For the last century, economic analysis has been wedded to the idea of equilibrium, in spite of the evident fact that most economic relationships are in flux. The theory of transformational growth in this work replaces equilibrium with history. The role of the market is not to allocate resources, but to generate innovations, which are “selected” by competition in an evolutionary process. These innovations in turn change the way markets work and how they adjust, thus creating new problems and new kinds of pressures to innovate. Different historical periods can be distinguished, with a new one perhaps on the horizon. In each period a distinctive style of technology prevails, associated with corresponding institutions and patterns of market behavior.

To analyze market behavior requires theory. This rests on a core of basic value relationships between prices and profits, growth and quantities, centering on the cross-sectoral wage-bill capital requirements condition, which can be shown to reflect the Golden Rule, and to imply a linear wage-profit tradeoff. The core relationships provide the foundations for a theory of monetary circulation, which makes possible a revised Keynesian approach, based on Classical foundations.
THE GENERAL THEORY OF TRANSFORMATIONAL GROWTH
The general theory of transformational growth
Keynes after Sraffa

EDWARD J. NELL
The New School for Social Research
In memory of Marcella
With gratitude to Marsha
And hope of better times
For Adam and Jacob, Miranda and Gwen
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PREFACE

Economics is a difficult subject at the best of times, but taking an unconventional approach makes it doubly so. It is hard enough to keep up with current research as it is, so there must be important reasons for asking the reader to take the trouble to look at the world in a different way.

Moreover, some efforts at a new approach may give one cause to be skeptical. A good deal of the best work is chiefly critical (which is emphatically not the case here). Such work is widely ignored, or cited only in obscure footnotes. It is generally acknowledged that neo-Classical theory has significant flaws – but it is equally widely believed to be the only useful and relevant scientific approach. Neo-Ricardian theory, for example, is just as abstract, calls for a counter-intuitive interpretation of Keynesian macroeconomics, and has little or no behavioral theory. Its approach to policy and empirical work is underdeveloped, to say the least. The post-Keynesian school has a better record on policy and empirical work, and begins from a reasonable position in macroeconomics, but has an eclectic approach to pricing, and, indeed, to theory in general. Some would say it is more an attitude than a doctrine.

By contrast, neo-Classical theory is not only fully articulated in virtually every area of interest, but is able to find room for almost any position on any issue. It has become the language of economics, and like a language it has a grammar. But the grammar does not commit one to any particular explanation or ideology over another, any more than English or French grammar does. However, it does have implications, as we shall see.

Nevertheless the alternative schools have focused heavily on criticism, and this has not proved useful. Firing critical salvoes at neo-Classical theory can be considered anything but constructive. For one thing, neo-Classical thinkers themselves have already done the job. In a practical sense general equilibrium theory has all but self-destructed. What can we learn about the economy from it? It is unrealistically abstract, yet nevertheless requires strong assumptions with little or no economic justification. And even so, it still leaves us with the possibility of multiple equilibria, some or even all of which may be unstable. Moreover, the theory cannot easily accommodate a general rate of profit on capital. How
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then can it be a theory of capitalism? What has happened to the profit motive if business and finance are not interested in obtaining the highest returns on their investments? Even worse – if possible – conventional general equilibrium theory can incorporate only the most rudimentary concept of money, and it handles that badly.

The reply is surely that general equilibrium theory is not supposed to be practical – it was designed to illuminate market interdependence. Practical theory operates at a different level, the partial equilibrium theory of Chicago, or Solow’s “middlebrow” aggregate theory. Of course, these approaches are legitimate targets for “capital theory” criticisms. But again, what use is such criticism? How will it help anyone to understand the causes of unemployment or inflation or the productivity slowdown, or whether corporate pricing policies will pass along the effects of changes in exchange rates? Flawed or not, neo-Classical models can be used to address the questions of the day. If the flaw is important, account can presumably be taken of it in drawing conclusions from the model. Or the model can be recast to avoid relying too heavily on the flawed bits.

The critics try to show that neo-Classical theory rests on one or another fatal error of logic, for example, in distribution theory, or in theory construction in regard to effective demand. Yet this sometimes seems almost arrogant. Is it plausible that so many, for so long, have been deceived by so few – the founders of neo-Classical theory? Is the last century of mainstream economics nothing but a vast sea of errors and mistakes? Did neo-Classical theory develop and become dominant for no good reason at all? Can all this be explained by scientifically irrelevant factors, for example, that it serves as an ideological cover story for modern capitalism? This hardly seems convincing.

On the other hand, the defenders of neo-Classical thinking do not seem on firm ground, either. Virtually every trained economist has his or her own pet peeve about accepted professional thinking – an utterly indefensible, but basic, assumption here, an outrageous oversimplification there, in some other place a conclusion wholly at variance with well-established facts. These are not isolated cases; the aura of unreality is absolutely pervasive. Everyone who has worked with neo-Classical models has felt the sense of being in an imaginary land – a market-driven Oz with an all-too-fallible Wizard half-concealed behind the veil of assumptions, working the optimizing machinery.

Critics and defenders alike tend to present the profession with a choice: the theory, or parts of it, are right or wrong. It passes or fails. If it passes it can continue to be accepted; if it fails it must be replaced by a correct, or more nearly correct, theory. The Bohr model of the atom was accepted as correct; it was then shown to be flawed, and replaced by a model based on
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quantum mechanics. The critics contend that the “capital theory” arguments have shown the flaw in marginal productivity theory, and therefore in the whole edifice of costs and supply, as well as distribution theory. Defenders maintain that the flaw is truly marginal, and that, in any case, no quantum mechanics are in sight. “You can’t beat Something with Nothing.” Both sides treat the question as an issue of the timeless truth of a scientific theory.

Atoms have no history. They do not behave at one time, in one era, in one way, and then differently at another time or place. Markets, however, may do just that. They are social institutions, and institutions develop and change, historically. If markets do change, then a theory describing their working may be true for one era, for one time and place, and not true for another. Classical theory, or theories, for example, might describe the eighteenth and early nineteenth centuries, neo-Classical approaches the later nineteenth and early twentieth, and variants of Keynesian theory, the Great Depression and after. This is not to underrate the importance of getting theory right; a model with a logical flaw describes nothing at all. But it is to suggest that different approaches to theory may have developed for good historical reasons, and may reflect different institutional arrangements – differences that may still be relevant today.

But there is no room for such an idea in the dominant methodology of economics. The language of the subject is the vocabulary of rational choice, its grammar, the algebra of optimizing. Rationality is the same in all eras, all times, and all places. The setting and constraints may differ, but the problems and the methods of solution are the same. Markets at bottom reflect rationality, i.e. optimal choices, which, in competitive conditions, will emerge in equilibrium. The method of analysis is to compare equilibria; dynamics, insofar as it is necessary or useful, is equilibrium dynamics, or concerns the stability of equilibria.

It is not what this points to that is objectionable; optimizing surely has a role in any sensible economic analysis. But the neo-Classical approach uses optimizing to determine equilibrium, usually understood to be the norm toward which ordinary behavior will gravitate. In actual practice, optimizing – for instance, operations research – is generally used to find a way to improve on the current norms of ordinary behavior. Normal behavior – the empirical counterpart to equilibrium – is likely to reflect rules of thumb and practical compromises; optimizing will lead to changes in this behavior – changes which may be quite disruptive.

The problem is that in focusing on allocation the neo-Classical approach has directed our attention away from vastly more important problems – the nonallocational effects of markets, their role in creative destruction. Competition may or may not have strictly allocational effects.
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If it does they are surely minor compared to its role in bringing about innovation and productivity growth, causing uneven development, leading to stagnation, inflation, booms and busts, and all the rest.

The pernicious effect of the presumption that economic analysis must be equilibrium analysis can be illustrated by considering the theory of steady growth. This began from two sources in the 1930s—the von Neumann model, and Harrod's famous essay, amplified later by Domar. During the 1950s and 60s an explosion of models developed the Harrod-Domar analysis, the neo-Classical counterapproach, a neo-Keynesian version, and two-sector variants of both. These were used to explore problems in distribution theory and the monetary role in growth, and to develop the theory of optimal growth, among other things. Multi-sector models were developed to examine the conditions for steady growth, the nature of the turnpike, the optimal choice of technique, and many other topics.

During this time, while economists were developing the theory of steady growth,

- most advanced capitalist economies (ACEs) operated with a substantial and varying margin of excess capacity
- most Soviet-style economies operated under varying conditions of shortage
- in all ACEs, the share of agriculture in labor and output steadily declined
- in all ACEs, the share of manufacturing first rose, then leveled off; in some it then began to decline
- in all ACEs, the share of services rose, and changed character
- in all ACEs, the share of white-collar work rose relative to blue-collar
- toward the end of the period, “high-tech” industries began to increase, while steel and other traditional Mass Production industries began to decline.
- in the United States recent “downsizing” has brought a sharp loss in high-paying manufacturing jobs and in white-collar middle management

Except for the last, these have been persistent trends, forming a long-term pattern. Arguably – it will be argued in this book – this was the way the economy grew; specifically, this was how it worked to increase productivity. But in developing growth theory this pattern has been ignored – in the “new” growth theory as much as in the old. While it is perfectly reasonable to disregard short-term fluctuations in conducting a long-term analysis, it is not reasonable to ignore persistent long-term changes. There must be something wrong with a discipline that devotes its
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scarcest and most valuable resource – high-powered analytical skill – to the analysis of steady, balanced expansion in the face of a pattern of persistent trends leading over a century to fundamental changes in the proportions and structure of the economy. In general, economists have allocated a massive amount of theoretical time and energy to the study of equilibrium, in spite of the all-too-obvious fact that it is seldom, if ever, encountered.

There may well be good reasons to study equilibrium. Certainly a large and complex array of theoretical tools has been devised, for which any serious student must be grateful. Yet it can easily be seen that the price – the opportunity cost – has been high: we know very little about the way the ACEs actually grow, particularly about how they generate productivity growth. Indeed, our theoretical understanding of the causes and effects of productivity growth is, in general, remarkably slim. And part of the reason must be that economists, concentrating on equilibrium, have paid comparatively little attention to historical dynamics and the causes of structural change.

The theory of Transformational Growth is an effort to make a fresh start, to begin all over again, from History rather than from Equilibrium, from structures and institutions rather than from rational choice. In particular, markets will be approached as competitive monetary exchange systems, rather than implicit barter systems. This will require a new treatment of the theory of money, one in which circulation, rather than asset-holding, assumes pride of place, and in which circulation velocity is tied to productivity.

In emphasizing History we shall nevertheless find a place for Equilibrium; in emphasizing structure and institutions, we will find a place – many places – for optimizing and rational choice, so long as the relative roles are properly understood. Traditional microeconomics, in its Marshallian configuration, will turn out to have a major assignment, as a theory of market adjustment under certain conditions. Even equations determining vectors of barter exchanges will find a place in the sun – though only at certain times of day. The traditional tool-box will find many tasks. Much of the vocabulary of economics will remain, but there will be significant changes in grammar. So the overall balance will be very different, and the subdued roles assigned to many conventional stars may not prove satisfactory to some audiences. Nevertheless, this appears to be the price which must be paid to convert our subject into one capable of addressing the transformations of the economy.
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Two recent books of mine complement this one. Transformational Growth and the Business Cycle provides empirical support for many of the positions developed here, and Making Sense of a Changing Economy sketches the larger picture and draws some of the policy conclusions. Finally, I should note that I have written on many of the subjects covered here on previous occasions. Much of what I have said in those earlier efforts is superseded by the present work.