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UNDERSTANDING ECONOMIC GROWTH AND ECONOMIC DEVELOPMENT

Review of *Explaining Growth: A Global Research* Perspective, edited by Gary McMahon and Lyn Squire and *Productivity Growth, Inflation, and Unemployment: The Collected Essays of Robert J. Gordon* by Robert J. Gordon

Economists' writings on economic growth tend to be uncharacteristically humble. It is commonplace to read that the profession "knows so little about such an important topic" or that "there are still more questions than answers" or that the answers are "elusive." Yet, almost everybody agrees that economics as a discipline started in an attempt to understand the creation of "The Wealth of Nations." Why does economics have such a poor track record?

Although we can with confidence state that investment in physical capital matters for economic growth, that institutions matter, education matters, incentives matter, competition and trade policies matter, etc., the degree of confidence varies significantly across economists (by *how much* do they each matter?), and it is not always transparent *how* each of these factors help achieve sustained economic development and growth. From the beginning, divisions have been deep at the theoretical level. The message of the neo-classical model developed by Solow (1956) and Swan (1956) (and reviewed in Barro and Sala-i Martin, 1995) is loud, clear and at first somewhat surprising: unless economists are willing to give up their long-held conviction in diminishing returns to capital, then they must ultimately concede to the implication that long-run growth is bounded by the growth of technology. However, according to the initial versions of these models, technological progress consumed no real resources and was not intermediated through markets: it was unsolicited and serendipitous. More recently, models of "endogenous" economic growth (reviewed in Grossman and Helpman, 1991; Aghion and Howitt, 1998; Howitt, 2004) stress that purposeful creation of new knowledge and technology (or total factor productivity growth) is the ultimate source of long-run growth. According to this viewpoint new knowledge is the outcome of profit seeking activity, and the organization of "institutions" and "markets" within which this activity is undertaken are crucial for the rate of technological progress. Endogenous growth theorists argue that research and development (R&D) and human capital accumulation, both viewed as the outcome of a rational trade-off between current investments and future (expected) returns, is essential for sustained economic development. None of this conceptualization should be shocking to economists, although some of the technical assumptions that are necessary to generate sustainable economic growth remain controversial. The real shock is

Note: Without implicating them for any of the remaining errors and confusions, I wish to thank Ben Dennis and Lars Osberg for helpful comments and suggestions.

to find out that the empirical growth economists are running cross-national growth regressions after culling data, some of which are of dubious quality, from all sorts of countries that make up the world atlas, and are trying to squeeze robust results out of data sets in which potential conditioning variables often outnumber the sample size. As a brilliantly titled article by Sala-i Martin (1997) put it: "I just ran two million regressions"!

Fortunately, two new books by Robert Gordon (2004) and Gary McMahon and Lyn Squire (editors, 2003) remind us that economic growth is not *just* about mining data, and getting bogged down in technical details and esoteric theorizing. These two books—both of very high caliber—demonstrate that it is possible to make considerable progress on economic growth by going back to economic principles and devoting time and thought to issues of economic significance. Gordon (2004) is a collection of essays on productivity growth, inflation, and unemployment; the three main pillars of macroeconomics and monetary economics. For the balance of this review, I shall focus on Gordon's writings on productivity growth, although his work on inflation and unemployment is equally important and well known. While most of the essays included in this book have been previously published, there are comprehensive introductions to each of the three parts of the book, as well as several essays that are published for the first time. McMahon and Squire (2003) summarize the collaborative output of an ongoing, ambitious and fascinating "Global Research Project—Explaining Growth" undertaken under the auspices of the Global Development Network (GDN)—an independent association of research and policy institutes from around the world "whose goal is to produce, share and apply to policy multidisciplinary knowledge" relevant to economic development. The first phase of the project commissioned studies on the growth experiences in six regions of the developing world (East Asia, South Asia, Latin America and the Caribbean, Eastern Europe and Former Soviet Union, Middle East and North Africa, and Sub-Saharan Africa). This volume summarizes and fleshes out the common findings emerging from the regional studies, and is organized in four thematic topics: sources of aggregate growth (written by Charles Soludo and Jongil Kim), microeconomic determinants of growth (Sergei Guriev and Djavad Salehi-Isfahani), markets and growth (Štěpán Jurajda and Janet Mitchell), and political economy of growth (Micael Castanheira and Hadi Salehi Esfahani). There is also an introductory chapter by the editors that provides a broad perspective on why the themes were chosen in the first place, and a concluding chapter (Lant Pritchett) that builds on the findings of these chapters and identifies issues that may be particularly important for individual country studies—which apparently will be the next phase of the project. Quite fittingly, both volumes come with a forward by Robert Solow whose insights are, as usual, fresh and stimulating.

The Global Research Project emphasizes markets and incentives as the key to understanding economic growth and economic development. It attempts to place markets in an institutional context within which various "agents" of change, such as consumers, firms, politicians, and voters interact. Most economists would feel comfortable with this approach, especially given the authors' ability to frame complicated issues using the modern language of macroeconomics, institutional economics and political economy. However, some (especially those with a "struc-

turalist" bent) would most likely think that not all the forces of economic development have received due attention. Many traditional issues that preoccupied structuralists—such as technology transfer and adoption, agricultural policy, and urbanization—are conspicuously missing in this volume. Gordon's emphasis is, by contrast, on the structural determinants of productivity growth.

Such differences in emphasis ("agents" versus "structures") has intriguing implications for cross-national empirical growth analysis, which views the most and less developed countries along a continuum of economic development, assuming that output per worker in, say, the United States today can be viewed as output per worker in, say, Niger extrapolated into the (not so distant) future. One could qualify this view by taking into account factors that lead to permanent differences in income levels, but the crux of this empirical strategy is essentially the same: contemporary data on cross section of countries is economists' time machine. Remarkably, both the neoclassical growth economists with their emphasis on supply (e.g. Barro, 1997) and structural economists with their emphasis on demand (e.g. Chenery and Syrquin, 1975) advocate this approach. Thus, despite covering different territories (the Global Development Project examines the less developed regions and Gordon focuses on one of most developed regions of the world, the U.S.), the agents versus structures dichotomy underlying these books is a useful reminder about the existing plurality of opinions on economic development, and cautions us about indiscriminate use of cross-national regressions.

What is new in these two volumes? There are many novel ideas and findings, but three interrelated issues in the context of both within and cross-national studies of economic growth stand out: data collection, heterogeneity and aggregation.[1] The cross-national growth regressions that form the main pillar of new empirical growth literature are very appealing to economists because they are thought to be informative about systematic differences across countries in per capita output and the proximate or ultimate drivers of (total factor productivity) growth (Barro, 1997). (The availability of the Heston and Summers data set (updated version: Heston *et al*., 2002) made this exercise vastly easier.) One of the contributions of this literature has been to construct proxies to measure "human capital" at the aggregate level.

As well, given the uncontroversial idea that diffusion of existing knowledge and production of new knowledge is intermediated through institutions and markets, there has been a flurry of activity searching for indexes that somehow "measure" the quality and nature of the economic environment broadly understood. Bold attempts include Mauro (1995) and Acemoglu *et al*. (2001). But, there are also numerous traps. Some of these indexes tend to be highly subjective (e.g. "institutional quality"), or mostly driven by forces of economic development and well-being that the indices themselves purport to explain or that measure something different than their labels would suggest (e.g. "openness to international

[1]Given that feedback effects are unavoidable in economic development, from an empirical standpoint, another important issue is endogeneity of many (all?) economic, social and institutional variables. The endogeneity issue primarily effects the judgements about by how much an "exogenous" change in an input today contributes to future economic growth. Whereas tightly parameterized theoretical models of economic growth allow one to implement exogenous changes and quantify their consequences, econometric models can only identify exogenous changes under restrictive assumptions.

trade"). For example, Rodríguez and Rodrik (2000) forcefully argue that the ubiquitous index of Sachs and Warner (1995) mingles proxies for macroeconomic policy making (whether the country had socialist economic policies or had parallel market exchange rate premium), but is called a measure of trade openness. While busy working with these highly controversial indices or instrumenting them to control for endogeneity, economists have shied away from more direct measures of policies (e.g. tariff rates), which tend to have low correlations with future growth. The more fundamental measurement issues that underlie growth empirics have been ignored or neglected.

Consider the case of capital services. The standard method of estimating capital services is based on the estimates of capital using the perpetual inventory method. For this method to work properly, one typically requires reliable capital stock estimates from a reference year, and cumulates *net* investment forward. This method implicitly assumes that investment recorded in national income accounts is an efficient process, whereby current resources are used to expand immediately the actual productive capacity of the economy. How closely does this match reality? Suppose a corrupt regime (e.g. in Nigeria) invests over twenty years five billion dollars on a steel plant (e.g. Ajaokuta Steel Company), that is supposed to employ over 100,000 workers, without ever producing steel (Kasumu, 2004). Or, suppose a country (Uganda) reports significant public investment targeted for education, but only a small fraction of it reaches to schools (Reinikka and Svenson, 2004). If such instances are widespread, use of the perpetual inventory method to measure physical capital stock leads to nonsensical conclusions. Further, the use of common deprecation allowances and asset lives to calculate net investment is meaningful only if maintenance and utilization rates of existing machinery, equipment and structures are uniform across countries. Natural disasters and civil wars cause (at the margin) disproportionately larger destruction to capital stock in developing countries, but these are rarely accounted for in cross-national studies. Mechanical application of the perpetual inventory method without being informed by particulars of individual countries glosses over these issues.

Although collection of primary data is not one of the stated goals of the Global Research Project, improving the quality of data commonly used by empirical growth economists is a potentially high value-added enterprise, and the project has a unique opportunity to coordinate such an effort. For instance, individual country or regional profiles and chronologies that inform users of these data sets about their precise drawbacks and cross-country as well as time-series comparability problems will be welcome additions to knowledge. Capital services have already been mentioned but measurement issues surrounding employment are no less pressing. Clever instruments to control for endogeneity seem to carry a handsome premium in the academic publication business, but what use do they have if the genuinely important data are, shall we say, noisy?

These data related issues are not of second order when it comes to measuring sources of economic growth and productivity growth. Gordon (chapter 2) argues that such measurement issues can fundamentally change the way we interpret economic data. Consider the notion of balanced growth path (BGP) along which basic aggregate variables such as consumption, output and capital grow at the same constant rate, and hence the long-run output to capital ratio is constant.

Long-run growth models sometimes exclusively focus on the BGP. BGP is also the focal point of the dynamic general equilibrium macroeconomic models which essentially analyze the response of an economy to "shocks" as it departs from and return to its BGP. The empirical justification given for the BGP is the Kaldor (1963) facts which are thought to receive their primary support from the post-1940 U.S. data.[2] Gordon carefully re-estimates the aggregate U.S. capital stock after making adjustment for composition and quality and finds that output to capital ratio was indeed fairly stable between 1870 and 1929, but became highly volatile until the end of the Second World War, and has been *decreasing* since then (until 1996).

This finding not only requires macroeconomists to rethink their balanced-growth path assumptions surrounding aggregative models of economic growth and fluctuations, but also turns out to have important implications for the productivity slowdown and revival literatures. Indeed, equipped with the new estimates of capital stock, Gordon shows that from 1870 to the early 1970s total factor productivity growth rate was consistently high (with some acceleration after 1913), which he calls the "one big wave" in long-term productivity growth. Gordon's insistence on one big wave of productivity growth is colored by his deep conviction that technological innovations and inventions from 1870 to 1970 were fundamentally different from more recent innovations, and at the margin remarkably productive. Gordon argues that, if we rank technological revolutions according to their marginal contributions to economic growth, computers do not score all that well. Although innovations in information and telecommunications (IT) industry have been important, Gordon is a staunch skeptic of the "New Economy" hype, and thinks that the marginal contribution of IT to productivity was much higher when first adapted by large corporations and the governments around mid-1970s. Industry composition effects would have boosted productivity growth even further: businesses that have the largest marginal return to adapting to IT technology would also be the most efficient ones and would flourish, and those with the least return would be the least efficient and would exit. But, despite the initially higher marginal rates of return to new technology use and compositional effects, the productivity growth rate was low and flat from 1972 to 1996.

Despite Gordon's insistence on diminishing marginal returns to commercially profitable new knowledge, historical evidence can be interpreted more favorably for the computer. David (1990) draws a parallel between the computer and the dynamo, because both dynamo and computers share the common technological characteristic of large set-up costs and give rise to network externality effects. The invention of the dynamo did not lead to an immediate, dramatic jump in the productivity growth rate. It took decades (until the 1920s) for some factories to retool and for profit-maximizing management to adapt to the organizational realities of switching from shaft drive to wire. In the case of dynamo, the evidence is consistent with the slow productivity growth hypothesis at the initial implementation stage, as it was not until the 1920s an acceleration in productivity growth rate took place

[2]The Kaldor "stylized facts" are: (1) constant growth rate of output per capita; (2) constant capital-output ratio; (3) constant shares of capital and labor in national income; and (4) constant real rate of return to capital.

(as Gordon's own numbers show). So, if history of technology and technological diffusion is of any guidance, Gordon may have too harsh a verdict on computers, especially since the recent productivity numbers from the U.S. have been encouraging for the opposite camp (Stiroh, 2002). Indeed, some (e.g. Greenwood and Yorukoglu, 1997) have argued that the productivity slowdown was transitory, largely reflecting learning costs of transition to a novel general purpose technology.

Time will tell whether the current productivity trends are sustainable. In the meantime, however, economists who favor explanations of endogenous, sustained technological progress based on incentives and markets still have to figure out why the productivity growth rate was so much *higher* during the "one big wave." After all, it is difficult to argue that economic conditions were extremely favorable to high productivity growth, especially after the 1930s, with limited global openness to international trade and capital movements, regulated markets in which governments had a much more redistributive role, and significant destructions in world capital stock and infrastructure in a major world war. The question Gordon poses is an important challenge for the advocates of current endogenous growth models.

However, discussion of productivity slowdown at the technological frontier has very little to say about economic growth in developing countries. While the most developed economies are trying to invent the fundamental sources of growth that will fuel their economies in the next 10 to 20 years, less developed economies are still trying to close the standard of living gap, which is arguably a more easily identifiable target. Since level accounting tells us that efficiency differences across countries is one of the most important contributors to income differentials (Klenow and Rodriguez-Clare, 1997; Hall and Jones, 1999; Kumar and Russell, 2002), economic development almost appears like a trivial, deterministic process: adopt the technologies, markets and institutions of the early developers, promote the shedding of "surplus labor" from agriculture into industry and services, gently ride the capital deepening saddle-path, and let others do the costly, uncertain inventing.

Since we know so little about the "right" institutions, markets and technologies to adopt—despite (or thanks to) "two million regressions"—the Global Development Project thinks that it is timely to shift attention to a better understanding of current markets and institutions in less developed countries, and ask why some of these have not delivered growth rates comparable to those that we have seen elsewhere. Rather than simply dismissing the existing structures of less developed countries as inherently inefficient or as reflective of rent seeking behavior, the project focuses on the "agents," as well as the incentives and the constraints surrounding the institutions and markets. This may ultimately give us the clues we badly need about why these institutions and markets came into existence in the first place, and allow us to change the (bad) incentives and relax the binding constraints (such as credit market imperfections). Such is the goal set by the Global Research Project, and the participants have their work cut out for them.

The primary challenge here is the perennial question facing macroeconomics: heterogeneity and aggregation. What is the appropriate level of aggregation that we should want if we think that markets and incentives are essential in understanding and promoting pro-growth policies? Consider the assumption of a unified

labor market which ultimately reflects the labor supply and demand decisions of all the relevant microeconomic units. Although this is, no doubt, an attractive starting point, and can potentially play a critical role in cyclical and structural adjustments as a mediator of labor reallocation, choice sets and constraints vary dramatically across "agents." For most rural households a crucial choice is dividing their time between on- and off-farm employment (if any is available). Integrating a national labor market may require considerable worker reallocation from isolated rural areas to urban centers, a painfully long process. Even with full integration of urban and rural labor markets, wage gaps across similar skill categories typically persist for decades. More importantly, there are often striking differences across regions in terms of educational attainment and investment in human capital. The resulting regional and occupational earnings differentials were in fact a very important ingredient of the twentieth century U.S. industrialization, although such comparative histories of developing and industrialized countries are typically underappreciated (Schultz, 1964; Mundlak, 2000). Economists traditionally have a hard time accepting deviations from a well-defined parity. Typical explanations, such as transportation costs in the case of regional wage gaps, don't seem to be nearly large enough to account for the observed differentials. Furthermore, modeling the origins of these deviations and working out their implications is technically much more demanding and therefore much less standard. But, none of these "imperfections" or "frictions" will go away (despite our convenient assumptions) and it would be useful to know how important such deviations are for short- to medium-run growth and income distribution. At a minimum, it would be useful to know how much economic policies can influence the sources of these imperfections or frictions.

Interestingly the Global Research Project makes almost no reference to the "structuralist" literature whose original premise was to recognize the overwhelming microeconomic heterogeneity encapsulated in "sectoral imbalances" (Lewis, 1954; Kuznets, 1966; Chenery and Syrquin, 1975). This literature emphasized the role of differential employment absorption capacities of different sectors during economic development. Due to Engel's law, productivity growth in agriculture would lead to surplus farm output, declining farm terms of trade, reallocation of labor into non-farm employment and urban centers. The demographic changes that accompany structural transformation were also an important part of the equation, though they are less well understood.

The structuralist literature had many weaknesses, including perhaps an undue emphasis on the manufacturing employment and output growth as the primary driver of economic development. Putting so much premium on industry, one could argue, led to the neglect of the farm sector, and created a policy environment which encouraged excessive taxation of agriculture (Binswanger and Deininger, 1997). As well, this literature may look rather outdated for the technically inclined—a convenient excuse to dismiss the arguments as "muddied thinking." But, the structural transformation literature is well and alive (Eschevarria, 1997; Kongsamut *et al*., 2001), and reminds us that single-good models of economic growth with their implicit homothetic preferences are hugely inadequate for understanding long-term economic growth. The nature of agricultural output as a subsistence good implies situations in which domestic savings rate endogenously increases with

economic development. The rate of economic growth is interrelated to the growth rate of non-agricultural employment, which in turn depends on both the relative and absolute farm productivity growth. Twentieth century U.S. experience, for instance, suggests that farm out-migration was strongest when farm productivity growth outpaced non-farm growth rate (Dennis and İşcan, 2004). Publicly funded basic research at agricultural research and experiment stations was essential for observed rates of productivity growth and technical change (Griliches, 1964; Huffman and Evanson, 1992). If less developed country experiences of urbanization continue to parallel those in the more developed countries, there will be enormous challenges ahead for the provision of public goods and amenities (Williamson, 1988). One would think that many relevant policy changes can emerge from interpreting country case studies through the lenses of gradual structural transformation, rather than through those of Solow-Swan or endogenous growth models alone.

The premise that a careful analysis of markets and incentives will reveal the answers to pressing issues in developing countries is promising but one should not be too naive. Even under circumstances which are free of political interference and price distortions, if R&D is predominantly undertaken in advanced capitalist economies with an eye toward their own comparative advantages, developing countries may find the resulting technologies inappropriate for their own relative factor endowments and comparative advantages (e.g. Acemoglu and Zilibotti, 2001). Simply importing the cutting-edge technology (although a reasonable thing to do in the Solow-Swan model) may not be optimal. Specialization due to international trade can also inhibit the catch-up of late developers with early developers (Acemoglu and Ventura, 2002; Cuñat and Maffezzoli, 2004). Is it sufficient to state in passing that developing countries should be mindful of these possibilities? Or should they design mechanisms (markets) that direct innovation and specialization toward their own needs and relative factor endowments? It is doubtful that a framework which sidesteps heterogeneity and assumes that the right incentives will ultimately deliver symmetric international outcomes is adequate for development analysis.

The problem is further compounded under partisan and fragmented political settings. Pro-growth incentives are designed and implemented within a political context, and it is doubtful that undemocratic institutions that divert funds to maintain the status quo, or prop up the elite, can be entrusted to structure such incentives in the first place. There is no presumption here that late developers are doomed, but in development analysis, one must at times go beyond the framework of existing market mechanisms and design them. "Institutional change" is a slow process, and the complementarity between country-specific policies, institutions and economic development should not be overlooked.

The second difficulty associated with linking micro-markets and incentives to macro outcomes is the lack of a widely accepted aggregation mechanism within which this exercise can be carried out. In some countries artificial constraints impede the proper functioning of certain markets because these constraints have been put in place to funnel funds to the elite, protect monopoly profits, etc. At the same time, there may exist other parallel or informal markets that operate jubilantly beyond the reach of *any* regulation. What is the appropriate metric to

aggregate over these markets to reach any meaningful conclusion about markets and growth? There is, of course, the temptation to cherry pick, and over-explain economic growth by "properly functioning markets" or lack thereof.

Although case studies on micro-markets and incentives can be extremely informative at this juncture, a favorite sport of economists is challenging the representativeness of the particular case(s) for the broader questions at hand. Gordon (Chapter 6), for instance, takes that challenge seriously when he studies the productivity growth experience of the electricity generating industry. This industry experienced productivity regression during the 1970s and as such provides considerable insights into the potential causes of productivity slowdown. Gordon's bottom line is that even in an industry which produces a homogenous good by (mostly) single-product establishments, technology is very heterogenous and its progress, as embodied in new vintages of capital, is not linear. The positive message is that case studies can be insightful, but most economists will demand systematic data and a comprehensive framework.

A third issue surrounding incentives is their unintended consequences. Incentives provide focal points and solve coordination problems. Agents concentrate their efforts, attention and abilities on a particular reward (or penalty), which takes away energy from other pursuits, attracts talent and triggers "smart solutions." However, individually optimal solutions may include exploiting legal loopholes at the expense of others, excessive risk-taking, and even fraud. Financial liberalization is a case in point. Growth interacts with financial sector development (Levine, 1997), and while financial liberalization creates opportunities to improve allocative efficiency (Gurley and Shaw, 1955; McKinnon, 1973), it can simultaneously create perverse incentives including high-stakes gambles with other people's "money". When information asymmetries are prevalent, not all incentives automatically align with macroeconomic growth and stability. So, after numerous debacles, the prescription embraced by a vocal majority is gradualism in financial liberalization (Caprio *et al*., 2001), to allow individuals to learn about their environment, including about the ramifications of unintended consequences, and to avoid costly and irreversible mistakes.

Contributors to the Global Research Project do not envision "gangster" or "wild" capitalism as a role model for any of the developing regions. They emphasize institutions and policies because there seems to be a shared optimism that a regulated approach to markets can result in outcomes that match desired expectations. But, what exactly are these expectations? Unfortunately, the project is not very explicit about core principles and priorities. While this sort of debate is contentious and may be counterproductive in a collaborative research environment, those who participate in such an overarching project probably agree on some core principles about economic development and well-being, such as the degree of access to basic economic opportunities and resources as a meaningful measure of economic well-being. There should be a debate and perhaps even a consensus on what these basic opportunities and services explicitly are, if a platform, such as the Global Development Project, has aspirations for producing meaningful advice on policy change.

A more focused approach to economic development and growth might also serve as a useful organizing principle, with the added advantage of zeroing in on

the most important markets and institutions. Most economists would agree that education is not purely a consumption good and has elements of being "essential" for any sustained economic growth.[3] Put this way, the pressing question is not how the *market* for education operates, but rather it is where one is going to draw the line for "essential" education and, given specific objectives about the quality of education, how much reform is needed to improve the educational *sector* in individual countries. Access to healthcare is another essential sector in which development economists must ultimately take a position. Clearly, the particulars of these issues vary considerably across countries. For example, Cuba, with its high rankings in health and educational outcomes, is facing a different set of issues than Honduras, whose rankings in all these outcomes, as well as in standard measures of economic well-being, is not particularly inspiring. Although finding a common thread is difficult, to understand the key issues and measure future success, it is important to agree on core principles of development and growth.

Development economists have embraced markets and institutions to combat poverty and raise economic well-being through economic growth. While it is unlikely that this emphasis will diminish in the future, a number of issues are still outstanding. The role of demand in economic growth will continue to be very important for many less developed countries, and careful syntheses of structural change and neoclassical growth models are likely to uncover important mechanisms relevant for economic development. Education and R&D sectors are crucial drivers of long-run economic well-being, and both are influenced by policies, but we still know very little about how small open economies (most developing countries) should design optimal educational and R&D policies to bolster growth. Endogenous growth theorists have often taken the position that the global technological frontier is the only appropriate constraint for economic development and downplayed the significance of production of appropriate local technology in (small) developing economies. Structural economists, by contrast, have often shunned global economic integration by pointing to inappropriate technologies that are being exported to developing countries. Structural change models linked to endogenous technological progress should be a high priority. Considerable progress on the agricultural sector has already been made (e.g. Ruttan, 2001), and extensions are likely to produce interesting insights (e.g. about competition policy). As well, most developing countries have not completed their demographic transitions yet (Lee, 2003), and the interaction between demographic forces and economic growth has dramatic implications for national social security systems and for asset valuations worldwide. The challenges facing such a synthesis are numerous, but the rewards, at the margin are, staggering.

Talan B. İşcan
Dalhousie University (Talan.Iscan@Dal.Ca)

[3]Formal education, as measured by average years of schooling or primary- or secondary-school enrollment rate, is a *poor* indicator of future economic growth in cross-national regressions. However, indicators of labor force quality which are influenced by schooling are better predictors of future economic growth (e.g. Hanushek and Kimko, 2000), although there is uncertainty concerning whether the contribution corresponds to transitory or permanent growth effects.

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