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# Theory and Practice in Development Economics

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*"We have no general theory of development, and we need none"*

Benjamin Higgins[1]

In 1949, the World Bank sent a mission, headed by Lauchlin Currie, former advisor to Marriner Eccles at the Fed and personal economic advisor to Franklin D. Roosevelt, to Colombia to study the economic and social conditions of the country. This ambitious mission was meant to "form [a] pattern" for what would arguably become one of the most important aspects of the Bank's activity in the development field: the assessment of economic and social conditions in less developed countries worldwide.[2] However, Currie recalled, "there were no precedents for a mission of this sort and indeed nothing called development economics. I just assumed that it was a case of applying various branches of economics to the problems of a specific country, and accordingly I recruited a group of specialists in public finance, foreign exchange, transport, agriculture and so on" (Currie 1967, pp. 30-31).[3]

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[1] Higgins 1992, p. 247

[2] "Outgoing Wire", John J. McCloy to Professor Lionel Robbins, March 1, 1949; as quoted in Alacevich 2009, p. 13.

[3] Another pioneer of development economics, Benjamin Higgins, also remarked: "The status, or even the expression 'development economist', did not exist until after

Currie (theoretically, in a nutshell, an *ante-litteram* Keynesian) posed the question of the relationship between practice and theory as completely unproblematic. He started from the belief that fostering development in poor countries involved applying the economics of developed countries to less developed ones. And yet, within a few years a new disciplinary field called "development economics" actually emerged, as economists had to shape their own theories in order to address the practical problems they were facing in less developed countries: theory arose out of practice. The field was applied in the sense that it was dominated by practice, but this involved learning in the field and developing new analyses and concepts out of this experience, not just taking ideas from other fields and applying them in a different context. Thus, whereas Currie initially thought that to work in the development field was sufficient to pull from the existing toolkit of the economist, a few years later he and his colleagues were in fact committed to designing and refining a new toolkit.[4] During the 1950s the discipline prospered but, by the 1960s, it faced a crisis, for it came to be recognized that the experience of developing

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World War II. I never took a course in economic development in my life" (Higgins 1992, p. 28).

[4] Currie's comment on the application, in the late 1940s, of the standard economics toolkit to development problems finds an interesting parallel on the environmental economics front: rather than attempting to develop an entirely new toolkit that took account of environmental complexities, environmental economists drew on concepts developed elsewhere in the economics discipline, such as externality theory and the literature on market imperfections. I am grateful to one anonymous reviewer for pointing this out to me.

countries did not fit the models of development that had been constructed, and many of the main policy prescriptions had clearly failed. The result was a complete reorientation of the discipline, in which "neoclassical economics", hitherto believed irrelevant to developing countries, became central. The field started to apply, systematically, tools such as cost-benefit analysis and input-output analysis; it also became much more formalized than it was before. The result of this transformation—as shown by Higgins's opening quote—was a much more conventional application of theory to the problem of development.

Higgins's quote, however, shows only one terminus of the oscillating nature of development economics. Some will easily agree with it; others will find it basically wrong. The tension between theory and practice, in any case, has never gone away. This has become particularly evident at the World Bank, which by the end of the century had developed an enormous research department, becoming a major player in the discipline. Though undertaking academic research, the Bank faces the challenge of engaging with practice. Not surprisingly, there has been no return to the "old" style of development economics, but recent discussions within the Bank indicate a perception that research would benefit from less theoretical rigor and academic style, in favour of stronger adherence to the idiosyncratic reality of less developed countries, and acceptance of perhaps disturbing but nonetheless actual lacunae in data availability. As this article will show, there is an intrinsically unstable balance between theoretical and applied work in development economics.

## Theory and practice in early development economics

Whereas, according to Currie, it hardly existed in 1949, only a few years later, the new discipline of development economics was thriving.[5] Drawing on various sources, including the "developmentalist" efforts of the United States and of a number of less developed countries in the interwar period, the Federal Reserve missions to less developed countries between 1943 and 1950, and British plans for the postwar development of Eastern and South-Eastern Europe during World War II, development economics was soon dominated by the political context of the Cold War. Development policy and foreign aid were transformed into a fundamental tool of international power relations.[6] Walt W. Rostow's *Stages of Economic Growth*, made this link explicit: "We must demonstrate", he wrote, "that the underdeveloped nations – now the main focus of Communist hopes – can move successfully . . . into a well-established take-off within the orbit of the democratic world, resisting the blandishments and temptations of Communism." "This is", Rostow

[5] Early development economics is discussed more at length in Alacevich 2009; 2011.

[6] On the Fed missions of the 1940s, see Helleiner 2003; 2014; Alacevich and Asso 2009. On British plans for Eastern and South-Eastern Europe, see Rosenstein-Rodan 1943; Mandelbaum 1945; Alacevich 2013a; 2013b. For an influential analysis and critique of the "high modernist" State, see Scott 1998; on the US, Stalinist, and Latin American versions of the developmentalist state, see Ekbladh 2010; Kotkin 1995; and Sikkink 1991. On development and Empire, see Cooper 1997; Hodge 2007; on nutrition and health standards, see Cullather 2010. For development in the Cold War scenario, in addition to the volumes mentioned above, see Latham 2011; Westad 2005; Engerman et al. (2003); Gilman 2003; and Immerwahr 2015.

underscored, "the most important single item on the Western agenda" (Rostow [1960] 1990, p. 134). Similarly, Gunnar Myrdal noted "The fate of the underdeveloped countries [has become] a matter of foreign policy concern in the developed countries . . . and we economists are riding the crest of the wave" (Myrdal 1968a, p. 8).

There developed a large literature on planning, in Europe as well as elsewhere. According to James Meade, that was "the great economic issue which now confront[s] us: to plan or not to plan" (Meade 1949, p. v). In less developed countries, planning appeared necessary to steer the economy away from so-called vicious circles of poverty and onto a path of self-sustained growth. Planning seemed to be most effective in situations characterized by a scarcity of one or more particular factors of production or entrepreneurial resources, guaranteeing that waste would be minimized. Planning had also a particular appeal in the political arena: it offered ambitious goals to ambitious elites, and motivated the larger polity to participate in a common effort.[7]

Not unexpectedly, planning had its critics, and yet, even in the middle of heated debates, it was quite clear to all that the ultimate test was in the actual development performance. The debate could not remain theoretical but was tied to practice. Even inveterate planners like Paul Rosenstein-Rodan, W. Arthur Lewis, and Gunnar Myrdal recognized the unpredictable nature of processes of economic development, and the intrinsic limits of planning. As Lewis put it, "It is probably no easier or more difficult to plan an artistic

[7] For an early panoramic view of planning exercises focused on both developed and less developed countries, see Harris 1949; for a later assessment, Hagen 1963.

renaissance than to plan economic development" (Lewis 1966, p. 23), and Myrdal discussed the difference between the "ideal" or "optimal plan" and "the plan as a practical art", that is, as a tool of political mediation in the policy-making process (Myrdal 1968b, pp. 1878-1896). On the other side of the debate, critics of planning such as Albert Hirschman recognized the effectiveness of comprehensive plans as devices to introduce, in political negotiations issues that might otherwise be marginalized and neglected.[8]

Beneath this debate was a widespread consensus that development was a *process* of change, and that what counted, in the end, was how to initiate and foster it. Myrdal, for instance, insisted that it is "*part of the function of the plan to assist in striking compromises and bargains between conflicting interests*" (Myrdal 1968b, p. 1891, emphasis in the original). Hirschman described this as the art of "reformmongering" (Hirschman 1963). John Kenneth Galbraith expressed no unease in bracketing together the apparently irreconcilable views of planners like Myrdal and anti-planners like Hirschman, of teleological modernizers like Rostow and students of the many different paths to economic growth like Alexander Gerschenkron, when he praised planning and claimed that "we must recognize that economic development is a process" (Galbraith 1962, p. 12).

In sum, the practical dimension of development economics was paramount from the very beginning. In fact, as we will see in the remainder of this section, this practical dimension was a distinctive feature of the discipline from the late-1940s throughout the 1960s. Its goal was to help so-called backward

[8] See Hirschman 1963, pp. 231-232.

areas undergo a process of development within the liberal international order. As Hollis Chenery put it, "the field of development economics emerged as a by-product of America's aid programs and, more generally, as a means of coming to terms with the post-war economy" (Chenery 1992, p. 379). Development economists shaped their professional identity more as economic advisors to the governments of less developed countries than as academic economists, and development economics became a quintessentially peripatetic economics, different from orthodox economics. As Chenery put it, "the considerable mobility and exposure to country experience among practitioners in this new field was one of the factors responsible for divergences of development economics from mainstream economic theory" (Chenery 1992, p. 379).

In order to make sense of their experiences and practical observations, development economists developed their own theories. W. Arthur Lewis's discussion of unlimited supply of labor (a classic Marxian concept) in connection to the dualistic development of both capitalistic and subsistence sectors (perhaps geographically intertwined but nonetheless separated by altogether different dynamics) exemplifies this, as we see from Rosenstein-Rodan's and Scitovsky's discussion of pecuniary and technological external economies, Rosenstein-Rodan's insistence on indivisibilities, Hirschman's backward and forward linkages, and Myrdal's discussion of circular and cumulative causation (Rosenstein-Rodan 1943; Lewis 1954; Scitovsky 1954; Myrdal 1957; Hirschman 1958). However, though some development theories drew on mainstream ideas, development economics consciously distinguished and differentiated itself from mainstream economics.

It did not take much for this new disciplinary field to percolate through academia. The intellectual case for this separation was clearly made in a Harvard memo of the mid-1950s:

> Virtually all of our . . . course work is concerned with the sophisticated economic society in which markets and factor markets reflect modern forms of organization. . . . The undeveloped countries do not have such institutions or they are partial or primitive. As a result, problems take on a distinctive form. . . . It is also unlikely that in these economies one can do much with formal economic models. Anthropological and political issues bear heavily on virtually every question. The role of government is different and almost invariably more pervasive. These are broad and difficult questions of public v. private entrepreneurship which are quite different from those of the developed economies.

Galbraith, its author, concluded: "it seems fairly clear that there is an important and separate field of study here."[9]

A number of reasons may explain the early focus of development economists, despite their involvement with practice, on the theoretical foundations of the

[9] John K. Galbraith, "Economic Development as a Proposed Field", 1955/1956, Box 525, 8/53/E/3/8, Series 5. Harvard University File, 1949–90, John Kenneth Galbraith Personal Papers, John F. Kennedy Memorial Library, Boston University. See also Alacevich 2016, p. 17.

new discipline. One is that in the early postwar years empirical data on the social and economic conditions of less developed countries were simply missing. Development missions had thus to start from scratch to put together even basic series on national income and international payments. The temptation to think in general terms, or to draw lessons from the few available case studies and then apply them elsewhere was thus strong. Not by chance, the late 1940s and the 1950s witnessed a proliferation of "laboratories" of development policies around the world, in the hope that the experience of a specific case could be generalized and replicated in other backward areas. Eastern Europe (1944), Colombia (1949), Italy (1951), India (1954), Indonesia (1954), post-World War II Japan, were all labeled "laboratories" of development policies by prominent actors in the field.

Also, the focus on general, all encompassing, theories was probably due to the very young age of development economics as a distinct and independent disciplinary field. Studies in the sociology of science have shown that newly born disciplinary fields are often shaken by the competing claims of scholars who struggle to occupy the center of the discipline, and thus define it according to their own theoretical and methodological propositions. The appearance of comprehensive but mutually exclusive theories of development during the 1940s and the 1950s responded to this logic. The "balanced-" vs. "unbalanced-growth" diatribe that occupied the first fifteen years of life of the discipline, for instance, can be interpreted as a struggle for the soul of development economics.[10]

[10] See Alacevich 2011.

## The crisis of old development economics

Rooted in practice, development economics involved a blend of theoretical and empirical analyses but this versatility turned into a curse. Excessively theoretical for policy-oriented organizations and excessively empirical and incapable of producing elegant models for the academic world, development economics was increasingly marginalized by the larger field of economics – with paradoxical results. When the World Bank, in the early 1950s, abandoned its focus on reconstruction and embraced the development mission, it also closed its Economic Department and dismissed the development economists working in it. "The Bank was not the place for the development of broad economic policies or studies" – declared the Bank's vice-president – "Our job was applied economics".[11] As a consequence, the work of World Bank loan officers and engineers increasingly focused on specific projects and, at the country level, on the "creditworthiness" and "absorptive capacity" of the borrowing country. These two concepts, however, were addressed only to the extent that they offered elements to understand whether the client country would be able to repay in full and without delays its debt to the Bank.[12]

At the same time, however, mainstream economics was becoming increasingly formalized in a way that development economics was not (Morgan and

[11] Oral history interview, Robert L. Garner, July 19, 1961, p. 98.

[12] For further discussion on this issue, see Alacevich 2016.

Rutherford 1998; Weintraub 2002). Development economics, Krugman has claimed, "was virtually buried, essentially because [its] founders . . . failed to make their points with sufficient analytical clarity to communicate their essence to other economists, and perhaps to themselves" (Krugman 1992, p. 16). But perhaps, unlike in Krugman's explanation, the matter was not actually one of analytical skills. The increasingly visible divergence between development economics and mainstream economics, as well as a somewhat unresolved tension, within development economics itself, between a theoretical and an empirical dimension, may be interpreted as a specific instance (and ascribed to the very tension) of the broader transition from inter-war pluralism to post-war neoclassicism. It is worth noting, in this regard, that virtually all pioneers of development economics were trained before World War II.[13]

This double inadequacy of development economics – too theoretical for the practitioners, too applied for the theoreticians – did not go unnoticed to the pioneers of the discipline. In a 1981 exchange with Joseph J. Love, for example, Paul Rosenstein-Rodan tried, somewhat awkwardly, to downplay the applied nature of early development economics. He depicted the British study of Eastern-European backwardness and development plans, to which he participated in the early 1940s, as a purely theoretical endeavor. Eastern Europe, in his recollection, was simply a laboratory for the elaboration of development theories meant to have universal validity. "We selected Eastern European countries", he wrote to Love, "only as an example for a model of the

[13] I am grateful to one anonymous reviewer for pointing out this important parallel.

Third World . . . not because of any interest in Eastern Europe or Germany but only because representatives of the Eastern European governments in exile were in London, and one could use them".[14] Archival evidence, however, tells a completely different story, and in fact it is abundantly evident that the British study was a direct answer to eastbound Nazi European imperialism. Rosenstein-Rodan's argument, thus, sounds like an ex-post attempt to qualify the birth of development economics as a theoretical exercise.[15] One cannot help but suspect that Rosenstein-Rodan was reacting to the low theoretical status that his discipline suffered in the academic world by downplaying the applied nature of his earlier research.

According to Hirschman (1981), the decline of development economics was due to the joint attack that the discipline suffered both from neoclassical and neo-Marxist economists (the latter were mostly located outside of the United States.)[16] Neo-Marxists criticized the reformist approach of development

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[14] Paul N. Rosenstein-Rodan to Professor Joseph L. Love, May 13, 1981. I am grateful to Joseph Love who shared this document with me.

[15] On this, see Alacevich 2013a.

[16] According to Robert W. McChesney, the only tenured Marxist economist in the US between the 1950s and the 1960s was Paul A. Baran at Stanford University (McChesney 2007 [1984]). Stanford's top administration and a number of Stanford alumni, however, highly resented Baran's critique of the US policy towards Cuba, Vietnam and the Third World. Confidential Stanford administrative documents leaked to the *Stanford Daily* in 1971, seven years after Baran's death, show that Baran had been the target of continuous harassment and unequal treatment. In addition to a general climate of hostility that made his situation very stressful (in

economists, accusing them of suggesting strategies that in fact reproduced the exploitation of less developed countries by advanced industrial ones (for example, Frank 1967; 1969). In contrast, neoclassical economists claimed that the policy of import-substitution industrialization (ISI), supported by the majority of development economists, had devastating side-effects, misallocating resources and thus making the economic systems of less developed countries increasingly inefficient and unable to compete in the international markets (Lal 1983).[17] Many "comprehensive" development plans had produced disappointing results. A crisis was recognized not only by long-standing critics of planning, such as Peter Bauer (1972, pp. 69-94) and Ian Little (1982), but also by former supporters of planning (see, among the many contributions to the debate about the crisis of Indian planning, Lewis 1962 and Streeten and Lipton 1969).

Hirschman suggested that the crisis of development economics should be understood also from a broader, social and political perspective. Modernization theory, in the 1950s, had conveyed the idea that economic, social, and political modernization would proceed hand in hand. This vision influenced, at various degrees, also social scientists who, in other respects,

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the early 1960s, he suffered two heart attacks, of which the second one was fatal) Baran noticed that he had "a teaching load that is about twice as large as that of my 'peers,' with pay of about 60 percent of theirs," as reported in Lifschultz 1974, p. 56.

[17] Not all development economists agreed with ISI policies, the most distinguished case being perhaps that of Jacob Viner (1952). Viner's position, however, remained a minority position, within the development field, until the early 1970s.

were quite critical of orthodox modernization theory. In the 1960s, however, this vision was deeply shattered by the historical record of many less developed countries and, of course, by the Vietnam War, whose escalation was supported by many prominent modernization theorists, such as Walt W. Rostow. As Hirschman wrote: "The series of political disasters that struck a number of third world countries from the 1960s on . . . were clearly somehow connected with the stresses and strains accompanying development and 'modernization'. These development disasters, ranging from civil wars to the establishment of murderous authoritarian regimes, could not but give pause to a group of social scientists" (Hirschman 1981, p. 20).[18]

Despite this feeling of crisis, however, development economics was entering a new phase, less grandiose in terms of intellectual diatribes or vast planning efforts, but no less productive in terms of development knowledge. The journals *World Development* and the *Journal of Development Economics* were founded in 1973 and 1974 respectively. The World Bank launched its *World Development Report*, in 1978, with the ambitious goal of providing annually "a comprehensive assessment of the global development issues" (World Bank 1978, p. iii). Irma Adelman (1974, p. 3) described the field as "in a state of ferment." She questioned the causal link between industrial economic growth and raising living standards of the LDC's poor, arguing that tensions between pre-existing power structures and the forces of modernization had often increased the level of domestic violence and political repression, causing a

[18] See also Tomás Rangil 2010, who discusses how the economics profession played a role in the demise of modernization theory.

more unequal distribution of political power as well as economic welfare. "How is it," she asked, "that processes that lead to the further immiseration of the poor could take place virtually unnoticed by the intellectual leaders of the mainstream of the field of economic development? Why the long lag in realization of these facts, and the even longer time lag in the willingness to concede that a fundamental reorientation of the discipline is required?" (Adelman 1974, p. 3).

## Novel ways to do applied (development) economics

Adelman was in fact summarizing a sense of unease that had characterized the development field for almost a decade. During this period, this increasingly evident sense of crisis prompted a "reorientation" of the field, which involved a reconfiguration of the relationship between theory and application. Hirschman was among the first to address the question of project appraisal and evaluation in the development field, for it was increasingly evident that there was no effective assessment of what had and had not worked.[19] After publishing *The Strategy of Economic Development* in 1958, his most theoretical book on development and one of the classics of early development theory, in the 1960s Hirschman turned his attention to the detailed study of specific processes of policy-making and project management (Hirschman 1963; 1967

[19] For a discussion of this turning point and the question of project evaluation in development economics, see Alacevich 2014; 2017 forthcoming; on which the following paragraph relies.

[2015]; see also Tendler 1965; 1968). Not surprisingly, Hirschman analyzed several projects financed by the World Bank, "whose twenty-year experience in . . . development projects constitutes the most ample, varied, and detailed source of information and documentation in this area" (Hirschman 1967 [2015], p. 1). The Bank's economists were delighted. Dragoslav Avramovic, soon to be appointed to the new World Bank position of Director of Special Economic Studies, argued that "probably for the first time, the contemporary theory and practice of project appraisal in infrastructure will be subjected to a systematic ex-post methodological scrutiny".[20] However, the Hirschman's perspective and the Bank's expectations soon diverged, due to different visions of the relationship between theory and practice in project appraisal and evaluation. Hirschman's idea of development knowledge implied the intrinsic presence of "the element of the unknown, the uncertain and the unexpected, which deflects projects from the originally chartered course".[21] "Upon inspection," he claimed, "each project turns out to represent a *unique constellation* of experiences and consequences, of direct and indirect effects. This uniqueness in turn results from the varied interplay between the structural characteristics of projects, on the one hand, and the social and political environment, on the other" (Hirschman 1967 [2015], p. 172, emphasis in the original). Against this, Bank economists thought him naïve and unable to offer any operational tool in order to embed project evaluation

[20] Dragoslav Avramovic to Department Heads, IBRD, February 18, 1964, as reported in Alacevich 2014, p. 146.

[21] Albert O. Hirschman, "A Study of Selected World Bank Projects—Some Interim Observations," August 1965, as reported in Alacevich 2014, p. 150.

in the organization's routines. Instead, they sought more rational and technical methods of planning. Two key documents were the *Manual of Industrial Project Analysis in Developing Countries* (1968; 1974), written by Ian M. D. Little and James A. Mirrlees for the Development Centre of the Organisation for Economic Co-operation and Development (OECD), and the *Guidelines for Project Evaluation* that Partha Dasgupta, Stephen Marglin, and Amartya Sen published in 1972 for the United Nations Industrial Development Organization (UNIDO) (Dasgupta, Marglin, and Sen 1972). Such methods were introduced into the Bank by Robert S. McNamara, who created a Programming and Budgeting Department at the Bank. From 1970, this incubated an Operations Evaluation Unit that became an independent Department in 1973. Two years later, the World Bank published its own manual of project evaluation, *Economic Analysis of Projects* (Squire and Van der Tak 1975).

Although these volumes proposed different measurement techniques, they all shared the same approach to the problem of project appraisal, that is, the use of "social" (as opposed to "private" or "commercial") cost-benefit analysis measures, and the use of "shadow-prices" for the many cases of social variables whose market price was either unreliable or simply non-existent. Shadow prices, that is, would make it possible to measure the social value of benefits and costs, as opposed to their private profitability. The underlying assumption, in Dasgupta's words, was that "due to various 'imperfections' of the market the prevailing prices of the goods and resources that are involved

in an industrial project do not necessarily reflect their *real worth* to the economy as a whole" (Dasgupta 1972, p. 33, emphasis in the original).[22]

With this development, cost-benefit analysis, originally an engineering tool for water resources management, became central to development economics. In a decade, social cost-benefit analysis was used sufficiently widely that it could even be considered "a school" that directly influenced "the thinking of economists engaged in planning in developing countries" (Kornai 1979, p. 76). Project appraisal came to be tied even more closely to the wider economics discipline through becoming linked to the emerging field of the economics of imperfect information. For example, Dasgupta collaborated with Joseph Stiglitz, a key figure in the economics of information, to develop "a fully articulated model for arriving at the *detailed* recommendations" that governments "ought to follow in choosing industrial projects in the public sector" (Dasgupta and Stiglitz 1974, p. 2, emphasis in the original; see also Stiglitz and Dasgupta 1971). Unlike the work of early development economists such as Hirschman, this literature was highly formalized.

There was also a visible reorientation in the development policies of the major organizations offering loans and technical assistance. Persistent poverty in many less developed countries prompted the World Bank to bring the war on poverty to the top of its agenda. As McNamara put it, "Of course it would be comforting to continue to believe that there is no conflict between rapid overall growth and comparable improvement in the incomes of the poor. But,

[22] For a discussion of the OECD and UNIDO volumes, see also Little and Mirrlees 1973, and Stewart and Streeten 1972.

unfortunately, in the real world in which we live, the evidence suggests that there is" (McNamara 1972, p. 219). *Redistribution with Growth* (Chenery *et al*. 1974), a volume produced jointly the Bank's Development Research Center and the Institute of Development Studies of Sussex University, may have lacked any specific analysis about how to empower a political coalition supportive of redistributive policies (Ahluwalia 1974; Bell 1974), but it nonetheless signaled a major reorientation in development institutions.

The International Labour Organization (ILO), also, was conducting a huge research on worsening living standards in less developed countries. The basic needs approach, developed in the mid-1970s by the ILO and soon adopted by a group of researchers working for the World Bank, was another attempt to redirect the focus away from grand theories of long-term development, exclusively centered on aggregate economic growth, to the much more specific question of how to fulfill basic needs in order for individuals living in poor countries to live a decent life. This approach insisted not only on basic physiological needs such as nutrition, access to potable water, health, housing and clothing, but also access to education, and the exercise of civil and political rights (ILO 1976; 1977; Streeten 1981; Stewart 1985). The basic needs approach can be considered in many ways a progenitor of the today very fashionable capabilities approach.

One major element of the 1970s reorientation related to international trade policies. An important milestone in this discussion was the publication, in 1970, of the OECD-sponsored study by Ian Little, Tibor Scitovsky, and Maurice Scott on industry and trade policies in seven less developed countries (Argentina, Brazil, Mexico, India, Pakistan, the Philippines, and Taiwan). Based

on detailed empirical analysis, the study claimed that import-substitution policies had become harmful. It called instead for a quick endorsement of free trade policies, increasing exports, and more labor intensive policies: "promoting" industry as opposed to "protecting" it (Little, Scitovsky, and Scott 1970, pp. 14-15). The following year *The Structure of Protection in Developing Countries* (Balassa and Associates 1971), the outcome of a project organized by the World Bank in collaboration with the Inter-American Development Bank, analysed problems with protection.[23] In 1970 the National Bureau of Economic Research (NBER) inaugurated a project on Foreign Trade Regimes and Economic Development with a strong comparative component. Directed by Jagdish Bhagwati and Anne O. Krueger, this produced case studies on Turkey, Ghana, Israel, Egypt, the Philippines, India, South Korea, Chile, Colombia, Brazil and Pakistan, and two comparative syntheses, on liberalization policies (Krueger 1978) and exchange controls (Bhagwati 1978). This concluded that less developed countries had more to gain from export-promoting than import-substitution strategies, and was followed up by another NBER project on Alternative Trade Strategies and Employment, under Professor Krueger's direction. Finally, the University of Kiel's Institut für Weltwirtschaft (Institute for the World Economy) also became a relevant center for research on export-led development strategies.[24]

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[23] For preparatory material, see Balassa 1970; for examples of follow up studies, see Balassa 1971a; 1971b; 1971c; 1975; 1977.

[24] See, for example, Donges 1976 and Donges, Stecher, and Wolter 1974. See also Donges 1971 for work previous to Donges's affiliation to the Kiel Institute.

The rejection of protectionist policies for the long-term development of less developed countries was another sign of the penetration of neoclassical perspectives into the core of development discourse, and research priorities changed accordingly. As Robert Timmer wittily put it, "Getting prices right is not the end of economic development, but getting prices wrong frequently is" (Timmer 1973, p. 76). This reorientation eventually marked a paradigm shift for the entire field. As Kathryn Sikkink has shown, even the Economic Commission for Latin America (ECLA), one of the main beacons of center-periphery analysis and import-substitution policies in the 1950s and 1960s, was by the 1990s stressing export promotion, international competitiveness and openness, against import-substitution and the protection of domestic markets (Sikkink 1997, p. 233).

Even a cursory look at textbooks in development economics from the 1980s on clearly reveals the trend towards using neoclassical methods. For example, *Economics of Development* (Gillis, Perkins, Roemer and Snodgrass 1983, p. xv, xvi) stated that "this text makes extensive use of theoretical tools of classical and neoclassical economics in the belief that these tools contribute substantially to our understanding of development problems and their solution." Similarly, IMF economists Pierre-Richard Agénor and Peter J. Montiel (1996, pp. 3-4) introduced their *Development Macroeconomics* textbook by claiming that they were adapting "standard macroeconomic analysis" to the conditions of developing countries. Rudiger Dornbusch used the Foreword to this textbook to state that, different though less developed countries are from industrialized ones, "in some deep ways the problems are about the same everywhere" (Dornbusch 1996, p. xxi). As is evident, this

perspective is the opposite of Galbraith's claim, cited earlier, that conditions in less developed countries were fundamentally different. Dornbusch's programmatic intent was in fact openly polemical towards that kind of claim: "The battle cry 'this country is different' is ultimately a scoundrel's plea for protecting outdated interpretations or politicized policy advice from intellectual import competition" (Dornbusch 1996, p. xxi). If Dornbusch concluded that "the link between political economy and structure is the first step in engineering successful reform" (Dornbusch 1996, p. xxiii), the way he interpreted this link lay entirely within a neoclassical framework.[25] This perspective is the basis for the title of Dani Rodrik's *One Economics, Many Recipes*, a book "strictly grounded in neoclassical economic analysis" (Rodrik 2007, p. 3).

## The spread of formal techniques

Modern development economics exists as a diffused field of interest, tapping from an eclectic array of ideas within the mainstream canon. However, despite its neoclassical structure, it has preserved something from its old incarnation that may enrich the larger field of economics, especially the importance of institutions. In this respect, the trajectory of development economics from its

[25] Not by chance, he mentions as exemplary policy makers Anne O. Krueger and Larry Summers (World Bank Chief Economists in 1982-86 and 1991-93, respectively) and Michael Mussa, a member of the Council of Economic Advisers under Ronald Reagan and Chief Economist at the IMF throughout the 1990s (Dornsbusch 1996, p. xxi).

old to its new incarnation resembles the trajectory from the old to the new institutional economics.[26] While old institutionalists mostly rejected orthodox neoclassical economics as "overly formal, abstract, and narrow" (Rutherford 1994, p. 9), new institutionalists have often accepted the neoclassical framework, yet they have enriched it by incorporating in a central position the role and behavior of institutions. Coase, for example, argued that the distinctive characteristic of new institutional economics is to use "standard economic theory to analyze the working of […] institutions and to discover the part they play in the operation of the economy" (Coase 1984, p. 230). Similarly, Douglass North has argued that the theoretical framework of modern institutional economics must "be capable of integrating neo-classical theory with an analysis of the way institutions modify the choice set available to human beings" (North 1986, p. 230).

Not by chance, when a few years ago the World Bank made an attempt to acquire a more solid grasp of how institutions work in less developed countries, and how this may affect Bank's policies, it turned to North and his collaborators.[27] In the late 2000s, the World Bank organized a number of workshops based on North's neo-institutionalist approach, and World Bank economists were asked to develop in-depth neo-institutionalist historical narratives of country case-studies.

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[26] For an introduction to the old and new tradition of economic institutionalism, see Rutherford 1994.

[27] See North, Wallis, Webb, and Weingast 2007, and the subsequent volume North, Wallis, and Weingast 2009.

In a similar fashion, as we saw, development economics initially shaped its identity in opposition to the mainstream framework. Whilst it has fallen back into the orbit of neoclassical economics, however, today's development economics also retains that blend of theory and empiricism that was one of its prominent features when it emerged as a disciplinary field seventy years ago, as well as the empirical thrust that was a typical characteristic of old institutional economics.

Analytical narratives on economic growth, to borrow the subtitle of another book by Dani Rodrik, have also become important elements of the research landscape in the new subfield of development economics. Rodrik highlights the key role of institutions and the two-way interaction between institutions and trade, and institutions and strictly endogenous factors such as a country's resource endowments and productivity levels. According to Rodrik, however, country narratives show that institutional reform has often been overrated as a necessary prerequisite for the onset of economic growth. Analytical narratives thereby help debunk some of the myths about institutional reforms that are slowest to die, according to which "their complementary nature requires a long list of such reforms to be pursued simultaneously", de facto making progress virtually impossible (Rodrik 2003, p. 15).

Institutions are a crucial element also of Lance Taylor's analysis. His work, moreover, offers a good opportunity to observe the increasing spread of formal techniques. Introducing his *Structuralist Macroeconomics* in 1983, Lance Taylor recognized "the strong tendency in orthodox economics to make a fetish of techniques". Though taking an unorthodox stance, Taylor did not reject formal analysis, claiming that his book "responds to fashion",

purporting "to show that structuralist thought can be made rigorous" (Taylor 1983, p. 4). "An economy", Taylor claimed, "has structure if its institutions and the behavior of its members make some patterns of resource allocation and evolution substantially more likely than others. Economic analysis is structuralist when it takes these factors as the foundation stones for its theories. […]. Institutions are conspicuously lacking in [the neoclassical] calculus, as is recognition that men, women, and children are political and social as well as economic animals" (Taylor 1983, p. 3).

But structuralist macroeconomics has not been the only branch of development economics where the application of formal models has taken place. Theories of imperfect information, mentioned above, and the more recent trends in inequality studies based on detailed household surveys and enterprise-level data are two additional and notable examples.[28] Data availability also improved dramatically in the postwar years, and this, combined with the statistical and computing power developed mainly by governmental and multilateral agencies, made the advancement of econometric methods possible also in development economics (see Backhouse and Cherrier on computing, this volume). Hollis Chenery had a pivotal role, after an academic career at Stanford and Harvard and a spell at the United Nations Agency for International Development, as the Chief

[28] For an analysis of externalities in economies with imperfect information and incomplete markets, see Greenwald and Stiglitz 1986; see also Stiglitz 1988. For recent literature in inequality studies, see Bourguignon 2015; Galbraith 2012; and Milanovic 2005; 2016.

Economist of the World Bank in the crucial decade 1972-1982. After a stint in engineering, Chenery studied graduate economics at Harvard with Wassily Leontief and became a member of his research group on input-output analysis (see Halsmayer and Ballard-Johnson, this volume). He used input-output methods to conduct what he called "interindustry analysis", focused on the structural intersectoral changes that accompany economic growth, on which neither aggregate national income analysis nor partial equilibrium analysis could shed any light (see Chenery and Clark 1959). Chenery also undertook comparative econometric studies in order to identify uniform patterns of structural economic change in less developed countries (e.g. Chenery and Taylor 1968; Chenery, Elkington, and Sims 1970; Chenery 1971; Chenery and Syrquin 1975; and Chenery 1979).

Chenery's expertise in quantitative general equilibrium analysis was one of the reasons why McNamara hired him. During his tenure at the World Bank, Chenery put a strong emphasis on "narrowing the gap between theoretical analysis and its practical applications in developing countries", as he wrote, applying input-output analysis and linear programming to economic planning, taking advantage of the widespread acceptance of shadow pricing for the appraisal of investment projects, and making use of digital computers for the elaboration and calculation of large, previously impracticable multisectoral models (Chenery 1975, pp. xi-xiii; Blitzer, Clark, and Taylor 1975). Since the late 1950s and early 1960s, especially, the use of increasingly refined quantitative models and input-output analysis, concomitant to the above-mentioned (and initially very slow) increases in data availability and

computing ability, promised to put economic planning in less developed countries on more solid bases than before.

Early examples from those years are Irma Adelman's work in South Korea for USAID when Chenery was director of USAID's research division, and the work of the MIT Center for International Studies (Cenis) in support of India's Five-Year plans under Paul Rosenstein-Rodan's directorship.[29] Under Chenery's scientific leadership, in the early 1970s the World Bank conducted a series of studies that provided economy-wide computable general equilibrium models on a number of countries (Ballard and Johnson, this volume, discuss other developments in CGE). Because of the new awareness of the perverse distributional effects of economic growth mentioned above, many of these models included a distributional dimension, and made use of social accounting matrices (see, for example, Adelman, Taft Morris, and Robinson 1976; Adelman and Robinson 1978; Taylor et al. 1980).[30]

It is important to note that, even though development economists increasingly applied theoretical ideas from outside the field, ideas have also flowed in the

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[29] See Adelman's reconstruction of those years in Adelman, Zilberman, and Kim 2014, pp. 7-11. An example of Cenis' contribution to India's Five-Year plans are in Rosenstein-Rodan 1964a; 1964b.

[30] Unlike input-output tables, social accounting matrices include different classes of individuals, making it possible to study distributive dynamics. Recently Adelman was asked "What do you get with a social accounting matrix that you can't get from an input-output model?" to which she replied: "Distribution among classes" (Adelman, Zilberman, and Kim 2014, p. 11).

other direction: the working of economies characterized by asymmetric information and imperfect and incomplete markets, increasing returns to scale, multiple equilibria, self-reinforcing mechanisms of cumulative causation, coordination failures, path-dependence mechanisms and the persistence of dysfunctional institutions, and efficiency-wage theory are all ideas to which development economists contributed (Bardhan 1993, pp. 129-130). As Nobel laureate W. Arthur Lewis put it, "Dual labor markets are standard in the Labor Economics Department; the International Finance Department handles the effects of disequilibrium in small, open and poor economies; and the Industry Department is studying multinational corporations and appropriate technology" (Lewis 1984, p. 1). More recently, methodological exchanges have taken place between medical and economic testing techniques, such as the application of randomized controlled trial methods to the analysis of development policies in less developed countries (for a particularly successful example, see Banerjee and Duflo 2011; Panhans and Singleton, this volume, discuss RCT in other contexts).

Randomized control trials have been criticized on many grounds, not least for the narrowness of the questions they focus on and their frequent misdiagnoses, despite the claim of being able to assess the "true" impact of specific causal variables, net of noise or other confounding variables. As Sanjay Reddy put it, a development economics based on randomized control trials "leads not so much to increasing rigour as to rigor mortis, by severely limiting the questions that can be asked and shoring up a practical philosophy that is quiescent in relation to many important questions . . . related to the structure and dynamics of markets, governmental institutions,

macroeconomic policies, the workings of social classes, castes, and networks, and so forth" (Reddy 2012, p. 63).

Reddy also notes that "cheap computing power has enabled the proliferation of data and of econometric analyses that present often contradictory conclusions" (Reddy 2012, p. 62). There is more than a grain of truth in this, and one can only hope that the return of development economics back into the economics mainstream, together with the improved computable abilities available today, will not deflate the importance of broad questions and intellectually lively and stimulating analysis – as Bardhan, Lewis, and Taylor, among others, show.

## The intrinsic tension between theory and practice

The rise and eventual crisis of the intellectual and disciplinary project of the first generation of development economists, the expansionism of neoclassical economics as an analytical tool also of development economics – from which it was initially barred – and the concomitant preservation of an institutional perspective have transformed development economics deeply. And yet, despite this transformation, there remains a continuing tension between theory and practice. Almost thirty years after publishing his 1967 book on World Bank development projects, Hirschman recalled that while he was working on it and visiting development projects at the four corners of the globe, he was so immersed into the particulars of the specific stories "that storytelling came at times to overshadow analysis" (Hirschman 1994, p. xvi). The question undergirding his research was how development knowledge can

emerge from practical problems, and how in turn, this knowledge affects the project cycle. The outcome of that endavour was quite paradoxical, for whereas Hirschman based his analysis on a deep immersion in the practical dimension, the Bank's economists found in his book neither theoretical illumination nor practical suggestions that could help them appraise and evaluate projects better than before.

A few years later, Judith Tendler noticed that there are huge similarities between a development agency and its client countries, as both start from a condition of basic ignorance, and their main question is to understand what works and what doesn't in view of a certain development goal, often to be defined as well. Problem-solving routines familiar to other organizations do not exist in the development world, while huge gaps in experience and understanding of the development problems are the rule. As Tendler put it, "The task of development assistance, then, involves not only 'doing'. An essential portion of it has to do with learning . . . Development know-how was spoken about as if it were like capital – a stock of good capable of being transferred from its owners to the less privileged. But development knowledge is not simply a stock with transferable properties. The peculiar nature of the development task makes knowledge a *product* of the transfer experience itself" (Tendler 1975, pp. 9 and 10, emphasis in the original).

Tendler's lucid analysis of the nature of this "augmented definition" of development knowledge, as she calls it (Tendler 1975, p. 10), pointed to the very rationale for development assistance when the assisting actor's first move should be in fact to admit that it knows as much as the assisted (or even less, as it is more ignorant of local specificities). Not unexpectedly, however,

development organizations have failed to adopt this augmented definition and *ignoramus* attitude, as this in turn would raise concerns about their credibility and their status of "expert" organizations.

The tension between theory and practice that emerges from discussions about how development knowledge is generated characterizes also today's development field. In 2006, a panel of experts chaired by the recent Nobel laureate in economics Angus Deaton, evaluated World Bank research in the years 1998-2005. Deaton and his colleagues confirmed the need for the organization to have a strong and well-funded research department (in fact, they criticized the Bank for underfunding it), and praised both the outcome of the research department and the fact that Bank researchers customary devote 30 percent of their time to directly assisting colleagues in operations.

The panel, however, noted that research on certain important themes such as globalization and inequality was often used to directly influence the policy debate in a systematically biased way. In the words of the Deaton report, "this research was used to proselytize on behalf of Bank policy, often without taking a balanced view of the evidence, and without expressing appropriate skepticism. Internal research that was favorable to Bank positions was given great prominence, and unfavorable research ignored" (Deaton et al. 2006, p. 6). But worse than political bias was perhaps the fact that too large a fraction of Bank research appeared to be irrelevant to practical purposes, and academically undistinguished – in sum useless. The problem is in fact a structural one, and resides in the "publish or perish" rule of the Bank's research department, as World Bank researchers are required to publish two academic papers a year. Deaton and his colleagues argued:

> The cost . . . is a large number of less than outstanding papers driven too much by the concerns of journals and their referees and too little by the policy needs of the Bank. Nor do these papers make use of the Bank's comparative advantages of local knowledge and a constant stream of important new problems. At the same time, there is great pressure for researchers to demonstrate policy relevance, which frequently leads to drawing conclusions that are not supported by the evidence . . . Satisfying the requirements of academic editors and their reviewers is not the main business of the Bank . . . [and] too much pressure to publish leads researchers to ignore important policy issues in favor of an academic style that is sometimes of limited value (Deaton et al. 2006, pp. 7-8).

Similarly, the newly appointed Chief Economist of the World Bank, Paul Romer, has claimed that too many World Bank policy recommendations address issues of low impact simply because data are readily available and firm conclusions can be reached. He argued that the Bank should work on high expected-return policies despite incomplete information, "with no concern about whether [one] will be able to publish the results from this work in the standard economics journals".[31] He wants to make research more relevant for practice, and at the same time make researchers appreciate the value of

[31] Paul Romer, "Botox for Development", blog entry of September 15, 2015, https://paulromer.net/botox-for-development/

knowledge derived from practice, even if it is incomplete and difficult to categorize or handle or manipulate.

Paul Streeten, once claimed that "The history of development economics can be regarded as a progress from large generalizations and high abstractions to greater specificity and concreteness" (Streeten 1983, p. 877). Balanced and sensible though as synthesis is, this paper shows that the relationship between development theory and practice and, as a corollary, what it means to apply theory to the problem of development, constitutes, to say the least, an unstable equilibrium.

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