, which confines "economic science proper" by definition to the ("static") mechanics of "the" stationary state, places *all* "dynamic" change in the category of the "exogenous", it naturally places "political" and "social" changes there too. But this reason for doing so disappears with equal naturalness when we adopt the true evolutionary conception of the science.

(b) Just because static atomism is atomistic, it is compelled to refuse to undertake to handle any changes which it can *discern* to be inexplicable in terms of localised mechanical impact.<sup>49</sup>

I must not here recapitulate or even adapt the arguments

<sup>49</sup> The desperate shifts to which it is put in order to sustain its own faith in its alleged rigorous logical "precision" cannot be better illustrated than by the fact that, even when a *fear* of a "political" event (not even the actual occurrence of the event) can be shown to be the *immediate and direct result of a general* "economic" situation, *and*, in turn, the immediate *cause* of a change in that "economic" situation, the "Austrian" is driven to regard it as "exogenous". See Professor Robbins' remarkable claim (*op. cit.*, p. 117) that a "crisis" is due to "purely economic" factors if it can be shown to be "entirely due to obstacles implicit in the given conditions of world supply and demand": but that, if it can be shown to arise from "financial panic, induced by the fear of political revolt at the magnitude of the... tax burden", *then*—"the political reaction... intervenes"! Words fail me to characterise the order of aesthetic satisfaction that is derived from a distinction of this kind.

Since, as we have seen, the fact of the matter is that, for this school of thought, *anything at all* that "intervenes" is, by definition, "non-economic"; and since the foregoing remarkable feat of analytical psychic chemistry is performed in the interests of logical precision by those who hold that it lies quite outside the field of economic science to *inquire at all* into the motives of action underlying "individual preferences"; a certain inarticulateness in the critic seems excusable. Only a school of thought hypnotised by the "necessities" of elementary geometry would seek to carve up and eviscerate "a Serious Subject" in this fantastically illogical manner. Is it any wonder the Marxian jeers? One is reminded of the physicist's famous two-dimensional intellectual ants, who simply could not understand how the surface they inhabited could be "curved".

by which it has already been shown<sup>50</sup> that *all* organic change is essentially "holistic"; and that all economic change is organic change. As has just been remarked, even when the *immediate inducement* to change is capable of a very high degree of specific localisation,<sup>51</sup> the change itself is, by definition,<sup>52</sup> "holistic"; while any attempt to investigate the "content" of the inducement (or stimulus) itself, with precision and thoroughness, again leads to organic conceptions.<sup>53</sup>

(c) The main other feature of "political" change which is likely to give trouble to static atomism arises from the notion of political "interference" with the "free" functioning of the economy. This is, of course, probably the most important *practical* distinction with reference to a

<sup>50</sup> Sections vii and viii.

<sup>51</sup> P. 136, n. I am imagining an extreme case in which a specific price change in one commodity is definitely and consciously credited with producing diffused organic changes in a structure of habits and preferences. It is by no means inconceivable that a single (marked) decline in the price of a single common stock at a particular moment might have far-reaching psychological consequences!

<sup>52</sup> Section viii.

<sup>53</sup> So strong are the chains of pseudo-precision that I venture to quote again:

! "Modern physics has abandoned the doctrine of Simple Location. The physical things which we term stars, planets, lumps of matter, molecules, electrons, protons, quanta of energy, are each to be conceived as modifications of conditions within space-time, extending throughout its whole range. There is a focal region, which in common speech is where the thing is. But its influence streams away from it with finite velocity throughout the uttermost recesses of space and time. Of course, it is natural, and for certain purposes entirely proper, to speak of the focal region, thus modified, as the thing itself situated there. But difficulties arise if we press this way of thought too far." Whitehead, *op. cit.*, pp. 201-202. And so it is in economics. Unless we are prepared, in the quest for Precision, to seek the Absolute behind the whole Economic Space-Time Continuum, we must be content, with Alfred Marshall, to be "loose with system".



predominantly "individualistic" economy. Its thorough discussion belongs to political philosophy; and it must be examined with very considerable care in the study of Maximum Net Social Satisfaction through Time. Here I will merely assert that it, too, is inherently incapable of being made, with "precision", into a logical basis for segregating the "economic" from the "non-economic". For if an "economic utility" is anything that satisfies a human want, and involves sacrifice, no *separation* of the "political" from the "economic" can be made here.<sup>54</sup> As Marshall pointed out, "economic freedom" may itself tend "in the direction of cooperation and combination of all kinds good and evil".<sup>55</sup> People may give "objective" expression to their "preferences" by collective as well as individual action. They may, indeed, even develop a "preference" for collective action for its own sake. From a strictly "positivistic" standpoint, these preferences are simply "given". And the State itself is the ultimate organ of collective action. Nor is the fact that the minority may consent against their will a logical differentia of either group or even Governmental action. The same thing may be true of members of an "atomistic" laissez-faire economy. This is merely another aspect of the general truth, already noticed more than once, that successive forms in a process of organic emergence

<sup>54</sup> This is in one aspect (that of classification of "ends") frankly admitted by Robbins. See especially Chapters II and VI. "There are no economic ends. There are only economical and uneconomical ways of achieving given ends". The implied mechanical separation of means and ends; the assumption that "ends" are "given" and themselves raise no problem which penetrates in turn the problem of "means"; and the notion that "ends" can be conceived as a mechanical plurality; all these belong to our *normative* study. For a brief discussion of some of the difficulties of a Positivism which yet believes itself to possess a real problem of "economy", see my "Nature and Significance of Economic Science" in *Recent Discussion*, Q. J. E., May, 1933.

<sup>55</sup> Cf. Section II.

are one and all capable of *analytical* resolution "without residue" into the terms of the next stage below.

The main other probable stumbling-block against acceptance of a thorough-going theory of the relativity of the distinction between static adjustment and dynamic change raises the ultimate problem of Freedom and its true relationship to (methodological) "scientific determinism". The task of setting forth both briefly and clearly, and in a manner adapted to the usages of economic science *and* to the customary patterns of thought of the "static" economic "theorist", the valid essence of a highly technical metaphysical doctrine, is clearly not a simple one. I can perhaps best begin by commenting briefly on the following quotation from a recent discussion of the problem:<sup>86</sup>

We can see the relevance of these distinctions to the problem of prognosis if we consider once more the implications of the theory of money. *Given certain assumptions* with regard to the demand for money, we are justified in asserting that an increase in the volume of any currency will be followed by a fall in its external value. This is an endogenous change. It follows from the original assumptions, and, so long as they hold, it is clearly inevitable. We are not justified in asserting, however, as has been so often asserted in recent years, that if the exchanges fall, inflation *must* necessarily follow. We know that very often this happens. We know that governments are often foolish and craven and that false views of the functions of money are widely prevalent. But there is no *inevitable* connection between a fall in the exchanges and a decision to set the printing presses working. A new human volition interrupts the chain of 'causation'.

The sufficient and conclusive answer to the statement that

<sup>86</sup> Robbins, *op. cit.*, p. 116. First italics mine. The "distinctions" referred to are those between "endogenous" and "exogenous" changes; between changes which "occur within a given structure of assumptions" and changes which "come from outside".

"we are not justified in asserting . . . that if the exchanges fall, inflation *must* necessarily follow", is simply this: *given certain assumptions*, and they "necessarily" *will*—exactly as in the case of "static" conceptions of "necessity". In other words, all "scientific laws" are "hypothetical". The distinction between "endogenous" and "exogenous" changes is purely relative to the standpoint and assumptions (the "frame of reference") of the "scientific observer". The notion that the distinction is an "absolute" or "radical" one is purely a product of the intellectual limitations of the amateur geometer.<sup>57</sup> All we have to do in order to bring a previously "exogenous" change within the ambit of our "assumptions", and so convert it into an "endogenous" change is to enlarge the structure of our assumptions.<sup>58</sup>

It is most particularly to be noted that while, *in profession*, the "static" theorist takes *all* forms of psychic process "into account" in his "scales of relative valuation",<sup>59</sup> *in fact* he empties them all out. The result is that, wherever he is brought face to face with *any* concrete psychic process at all, he promptly dumps it into the hold-all of "exogenous change". He must; for he lacks the intellectual resources to do otherwise. Here again it is *static atomism* that is the root of the trouble. And the paradox of static atomism is that, while it imagines itself to be careering spectacularly round the tramlines of remorseless "necessity", it is really

<sup>57</sup> Cf. above, pp. 127-128.

<sup>58</sup> It may be remarked in passing that these considerations have the merit of enabling us to look with a kindlier eye on the aberrations of "*soi-disant* economists" who compensate for their unfortunate endowment by Providence with "sterile minds" by "continually criticising well-established economic laws". Cf. Robbins, *op. cit.*, p. 112. It is indeed perilous to believe that *anything* is "the last word that need ever be uttered" on anything. *Pride goeth before destruction, and an haughty spirit before a fall.*

<sup>59</sup> Cf. Robbins, p. 88.

the bewildered occupant of the cage of "absolute freedom." For "there is complete contemporary freedom".<sup>60</sup>

It will, I think, serve to complete this very brief discussion if I now simply recall to the reader that the analysis of earlier sections enables us at this point to go beyond the mere assertion or demonstration of paradox, and show definitely just how and where *real freedom interpenetrates* the deductive "necessities" of our demand and supply curve apparatus. It does so by virtue of the fact that *our* analytical curves have *direction in Time*. For, while a strictly "instantaneous" curve leaves us with the choice between iron necessity and sporadic and slavish "impulse", a period curve depicts a hypothetical sequence of real situations in Time, each assumed as the "consequence" of the conjoint occurrence of certain "stimuli" and "reaction" thereto in terms of certain "motives". But the direction of our curve in the time-dimension enables us to do what an instantaneous "statics" can never do—give concrete content alike to "stimulus" and to "motive". And from this there follows a very interesting corollary. For, as we have already seen, a "given" situation at a "given" point in Time does *not* yield simply *one unique* "period" curve as relevant to that

<sup>60</sup> Cf. above, p. 71, n. The fact that an abstract concept to which all the myopic resources of a neurotic logic have been painstakingly applied in the fruitless endeavour to make it "precise" and "consistent", dissolves at the last moment into its "opposite", is still nervously shied at by shoals of sentimental thinkers who, cherishing the pathetic illusion that they are hard-headed "scientists", encourage themselves by deriding it as "pseudo-Hegelian twaddle". But in fact, if the gigantic intellectual neurosis of Positivism be not speedily and pitilessly eradicated from the intellectual life of our time, modern civilisation must rot from the core outwards. Hence the enormous philosophical significance of the recent revolution in physical science. (On the bearing of this on the modern physical conception of Time, the reader should consult Whitehead, *op. cit.*, chap. xii—"Past, Present, Future"). There is profound significance in the irritation with which modern Marxism views the "Idealist Reaction" in physical science. See Strachey, *op. cit.*, p. 181.

situation. It yields *simultaneously* as many different curves as there are potentially present in the situation different possible sequences of motivation or response. If it is further recalled that this applies both to demand and to supply curves for all periods; that a "shift" in a curve for one period is an "elasticity adjustment" along a curve for another period; and that the "stimulus" to an "adjustment" on a demand (or supply) curve for any period is a shift in the appropriate supply (or demand) curve for the same period; then I think it will be perceived that (to use a paradoxical phrase) the *abstract* possibilities of real freedom in Time are infinite!

I purposely put the matter in this way because it places in the mouth of the static atomist an "objection" which might not occur to him unaided: that, after ridiculing the "tyranny" of the "absolute freedom" of his cast-iron "instantaneous statics", I have after all only created for myself a vaster and more terrifying intellectual cage with an extra dimension to it. But this is not so. It is true that the *abstract* conception of *pursuit of maximum net advantage* still *abstractly* leaves us with an infinity of possible choice in Time. But the introduction of Time now enables us, in strict conformity with the logical character of our abstract graphical apparatus, to introduce the *real range of concrete choices* that are *relevant* to the given "moment" in the evolutionary process, and that are consciously present to the human personalities concerned.<sup>41</sup>

<sup>41</sup> It will be evident to the reader that for this (as I have the temerity to think) rather "neat" way of putting the matter, I am jointly indebted to Professor Whitehead's latest formulation of his philosophical position in a manner accessible to the general student, and to Mr. G. F. Shove's most timely publication of his all too brief remarks on the supply curve. Without the aid of the latter, I should have been condemned to a cumbersome, lengthy and, I fear, unsatisfactory method of expressing what I have here in mind. Cf. *supra*, pp. 85-86, note.

To pursue this matter still further in its most general methodological aspects leads directly into the problem of the *structure* of the relationship of the individual personality to Society—a topic which belongs to our *normative* sequel to this Essay.<sup>62</sup> For this we are not here fully prepared. But in view of what I conceive to be their general interest, I conclude this section with some brief observations on the *practical* significance of the concept of Freedom. And for this purpose, I cannot do better than revert for a moment to Merz:<sup>63</sup>

The reason why the atomising process is inadequate seems to be twofold. First, the actual arrangement of separate things, *be they physical particles or mental ideas*, if once broken up cannot be again restored as it was found and seen in its actual existence; something is lost which cannot be regained. And further, the process of analysis, of finding the ultimate constituent elements, is endless: as space is infinitely divisible, so also the elements out of which things natural are compounded seem to be out of reach. The lane through which we walk in the attempt to reach the last constituent elements of things natural has—contrary to a popular saying—no end, it never turns, and the point which we choose for retracing our steps is purely arbitrary, fixed by the knowledge of the moment. The analytic process is irreversible. The point at which we start to synthesise or put together again is purely arbitrary, fixed by our knowledge or rather our ignorance, and the product of such synthesis is accordingly artificial, not natural: the world of things, images of thought or practical constructions, is accordingly artificial.

This is the ever-receding Waterloo of methodological scientific determinism. The irony of the situation resides in the fact that it can never, in the nature of things, satisfy

<sup>62</sup> And, if I rightly understand Professor Whitehead, to problems whose general *form* is capable of mathematical depiction by the more esoteric developments of projective geometry.

<sup>63</sup> Vol. IV, p. 776. Italics mine.

itself empirically that the "ultimate" has been reached: there is always the bare possibility of new knowledge. Hence, *whenever* the limits of practical resources for analysis have for the time being apparently been finally reached, there emerges a Doctrine of Indeterminacy.<sup>64</sup> It may thus be said that, while Freedom always has the last word, it is always the duty of "science" to dispute it. For our present purpose, there are two points to which I would direct the attention of the reader:

(a) To assuage the gnawings of the scientific conscience, which *must* always press on to ever more minute analysis, it would be a matter of great interest to economic science—in view of its surprisingly intimate affiliations with psychology—to discover whether there is today any prospect of happy release for the time being in the discovery of discontinuous psychological quanta; with which we could presumably then make shift with quite sufficient accuracy for broad social purposes.

(b) Without pressing any further in the present connexion the dangers inherent in reification of ultimate psychological quanta, with consequent visualisation of the living social process as resulting from mechanical (or even "statistical") interaction of these midget entities; and without urging further, either, the methodological superiority of the holistic concept of self-determination as equivalent to "real

<sup>64</sup> Thus Einstein and Planck—who continue resolutely to affirm the universality of the principle of causation—and the Schroedinger-Heisenberg School are perhaps *both* right in the abstract. The scientific difference between them may be merely one of optimism and pessimism. But if, as the thoroughgoing use of the "circular" concept of "infinity" may suggest to the impressionable, it were to prove that a very fundamental human limit to infinitesimal analysis had been practically reached today, then the event would be presumably epochal in the history of science: and we should have to return to the ancient world for an adequate analogue. It is tempting to wonder whether *physical* "holism" or "gestalt theory" could conceivably provide a way out!

freedom"; it may at least be pointed out as a practical matter that, even if all the speculations of all the economists had always been and still were the inevitable result of a strictly "causal" process—the diffusion throughout the community of increasingly accurate and adequate understanding of the nature and significance of the functioning of the economic system would become in turn a strictly "casual" factor making for "inevitable" and "determinate" modification of the economic organism in accordance with the character of that knowledge and the characters of those who received it.<sup>65</sup>

Economic Science is today, therefore, confronted with a great opportunity. Equipped as it now is to put the Marxian Devil to rout by rigorous and remorseless scientific demonstration of the old naive intuition that Man does not live by Bread alone—but by the Pursuit of Maximum Net Advantage: it need not rest tamely on its laurels; but can now adventurously turn its attention to the really practical task of ascertaining for the benefit of the bewildered human race in just what direction Maximum Net Advantage is to be found—and what it looks like when we find it. Discussion of the Pure Methodology with which it is imperative that the competent professional economist should equip himself before embarking on this enterprise belongs to the study of Maximum Net Social Satisfaction through Time.<sup>66</sup>

<sup>65</sup> As far as short-run consequences are concerned, the unfortunate statesmen of the world are today continually having their embarrassments increased by the surprisingly unexpected (even if "strictly determined") responses of mankind—to various "monetary" situations—occasioned by the beneficent diffusion of the fundamental laws of "monetary science".

<sup>66</sup> Having demonstrated, I think, in the foregoing pages, how a formalistic Austrian "statics" can be equipped with a time dimension and an immanent abstract developmental principle without loss of continuity or formality, I am tempted to lay claim to the distinction of discoverer of The Objective Preference Interpretation of History: but I suspect that at least the germs of this great truth are discernible in the work of previous scholars.



## XII

"We have to attribute as much character to the interspace as to the particles."

*A. S. Eddington.*

We are now in a position to attempt some sort of answer to the question: what, precisely, is a "moving general economic equilibrium"? We have already noticed<sup>66</sup> that the term "moving" is here not strictly and unambiguously appropriate; since "motion" is a relative concept, and we are considering the economy as a comprehensive "closed" system.<sup>67</sup> But the term has gained some foothold in economic science in the present not very definite signification;<sup>68</sup> and I retain it here in preference to the more flamboyant expression to which I have descended on the title page. Moreover, as we have also seen,<sup>69</sup> the notion of "absolute expansion" (or contraction) has difficulties of its own. For, if

<sup>66</sup> *Supra*, section iv.

<sup>67</sup> It is true that, if we think of the economy, in relation to its physical environment, simply in terms of number of the population in relation to quantity of means of subsistence available, we may be able to give the term "moving", used in this connexion, precise signification. The same would be true of volume of physical production; though of course the term would still in both cases be metaphorical—if "movement" be taken to apply strictly only to relative change in *space* relationships, and not to relative change in number or quantity. But if we conceive of the *essence* of "economy" as a continual "equilibrating" process having reference ultimately to "costs" and "advantages", the matter is not so simple; and ultimately involves, in fact, the philosophical problem of the *mode* of "relationship of Man to Nature". This raises issues examination of which is postponed to the study of Maximum Net Social Satisfaction through Time. The present discussion is therefore necessarily incomplete. It must accordingly be judged relatively to its restricted purpose, which will presently appear.

<sup>68</sup> *Cf. supra*, p. 137, n.

<sup>69</sup> *Supra*, *loc. cit.*

there are profound objections to considering a "closed" economy, *significantly* defined, as "moving" relatively to its *external* environment; there are also difficulties in the way of conceiving it (*significantly* defined) as *absolutely* "constant" or "expanding" in "magnitude" from some unique and absolute standpoint *internal* to the economy. I do not refer merely to the more superficial difficulties associated, for example, with the fact that "the general price level" may be rising for one member of the economy at the very time that it is falling for another member; or with the notion of the volume of the national dividend and its changes through Time. These are in my view but manifestations of deeper problems connected with our traditional mode of conceiving "real costs" and "satisfactions" in their relation to Time.<sup>70</sup>

Since the foregoing difficulties necessarily attach as much to the attempt to conceive a system which is, as a whole, absolutely "constant" in "magnitude" throughout a process of internal change, as they do to the attempt to conceive a system which, as a whole, is absolutely "expanding" in "magnitude" through Time; it must be frankly admitted at the outset that we do not have a very secure foundation upon which to build our discussion of "moving general economic equilibrium"—or of "an expanding economic universe". But, if the reader is prepared to be tolerant of this lack of precision; and, even more, if he feels that, on customary levels of discourse, we possess a pretty trustworthy general notion of what we mean by a "dynamic" economy in Time; then we may proceed without more ado to wind up our argument in

<sup>70</sup> So that the reader may know where he stands, I again state my conviction—to be explained and defended elsewhere—that the normative problems involved are intrinsically insoluble in terms of a quantitative calculus of atomistic "costs" and "satisfactions". The reader may profitably contemplate Sidgwick's sturdy, good-humoured and highly intelligent struggles in this quagmire. See his *Methods of Ethics*, especially Books II and IV.

this Essay by asking how, if at all, we are to distinguish "equilibrium" from "disequilibrium" with respect to a "closed" economy evolving in Time.<sup>71</sup>

We shall, however, proceed with a better conscience if we notice at the outset that—perhaps just because of the difficulty of conceiving appropriately of an abstract "magnitude" of the Whole, which magnitude is either absolutely "constant" or absolutely "expanding or "contracting" throughout the process of *internal relative* change—it is not the constancy or change of this "absolute" which is important for our present purpose. For, even if we were to assume that this problem could be, and had been, satisfactorily solved, and that we had in consequence a clear conception of a system which, as a whole, somehow continued "constant" throughout a process of internal relative change; we should *still* be faced with the problem of "moving equilibrium" *within* the abstract "constancy" of this Whole. That this is so becomes apparent, I think, if we reflect on the obverse consideration: the problem involved is so intimately bound up with the fact of relative internal change that, if we assume that no such relative internal changes occur, there seems, at first sight at least, to be no "absolute" problem left; and we are strongly tempted to regard an economic universe which,

<sup>71</sup>I again offer for the reader's consideration a comparison with physical science. In *The Expanding Universe*, Eddington, having intimidated us with a disturbing picture of a bubble universe steadily increasing in volume towards an ultimate and inevitable bursting-point; then assures us that, since "expansion is a relative term", this view of the matter is "true" only from our point of view, and not from that of the Whole. From the latter standpoint, the much more humiliating truth is that it is "really" we who are steadily shrinking. However, the end-result is apparently the same—Nothingness. This means that, despite the "relativity" of change, there is yet real development in the Universe. Nevertheless, behind this process of "becoming" (Nothing) stands the Cosmical Constant.

I do not think the problems of the Economic Universe can really be much more difficult.

in "expanding absolutely", undergoes *uniform relative expansion* in all its parts, as *indistinguishable* in any significant manner from one which remains "absolutely constant" during a similar absence of relative internal change.<sup>72</sup> This idea has been suggested by Marshall, with characteristic and commendable caution of phraseology:<sup>73</sup>

The Stationary state has just been taken to be one in which population is stationary. But nearly all its distinctive features may be exhibited in a place where population and wealth are both growing, provided they are growing at about the same rate, and there is no scarcity of land: and provided also the methods of production and the conditions of trade change but little; and above all, where the character of man himself is a constant quantity. For in such a state by far the most important conditions of production and consumption, of exchange and distribution will remain of the same quality, and in the same general relations to one another, though they are all increasing in volume.

Yet it would, I think, be hasty to conclude definitely that, if such an economy could be realised, it would exhibit *no* significant difference from "the" stationary state. For it would, in fact, contain *one* more or less significant item which would not share in the "general expansion": I refer to the individual human being. From *his* standpoint, the economic universe would be undergoing absolute expansion, even though its internal relative relationships remained the same. The issues thus raised transcend the present discussion;<sup>74</sup>

<sup>72</sup> Cf. Eddington, *op. cit.*, p. 126: ". . . an expansion shared by everything alike would be undetectable, and would in fact have no definable meaning."

<sup>73</sup> *Principles*, p. 368.

<sup>74</sup> Assuming that, in such an economic universe, every individual member experienced, on the whole, a net surplus of satisfaction over its opposite, there would undoubtedly be, as Sidgwick has remarked (*op. cit.*, pp. 414-7), an absolute expansion in the "aggregate" of net satisfaction. Since in the real world, the growth of population is not in fact unaccomp-

but mention of the matter serves to show why we must beware, in this partial discussion, of ruling out without further reflection the "absolute" problem. It also indicates why it is that we can proceed with consideration of the questions that do concern us here without now solving that problem. For, while we have failed to equip ourselves with a satisfactory definition of "absolute constancy", it will be generally agreed, I think, that we are not thereby debarred from discussing, on a more mundane level, the problem of "moving equilibrium" within a Whole as to which we are undecided whether it is or is not in *some* sense an absolute constant or a variable throughout the process of internal relative change and adjustment.

How, then, are we to distinguish between an evolutionary economic process which maintains itself in a continuous condition of "moving general equilibrium", and one which does not? That such a distinction is quite commonly and currently regarded as both theoretically and practically important (if not sometimes indeed, as *the* central problem of

anied by change in relative conditions, or (presumably) in the amount of the average individual net surplus of satisfaction (or dissatisfaction), further problems are raised. Sidgwick held that their solution must be sought on the basis of the view that, "strictly conceived, the point up to which, on Utilitarian principles, population ought to be encouraged to increase, is not that at which average happiness is the greatest possible,—as appears to be often assumed by political economists of the school of Malthus—but that at which the product formed by multiplying the number of persons living into the amount of average happiness reached its maximum" (pp. 415-6). This doctrine has apparently declined somewhat in popularity since Sidgwick wrote. Whether closer investigation would reveal that this is to be accounted for simply in terms of a change in the mathematical signs employed (or a shift to the view that the average individual "surplus" is one of "dissatisfaction"), I do not know. These questions do not concern us here. I merely mention them in order to show that we are not justified now in taking it for granted that the concept of "absolute expansion" can be excluded from all further consideration.

a comprehensive *normative* economic "dynamics"), there is, I think, no doubt.

Now clearly, logical consistency in the use of terms would seem unquestionably to demand that, if the term "equilibrium" is to be comprehensively employed to characterise both a "stationary" and a "moving" system, then the system which is "moving" must, to qualify for such characterisation, exhibit, *mutatis mutandis*, the same essential characteristics which have led to the application of the term "equilibrium" to the "stationary" system.

I do not think much help is "to be got" here from "dynamical analogies". In trying to fix our ideas, we may, if we like, conceive, for example, of two pendula, the one suspended from the end of the other, and so adjusted as regards their relative weights and lengths that, at each successive instant in the progress of the one, the other (dependent) pendulum remains "in equilibrium" relatively to it. Much more complex physical analogies are doubtless available. But, as we have noted, the concepts of "motion" and "rest" are, in modern physics, purely relative terms. Hence this analogy lands us in difficulty when we come to consider a "closed" system; for it enables us to distinguish a "moving" from a "stationary" equilibrium only in terms of a partial system which, while continuing in a condition of *internal* "stationary" equilibrium, is (as a system) respectively "in motion" or "at rest" *relatively* to some *external* point or frame of reference. In the case of a closed system or general equilibrium, this conception is, by definition, not available. Moreover, such an analogy provides us with a stationary equilibrium which is identical from instant to instant. It seems better to consider the economic system directly, and to ask what characteristics of a "stationary" economy must be continuously preserved in a "moving" one in order that the latter may appropriately be described as a "moving general economic equilibrium".

It has been noted that *one* of the characteristics of a "stationary state" would be, as Marshall pointed out, that in it normal price and average price would be convertible terms. If it were held that it is this characteristic which gives *significance* to the concept of a stationary state, or of stationary general equilibrium, then it would apparently follow that a "moving" general equilibrium, also significantly defined, would be an evolving economy whose dynamic processes were so mutually regulated that the changing long-period normal prices of all the constituent commodities and services in the economy were the moving averages of the actual changing market prices of these commodities and services. But, however this may be,<sup>75</sup> I do not think that the significance which

<sup>75</sup> I am not competent to discuss the mathematical problems involved. It is worth noting that Marshall apparently held that, under actual conditions, no such definitive relationship exists between changing normal and changing market price. "An average may be taken of the prices of any set of sales extending over a day or a week or a year or any other time: or it may be the average of sales at any time in many markets; or it may be the average of many such averages. But the conditions which are normal to any one set of sales are not likely to be exactly those which are normal to the others: and therefore it is only by accident that the average price will be a normal price; that is, the price which any one set of conditions tends to produce." *Principles*, p. 372. It may be tempting to conclude that, under conditions of cyclical fluctuation in the general level of money prices, the changing normal price under actual conditions will correspond roughly to the trend-line of actual prices. But this would, I think, be a mistake. For example, if a given commodity be subject at a given time to long run conditions of decreasing cost, which are gradually operating in response to a strong demand, its market price will be considerably above the ultimate normal price that is *relevant* to that "point" in Time (*cf. supra*, p. 77, n.). Now if the development of the economy as a whole through Time should steadily lower the whole normal supply curve for this commodity, while demand continues slowly but steadily to increase; then it would seem that the changing normal equilibrium price may continue for a long period of time to lie much below changing market price—and might conceivably even lie below the low points reached in successive price cycles. On the other hand, unless it be possible to define more rigorously than has yet been done those elements in the total economy that are "impounded in *caeteris paribus*"

economists have tended to attach to the concept of general

when a *given* long period supply curve is drawn, it may seem plausible to argue that long period normal price itself fluctuates with general fluctuations in money prices. But this is very obscure. The difficulty is only in part that of the causal relation between changes in the total supply of money and changes in the "real" elements in the economy. It is also connected with the fact, already discussed, that a "theoretically perfect" long period is, in a dynamic economy, a contradiction in terms. Unless, therefore, mathematical economists can assist in some manner I have failed to discern, it would seem that "precise" calculation of the normal price that is relevant to a given "moment" in actual Time is inherently impossible. In short, "such notions must be taken broadly. The attempt to make them precise over-reaches our strength." Marshall, *Principles*, p. 460.

Again, if the concept of normal price be regarded, either explicitly or vaguely and implicitly, as in any sense a *normative* concept in the sense of that ambiguous term which means "socially desirable", a further and distinct confusion is introduced. For, apart from the problem of "precision", there "underlies" any dynamic economy a system of "normal prices" changing through Time. It follows that there must be a deep-seated methodological confusion at the root of any proposal of "normative economics" which is based upon (or implies) some notion of modifying the functioning of the economy so as to make the behaviour of actual prices "conform" to changing "normal" prices. In a stationary state, "normal price" is simply the average level of actual prices; and the "deviations" of the latter from "normal" are not as such "abnormalities" in any *welfare* sense of the term, but functionally necessary elements in the economy as it is constituted. Similarly in an evolving or "dynamic" economy: and any modification of its functioning so as to bring changing market price into some other than its actual relation to changing "normal" price would alter the behaviour in Time, not only of market prices, *but also of normal prices themselves*. The *normative* ("welfare") problem is therefore one of selecting the "right" kind of system of both market *and* normal prices. On the dangers and confusions involved in identifying "normal" and "normative", see Marshall, *Principles*, p. 35.

The foregoing remarks seem to shed some light on what Mr. Redvers Opie (*Economic Journal*, June 1931, *loc. cit.*, p. 215, n.) calls "Marshall's almost morbid fear" of averages; and to cast doubt on his assertion that "Marshall never faced squarely outside the stationary state" the "relation between 'average' and 'normal'". There are, I suggest, as many forms of such relationship as there are various abstractly possible "dynamic" economies.



equilibrium, whether stationary or "moving", has lain in this or any other purely mathematical characteristic. It has lain, I believe, in a widespread tendency to identify general equilibrium, at first with "maximum net social satisfaction", more recently with a condition of *continuous employment of total resources*.<sup>78</sup>

As a result of the work of Marshall and Pigou, it is no longer possible today to identify "equilibrium"—conceived

Somewhat similar considerations, *mutatis mutandis*, apply, I think, to the strong temptation to assign normative significance to the trend lines of actual series in modern time-series analysis. The reader may consult my "'Equilibrium Economics and Business Cycle Theory': A Commentary", in *The Quarterly Journal of Economics*, November 1930, not only for a criticism of the impropriety of such a procedure; but also for the unhappy illustration which (despite careful qualification) I have myself there provided of the wrongful attribution of quasi-normative characteristics to the concept of "normal". My error is the direct result of taking stationary general equilibrium to be an "instantaneous" condition in which "total resources" are "fully" employed. On the significance of the fact that cycles and trends (like "market price" and "normal price") are *mutually* related, see Mitchell, *Business Cycles: The Problem and its Setting*, pp. 249-259.

<sup>78</sup> Cf., e.g., Hayek, *Prices and Production*, pp. 31-32: "If we want to explain fluctuations of production . . . we have to start where general economic theory stops; that is to say at a condition of equilibrium when no unused resources exist. If we are to proceed systematically, therefore, we must start with a situation which is already sufficiently explained by the general body of economic theory. And the only situation which satisfies this criterion is the situation in which all available resources are employed."

Consider: "The pure theory of equilibrium enables us to understand how, given the valuations of the various economic subjects and the facts of the legal and technical environment, a system of relationships can be conceived towards which existing relationships may be regarded as tending. It enables us to describe that distribution of resources which, given the valuations of the individuals concerned, satisfies demand most fully. But it does not by itself provide any ethical sanctions. To show that, under certain conditions, demand is satisfied more adequately than under any alternative set of conditions, does not prove that that set of conditions is desirable. There is no penumbra of approbation round the Theory of Equilibrium. Equilibrium is just equilibrium." Robbins, *op. cit.*, p. 127.

as the *kind* of "equilibrium" which results, or allegedly results, from laissez-faire individualism—with "maximum net social satisfaction". This tenet of a once robust "Liberalism" in its more extreme forms has been destroyed forever. But the notion of (stationary) "equilibrium" as continuous employment of total resources is today a much more insidious enemy of straight economic thinking. For atomistic Liberalism in economics has today gone into neurotic retreat. By means of a false pre-Marshallian segregation of "statics" from "dynamics", it has sought to confine "economic science proper" to the former alone. By means of purely destructive criticism of modern normative economics, it has sought to cast discredit on the laborious and painstaking attempt to build up valid criteria for social "interference" with "laissez-faire" conditions.<sup>77</sup> By a process of self-emasculation, it has sought to evade uncomfortable charges of "hedonism", and yet to retain the right to proclaim that a laissez-faire condition is one which yields "maximum satisfaction of demand". Whatever hypnotic influence it still possesses is the combined result of its refusal to face the Time problem; its repudiation of the fact that "exogenous" changes are themselves an integral element in the totality of "static adjustments"; its evasion of the truth that "maximum satisfaction of demand" is a tautological concept equally applicable (as is "equilibrium") to a "static" situation in which "interference" is present; and its assumption that laissez-faire "equilibrium" involves total employment of resources.

But if it be true, as I have argued in an earlier section, that,

<sup>77</sup>See Robbins, *op. cit.*, Chapter VI. It is imperative to distinguish sharply between purely destructive criticism designed to *eliminate* such studies from the "science"; and immanent criticism designed to increase the clarity and power of normative economics itself. See my "'Nature and Significance of Economic Science' in Recent Discussion," Q. J. E., *loc. cit.*

even if we adopt the *detailed* (un-Marshallian) conception of stationary equilibrium as a condition in which each individual member of the economy has found and permanently occupied the "best" position open to him under conditions imposed by the attainment of a corresponding position by every other member of the system, still, this does not imply either absence of "error", or perfect objective knowledge, or "perfect" timeless fluidity: then, by insisting on strict consistency, we can see how the term "moving general equilibrium" ought *properly* to be interpreted in a "strictly positive" sense. For it should be obvious that, in a system the "underlying conditions" of which are undergoing continuous change, the "best" positions open to its members will be subject to like change. It is therefore impossible to speak, in connexion with a "moving equilibrium", of each individual as *finally* occupying a *given* "best" position in the system. The corresponding "dynamic" conception must be that of the various members of the system successively occupying, through Time, the successive positions which are, successively, the "best" open to them at those successive moments. And since, in a stationary equilibrium, the term "best" is relative to the actual state of the capacity, knowledge, foresight, etc., of the individual (so that "error" may be present if only it be constant); it follows that the term "best" must be similarly relative in the case of a "moving equilibrium".

Now if, purely for purposes of argument, we make the elusive assumption that the "underlying conditions" of general stationary equilibrium are permanently given, but that the system is still in disequilibrium; we must recognise that the various adjustments towards the final stationary equilibrium require Time in order to work themselves out; and that, since both the qualitative nature and the temporal rates and durations of these adjustments are, in precisely the same

sense as is the consummated equilibrium, determined through every successive point in Time by the actual state of the capacity, knowledge, foresight, etc. of each and every member of the system; it follows that not only the consummated equilibrium, but also every successive step in the progressive approach thereto, is the "best" possible *at the time*. The same reasoning holds both for the naive notion of a discontinuous series of stationary general equilibria replacing one another in Time—as the successive "goals" of the process—under the discrete influence of an independent set of "dynamic forces"; and also for the more adequate Marshallian conception of a dynamic or organic continuum. In short, from the standpoint of scientific determinism, "this is the best of all possible worlds—and *everything* in it is a necessary evil".

There is thus no escape, I think, from the conclusion that, if the essence of "equilibrium" be sought in the terms just discussed, then we have no means, when speaking of an evolving economy, of distinguishing between "moving general equilibrium" and the actual historical process. The actual historical continuum is a "moving general economic equilibrium".

Nor do I see any good reason why this conclusion should be felt to be in any way outrageous—or, indeed, anything but perfectly natural. Those who find themselves experiencing resentment in the face of it should, I think, regard this resentment as significant evidence that their minds are not free of a complaint similar to that which Marshall detected in Adam Smith:<sup>78</sup>

. . . he had not quite got rid of the confusion prevalent in his time between the laws of economic science and the ethical precept of conformity to nature. "Natural" with him some-

<sup>78</sup> *Principles*, p. 758, n.

times means that which the existing forces actually produce or tend to produce, sometimes that which his own human nature makes him wish that they should produce. In the same way, he sometimes regards it as the province of the economist to expound a science, and at others to set forth a part of the art of government.<sup>79</sup>

The manner in which this confusion of "natural law" with something "beneficent" or "harmonious" continues to infect modern economic thinking is so subtle that it seems desirable, in order to expose it, briefly to direct attention to a distinction which is, after all, very elementary.

First, economics, regarded as a "purely positive" science—and we are continually being assured that it is, and should be, nothing else—is a scientific analysis of the economic system in terms of "what is and tends to be"; with a view to formulating the laws of its processes. These laws, regarded *individually*, are *hypothetical* propositions, of the form "If A, then B". But the hypothetical character of these laws is the direct consequence of their individual abstractness. If we succeed (more or less) in formulating a comprehensive system of such hypothetical laws, which, taken in its complex entirety, exhausts the content of our

<sup>79</sup> Marshall adds: "But loose as his language often is, we find on closer study that he himself knows pretty well what he is about. When he is seeking for causal laws, that is, for laws of nature in the modern use of the term, he uses scientific methods; and when he utters practical precepts he generally knows that he is only expressing his own views of what ought to be, even when he seems to claim the authority of nature for them." I do not think that this could justly be added with respect to certain forms of modern methodology. See the remarkable tautology: "Now, of course, given the desirability of individual liberty, absence of regimentation, power of continuous initiative, there is strong reason for supposing that conformity to the criteria of free economic equilibrium constitutes a fulfilment of these norms." Robbins, *op. cit.*, p. 127. As soon as we try to put concrete content into these empty abstractions, we are at once faced with all the baffling real problems of modern society.

"universe", then this *system* of (hypothetical) laws is explicative of the functioning of the economic system as it actually *is*—not as it (hypothetically) might be under some other and non-existent set of circumstances. The Marshallian analysis, in terms of "normal" tendencies and of "equilibrating" forces, is a "positive" analysis of the system in this sense of the term. The concepts of "normal" and of "equilibrium" which it employs are, therefore, hypothetical so far as the formulation of single laws are concerned—because every scientific law, in economics as in every other science, is necessarily hypothetical. This is the whole meaning of Marshall's insistence on the "relativity" of the term "normal"—an insistence which has been so frequently and so unintelligently misunderstood.<sup>80</sup> But, when we view this body of doctrine as a comprehensive whole, then, just because it is a whole, it ceases to be hypothetical and becomes actual. To assume that an entire body of doctrine relates to a hypothetical, and not to an actual, "universe" simply because each and every individual law of which that body of doctrine is composed is a hypothetical law (that is, simply a *law*), is to commit on a very fundamental plane the precise fallacy from which economists are fond of proclaiming it to be their function to deliver "the man in the street". It is to commit the fallacy of composition.

The reader will now perceive how thoroughly pernicious in its ambiguity is the assertion that "equilibrium theory" explains "equilibrium", but not "disequilibrium"; and how, too, this statement becomes worse than ambiguous, and definitely false, when it is coupled with the explicit assertion

<sup>80</sup> And which—since, as Professor Robbins has truly remarked, "bonest misconception is an excellent spur to effective rhetoric"—has produced so many charges of "evasiveness". The only evasiveness involved is in the manner in which the truth persistently evades the critic.

that a condition of (general) "equilibrium" is one involving continuous employment of total resources.<sup>21</sup> "Equilibrium theory" does *not*, strictly speaking, "explain equilibrium": it explains the actual functioning of the actual economy as a resultant of a complex, ever-changing network of "equilibrating" *tendencies*. And in such an economy "disequilibrium" is, as it were, part and parcel of "equilibrium". The phenomena of "industrial fluctuations", of "business cycles", are an integral element in the process analysed and explained by means of the Marshallian analysis. This does not at all mean that the Marshallian analysis itself provides a direct study of "business cycles": of course it does not. But it does mean that "business cycles" are a direct manifestation of the identical processes with which the Marshallian analysis itself deals. It means that the development of economic science beyond the Marshallian analysis in the study of business cycles does not consist in the transference of scientific attention from one set of concrete phenomena to another set of concrete phenomena; but rather in the adoption of a different *attitude* towards, the asking of a different set of questions about, the same phenomena.

This is not a superfine methodological distinction. Radical misconception of the true relationship of "equilibrium economics" to "business cycle theory"—sponsored by *soi-disant* "theorists"—gravely menaces the continuity of the science itself, and today threatens two quite deplorable consequences. In the first place, by encouraging the view that the former is a mere "instantaneous statics", it is a direct encouragement to abandon, in "dynamic studies", the very concept of "economy" (which involves *counting the*

<sup>21</sup> Even if it were true (and I have argued that it is not true) that there would be no idle resources in a *stationary "equilibrium"*, it does not follow that there would (or could) be no idle resources in an evolving economy of the same general institutional character.

*cost*), and to pursue certain *ad hoc* criteria of "stability" without discrimination or understanding of what their actualisation might involve. In the second place—and this is but the other aspect of the same thing—it encourages gross underestimation of the range and seriousness of the "control" measures that would probably be necessary in order to secure the desired "end". I do not think it is too much to say that, at a time when the moral pressure upon economists to provide real and immediately practical proposals for dealing with depression is literally (and quite naturally) tremendous, refusal to face the full implications of these remarks threatens the science with permeation by quackery. A false conception of "economic equilibrium" encourages the belief that the economist is dealing with a "machine" which has somehow or other "gone wrong", or contains some specific and localisable "flaw"; and that, if only he can discover and rectify this "flaw" in the "machinery", he will cover himself with imperishable glory. In this way it is forgotten that "the Mecca of the economist lies in economic biology rather than in economic dynamics", and the really significant and fundamental truth of the situation is obscured: namely, that business cycles are, and always have been, a "normal" constituent of an "individualistic" system; that the tremendous concentration of public and professional interest upon them in recent years is a manifestation of a deepseated change in social attitude towards the phenomena of instability and insecurity; and that, if the desire for stability and security is to be even partially satisfied, the necessary and inevitable price must be paid in a progressive modification of our fundamental institutions.<sup>83</sup>

<sup>83</sup> But while it seems to me of great importance that these things should be emphasised, I think it should also be realised that the problem of (social) costs and advantages which is an inherent and inevitable element in the study of industrial fluctuations should not be *statically* conceived.



It seems to me fair to say that these considerations are strictly relevant, though of course with varying degrees of force, not only to the various "remedies" offered from time to time by the camp followers of economic science; but also to certain of the underlying implications of some of the most serious and constructive scientific thinking that is being done today within the science. If the significance of the foregoing elementary remarks is squarely faced, it will surely be admitted that the popular current notion, for example, of maintaining the actual rate of interest in "conformity" with the "natural", or the "equilibrium", rate is permeated with false assumptions concerning the real nature of "equilibrium theory". In an evolving economy, the "normal" rate of interest is a changing general level of interest rates corresponding essentially to the changing "normal" price of any other "commodity". And this changing "normal" rate of interest is continuously "realised" *whatever* happens to the economy. At the very most, therefore, the *normative* problem here involved must be the intelligent selection and enforcement (if possible) of *one particular kind* of changing "equilibrium rate" of interest.<sup>22</sup>

It is not merely that we sacrifice so much "freedom" for so much "security", and that "you cannot get more than a pint out of a pint-pot". If we are to avoid the false opposites of "individualism" and "communism", the economist must be a political philosopher and a political scientist as well as an economist: he must display constructive imagination and capacity for constructive "political invention" in the development of improved institutional forms.

<sup>22</sup> Mr. J. M. Keynes is, of course, obviously aware of this; for the equilibrium rate of interest which he regards as important is one which he believes would serve to maintain equality between "saving" and "investment". But I venture to think that, though the specific misconception I have spoken of is thus absent from his work, the danger of underestimation of the full requisites of "stability" obtrudes again at only one remove. For it seems to me that, in insisting (*Treatise on Money*, vol. i, pp. 178-9) that his theory is concerned with continuous *equalisation* of "saving" and "investment", and not—as is Mr. J. A.

The second aspect of the elementary distinction referred

Hobson's, for example—with the absolute amount (under specific but complex conditions) of both saving and investment, Mr. Keynes is minimising the importance of the very factor whose "right" behaviour is the *sine qua non* of any continuous equalisation of the two. If the "stable" behaviour of the "general price level" is a mere resultant of the proper articulation of "saving" and "investment", the latter in its turn is a mere resultant of the proper articulation of more complex and deep-seated factors. If it were urged that the more "skillful" use of existing "mechanical" control devices to effect this equalisation would, in so doing, ensure the "right" behaviour "in the short run" of the absolute quantities also, I should reply that this is to attribute to these devices a power of "short period" regulation, of strong underlying economic forces, which they are in fact quite incapable of displaying in practice. Cf. D. H. Robertson, "Mr. Keynes' Theory of Money", *Economic Journal*, September 1931, pp. 410-411. There is of course room for argument on this matter; and it is clearly important that differences of degree should not be treated as radical differences in kind. But the discussion in this Essay has served to reveal some of the complexities and obscurities in the interrelationships of the "long" and the "short" period, regarded from a "purely positive" standpoint, I believe systematic investigation of the interrelationships of these concepts from the *normative* standpoint will reveal even more complex and baffling problems.

It is significant in this connexion that Mr. Robertson should have called attention (*loc. cit.*, p. 399 and p. 410) to the ambiguities lurking in the term "equilibrium"; that he should have doubted whether Mr. Keynes "fully takes account of the features which sharply differentiate an equilibrium so (dynamically) conceived from the so-called 'stationary state'"; and that he should challenge the notion that the "dynamic" conception of "equilibrium" is synonymous with "stability" (p. 410). I do not think these ambiguities can be thoroughly removed unless we frankly adopt the position that *any* evolving economy *is*, in the sense above argued, a "moving general equilibrium", and that, in attacking the problem of "stability", we are definitely departing from this "positivistic" use of the term and are engaged in exploring, from a social and not an "individualistic" standpoint, the prerequisites and costs of various hypothetical types of moving equilibria which, either definitely or tentatively, we regard as *normatively* "desirable". Only in this way can we avoid shirking recognition of the progressive "control" requirements that may be inherent in our vague and ill-defined conceptions of what we socially "want". Only in this way can we attain to full intellectual freedom to explore the rich possibilities of the notion that "norms" of "control", no less than actual "positive" dynamic processes, may have

to above is as follows. The economist need not of course, confine himself to formulating hypothetical laws which, when integrated into a coherent system, simply explain the economy as it actually does work or has worked in the past. He may also of course formulate a whole system of hypothetical laws conjointly designed to show how the economy *would* work if it could be made to function in accordance with this hypothetical *system* of laws. Since (apart from its pure scientific function of aiding explorative thought) the value of such a hypothetical system must be dependent upon the real possibilities of its practical actualisation, it is obvious that no sharp line can be drawn between these two uses to which a system of hypothetical laws can be put. But the distinction is nevertheless important. Thus it is apparently assumed by those who regard "equilibrium" as implying continuous maximum employment of total resources (and continuous "maximum satisfaction of redemand"), that this *would* come about under conditions of "free competition".<sup>84</sup> The ambiguities of the conception of "free competition" have been so repeatedly pointed out, and so extensively explored,<sup>85</sup> that it is difficult to understand how any

to be conceived more and more in "organic" and "holistic", rather than in "mechanical" and manipulative, terms; and that "social control" is perhaps most adequately and fruitfully conceived as "self-control". Only in this way can we finally eradicate from academic economic thinking the fantastically naive notion that the concrete "ends" of social action are immediately and definitely "given", and that the task of the "scientific" economist lies in devising the "most economical means" of achieving these "given ends".

<sup>84</sup> This is clearly implied, I think, by both Hayek and Robbins.

<sup>85</sup> Cf., e. g., Marshall, *Industry and Trade*—to say nothing of the volumes of more recent work.

It seems to me that effective cooperation between "theoretical" and "realistic" studies is still much hampered by the uses to which the former still quite commonly puts the terms "friction" and "imperfection". The naivete of identifying the two should be apparent. A "frictionless" system would be a timeless system. "Friction" is an *integral part* of

such assumption can be confidently made in the twentieth century. The very fact that it is made, however, suggests that there still remain, in this field of discussion, obscure methodological difficulties requiring careful and explicit investigation. Some of the insuperable difficulties associated with any attempt to construct a kind of "positivistic" economic "norm" in terms of atomistic conceptions of "free" or "perfect" competition will be explored in the study of Maximum Net Social Satisfaction through Time.

This Essay will therefore have failed of its purpose if the reader who has struggled through to the end does not now clearly perceive that a "purely positive" study of this kind—free as it is of all taint of ethics or metaphysics—is inevitably radically incomplete; and requires, even for *its own* completion, to be supplemented by an integrally related normative investigation. Our analysis and criticism of traditional demand analysis has shown that the real substance behind the persistent charges of "hedonism" to which the traditional analysis has been continually and, despite all defensive devices, repeatedly subjected, resides in the fact that it has been based on essentially "atomistic" and "mechanistic" conceptions. Psychological Hedonism was

any conceivable actual system. It may be either good or bad, necessary or unnecessary; and a finally valid judgment on this point in any particular instance always involves, in the last resort, comprehensive consideration of the part it plays in the system as a whole. As I shall show in a subsequent study, this means that it involves a philosophical judgment.

It may be pointed out that, even in a thoroughly "rational" economic universe, there might well be unemployment, of both men and resources, which was "involuntary" in the superficial sense that the idle, while idle, would "prefer" to be employed; but which would nevertheless be "voluntary" in the much deeper sense of being one of the necessary *and universally accepted* "costs" of the functioning of a total process rationally adjudged by everyone, the unemployed included, as on the whole "better" than any available alternative system. I believe that the term "waste" as very frequently employed in quite serious discussions is saturated with sentimentality.

merely one manifestation, in one particular science (intimately and irretrievably interrelated with economic science), of a scheme of thought which dominated a whole age—and every department of specialised intellectual activity during that age. The philosophical collapse of automistic mechanism a century ago has been followed by its collapse in one after another of the special disciplines—its most spectacular defeat being in physical science itself. Economic science is today in grave danger of being—if, in fact, it is not already—the laggard of the intellectual world.

The simultaneous emergence of "Hedonism" in both Ethics and Psychology is thus a fact of vastly more profound significance than the oft-repeated refutation of the illicit "reconciliation" of the two. It is no mere "historical accident" that positive economic science in its infancy was "associated" with psychological hedonism; and it is no mere "historical accident" that normative economic science has been associated with ethical hedonism. If the "breakdown of mechanism" in psychology calls for radical transformation in even the most abstract apparatus of positive economic science, it is only reasonable to expect that the "breakdown of mechanism" in ethics calls for radical transformation in the formulations of normative economics.

. . . always there remain the same beacons that lure. Systems, scientific and philosophic, come and go. Each method of limited understanding is at length exhausted. In its prime each system is a triumphant success: in its decay it is an obstructive nuisance. The transitions to new fruitfulness of understanding are achieved by recurrence to the utmost depths of intuition for the refreshment of imagination. In the end—though there is no end—what is being achieved, is width of view, issuing in greater opportunities. But opportunity leads upwards or downwards. In unthinking Nature "natural selection" is a synonym for "waste". Philosophy should now perform its final service.

It should seek the insight, dim though it be, to escape the wide wreckage of a race of beings sensitive to values beyond those of mere animal enjoyment.<sup>86</sup>

But the devil can cite Scripture for his purpose; and it is therefore well to remember also that

. . . in each revolution of scientific thought new words are set to the old music, and that which has gone before is not destroyed but refocussed. Amid all our faulty attempts at expression the kernel of scientific truth steadily grows; and of this truth it may be said—The more it changes, the more it remains the same thing.<sup>87</sup>

If “the most un-Greek-like thing we can do is to copy the Greeks”, the Classical definition of Classicism in economic science is that of the greatest of all Classical economists:

I do not myself hold a classical author to be one who more than others has said things which are true, as they stand. I don't feel myself bound to agree with him on many points, not even on any point. But he is not for me classical unless either by the form or the matter of his words or deeds he has stated or indicated architectonic ideas in thought or sentiment, which are in some degree his own, and which, once created, can never die but are an existing yeast ceaselessly working in the Cosmos. With that definition I can to my own satisfaction say pretty well whom I regard as classical economists.<sup>88</sup>

In seeking to bring the Foundations of economic science more thoroughly into accord with the kind of superstructure that must be built upon them in the twentieth century, we shall find, in investigating more profoundly the “Theory of Competition”, that “perfect competition” and “perfect

<sup>86</sup> Whitehead, *op. cit.*, pp. 203-4.

<sup>87</sup> Eddington, *The Nature of the Physical World*, p. 353.

<sup>88</sup> Alfred Marshall, Letter to James Bonar, *Memorials*, p. 374.

cooperation" are *identical* concepts: and we shall discover ourselves to be engaged again, with the aid of a more profound metaphysic than was at the disposal of Adam Smith, in *An Inquiry into the Nature and Causes of the Real Wealth of Nations*.

## VITA

---

RALPH WILLIAM SOUTER was born in Dunedin, New Zealand, February 13, 1897. After service in the World War he received the B. A. from the University of New Zealand in 1922 and the M. A. in 1923, specializing in Philosophy and Economics. In January, 1928, he was awarded a Rockefeller Fellowship from New Zealand and came to study at Columbia University. He held this Fellowship until January, 1930.

In July, 1929, he was appointed Lecturer in Economics, Faculty of Political Science, Columbia University, which position he now holds.

He is a frequent contributor to economic journals.



**CHECKED**  
**8003-04**