Preface

I became an economist because I sought the answer to a simple question, how do you accelerate the rate of economic growth in different countries and reduce the worldwide problem of poverty? It is clear that in order to understand how to do this you need to understand the determination of economic growth in the past. This book is an attempt to meet the challenge of reconciling a long-term with a short-term perspective and to bring together the general and the particular in a persuasive explanation of the inception of modern economic development. Despite an explosion in relevant publications on economic growth, there is still no good explanation of why modern economic development has occurred where and when it has. It is disappointing that that there is such a marked disproportion between the effort expended and its return. There are still no agreed policies on how governments can promote modern economic development. If there were, economic development would no longer be exceptional and the absence of significant economic development so common.

This book is the culmination of an academic career which has followed a sinuous course through the disciplines of economics, economic history and the area of management studies. The evolution of these discipline areas is disappointing from an intellectual perspective. There are themes which I have pursued throughout this career, notably concern with the analysis of economic performance both of nation states and of enterprises, focusing on why some units achieve good performance and others not. Country performance is the result of the performance of a large number of enterprises. I began my career studying history and economics at Cambridge University. During the 1960s there was a pessimism about the prospects for economic development ever becoming anything more than a European phenomenon and at Cambridge, a major debate about how to conceptualize the process of economic growth. There were serious criticisms of the basic neoclassical approach to economics. I still remember James Meade beginning a course on economic principles by saying that he would begin with 24 assumptions about the economic world he was analysing and relax them one by one – still to my mind an extremely odd way of getting to the real world. In that period I began research on an aspect of Soviet planning – an experience which told me what policies you should not adopt, and moved on to the study of long-term comparative economic
history, reflecting a feeling that there are persistent tendencies in history much stronger than often thought, that the Russian Revolution of 1917 has changed much less than usually assumed. This interest was reinforced when I migrated to Australia and extended my knowledge to its history and that of other areas of new settlement, and later to the Asian economic miracle.

The debate on economic growth petered out for nearly 20 years. The focus turned away from economic growth until the late 1980s when the new growth theory became the focus of attention, initiating another critique of key aspects of neoclassical growth theory, including a failure to properly account for technical change and increasing returns. There is a sense of déjà vu about the development of the new economic growth theory in the 1990s. Old issues were reopened, old weaknesses of neoclassical economics re-explored. Yet once more the new theory was reabsorbed into the neoclassical model, as had Keynesianism and neo-Keynesianism been before. There were some improvements. There has been a much more systematic attempt to test the theory against the real world and a more open-minded approach to exploring the theory by a few individuals. During this renaissance of interest in economic development, I was engaged in administration heading, successively, departments of economic history, economics and management. The fragmentation in method and approach of what should be linked disciplines is alarming. My research interest became focused for a period on strategy, at both the corporate and government levels, and the influence of risk and uncertainty on foreign direct investment decisions. In the latter work I was surprised to find in financial theory a total dominance of neoclassical economics and a failure to see the divorce between textbook models and reality. The end of the Cold War and the dissolution of the Soviet Union, with the simultaneous transition of planned to market systems, has left economics dominated by one theoretical paradigm. Throughout this period I have continued to read economic history and to note the application of rigorous theory and quantitative techniques even within this area, following the cliometric revolution. The total dominance of neoclassical economics has puzzled and troubled me, given its obvious and much discussed weaknesses. As a consequence, I set about a systematic reading of the rather large literature on economic growth theory which now exists, in an attempt to understand how economic theorists working within the neoclassical paradigm understand the economic growth process and what can be taken out of the theory helpful to understanding the process of economic development. Neoclassical theory is too entrenched to disappear. Sometimes theorists, when undisturbed by rival approaches, become trapped by their own theory, not even attempting to show the implications of that theory
for understanding real world processes. There is a tendency to take for
granted a particular paradigm without justifying its main assumptions
and the world view on which it rests. Neoclassical economists are poor
publicists for their own theory and they do not even notice the horror with
which it is greeted by many in other disciplines, a horror arising from the
failure to relate the theory to real world problems. There is a desperate
need for better understanding of the process of economic development and
of what neoclassical theory can contribute to that understanding.