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## 24. Albert O. Hirschman

*Michele Alacevich\**

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Reformers . . . behave like the country or the chessplayer who exasperatingly fights on when  
'objectively' he has already lost – and occasionally goes on to win!  
(Albert Hirschman 1963a, 271)

As with many things in Albert Hirschman's life, his career as a development economist was not the outcome of any planning. Born Otto Albert Hirschmann, the son of a Berlin upper-middle-class family of assimilated Jews, he left Germany for good in 1933, when Hitler seized power and the 17-year-old Hirschmann, a Social Democrat activist, risked immediate arrest. He moved to Paris, where he frequented the circles of German and Italian anti-Fascist exiles; then, in 1936, to London for a one-year fellowship at the London School of Economics. In late 1936 he joined the international brigades that supported the Spanish Republic against the coup by Francisco Franco, fighting in the Asturias and Catalonia, but left when the communists radicalized their takeover of the Republican armies. He joined his older sister Ursula in Trieste, Italy, who had married the anti-Fascist philosopher Eugenio Colorni there (Colorni would be killed by the Fascists in Rome; after his death, Ursula would marry Altiero Spinelli, a friend of Colorni's and one of the authors of the Ventotene Manifesto). In Trieste, Hirschmann continued his studies in economics, published his first articles on demographic and monetary issues in Italy and France, and also continued his anti-Fascist militancy, until he was forced to leave Italy because of the increasingly difficult situation for Jews and anti-Fascist intellectuals. The war found him in Paris, where he once again collaborated with anti-Nazi and anti-Fascist movements: this time Hirschmann assisted the American journalist Varian Fry, who had established and managed an undercover organization that helped thousands of Jewish and leftist refugees flee Europe for the United States.<sup>1</sup> Ultimately, Hirschmann also fled to the United States. At the immigration window, Otto Albert Hirschmann changed his name to Albert O., while his last name lost the second 'n'.<sup>2</sup>

At Berkeley, where he studied and wrote his first book, *National Power and the*

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<sup>1</sup> On this episode, see Varian Fry (2013 [1945]), and the 2001 TV movie *Varian's War*, directed by Lionel Chetwynd, with William Hurt and Julia Ormond.

<sup>2</sup> Jeremy Adelman's biography of Hirschman (Adelman 2013) is fundamental reading. Interesting information can be also found in Hirschman (1998) and in the memoirs of his older sister, Ursula Hirschmann (1993).

*Structure of Foreign Trade* (1945), Hirschman married Sarah Chapiro, the daughter of a Lithuanian Jewish merchant family. He then enlisted in the US Army, worked for the Office of Strategic Services in Italy and Germany and became father to the first of two daughters, Katia (born in 1944; Lisa would follow in 1946). Hirschman got his first proper job in the United States, after the end of the war, as an economic analyst for the Federal Reserve Board, where he focused on Western Europe, reconstruction, and Marshall Plan policies. Development issues were not prominent in his work, nor could one predict that they would soon become so. As Hirschman remembered this professional turn 40 years later:

In 1952, the possibility of either going to Europe . . . in connection with the European Monetary Union, or of going to Colombia as an economic adviser arose. I opted to take the second road, because it was new. At that point my wife thought I had gone mad: she wanted to return to Paris, where, after all, she had lived most of her youth. But after a short time she was quite satisfied with my choice . . .

In Colombia it was not easy . . . There was a new planning council that had been established on the recommendation of the World Bank, which had sent a mission to the country. But the Colombians said, 'If you want us to set up a new planning council, send us an economist who is capable of advising us'. The Bank looked around, my name was mentioned, and I was ready to come – and in fact did come. (Hirschman 1998, 80–81)

To the eye of a twenty-first-century observer, Colombia in the early 1950s may seem a rather peripheral destination, not as important as other developing countries, such as India or Indonesia, which in the postwar years were committed to the goal of economic development and modernization. But in fact, in the early postwar years, Colombia appeared to be a country rich in natural resources and full of potential: its government was committed to modernizing the country, so much so that in 1949 it hosted the first 'general survey mission' ever sent by the World Bank to a developing country. The goal of that mission was ambitious: to formulate 'a development program designed to raise the standard of living of the Colombian people' (IBRD 1950, xv) and to establish the model for future World Bank missions to less-developed countries.<sup>3</sup> Colombia, in sum, was seen as a 'laboratory' for the definition of development policies.

The 1949 mission to Colombia was headed by Lauchlin Currie, a prominent former New Dealer, top Federal Reserve (Fed) officer under Governor Marriner Eccles and later personal economic advisor to Franklin D. Roosevelt and Roosevelt's envoy to China. Under his leadership, the mission prepared a thorough study of the social and economic conditions of the country and published a voluminous report envisioning a major comprehensive investments plan (IBRD 1950).<sup>4</sup> As Currie remarked at a conference in Washington while he was drafting the report:

Economic, political and social phenomena are so inter-related and interwoven that it is difficult to effect any significant and lasting improvement in one sector of the economy while leaving the

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<sup>3</sup> 'Outgoing wire', John McCloy to Lionel Robbins, 1 March 1949, quoted in Alacevich (2009). For a detailed analysis of the World Bank 1949 mission to Colombia, see Alacevich (2009). For an analysis of the whole group of early World Bank general survey missions, see Alacevich (2011a). For earlier assessments of those early missions, see Spengler (1954) and Moore (1960).

<sup>4</sup> On Lauchlin Currie, see Sandilands (1990).

other sector unaffected . . . Poverty, ill health, ignorance, lack of ambition, low productivity are not only concomitants – they actually reinforce and perpetuate one another. (Currie 1950, 5)

Despite the widespread political and social violence that vexed Colombia before, during and after the mission as a consequence of the assassination of the opposition leader Manuel Gaitán in April 1949, the collaboration between the Colombian government and the World Bank remained strong. In 1950 a Committee for Economic Development – the *Comité de Desarrollo Económico* – was established to turn the mission's recommendations into actual policies. Lauchlin Currie, former head of the mission, was appointed economic consultant to the Committee, in fact acting as its technical secretary and marking the continuity between the study phase of the mission and the implementation phase of the *Comité*.

Albert Hirschman moved to Colombia in 1952 as World Bank appointee to the body that meanwhile had replaced the *Comité*, the *Consejo Nacional de Planificación* (National Planning Council). It did not take long for Hirschman and Currie to discover that they were taking opposite positions on basically all the deliberations of the *Consejo*. More specifically, Hirschman had little patience for the rhetoric of the comprehensive plan that characterized Currie's approach to development planning. Hirschman's first published criticism appeared in a 1954 article where he described integrated development planning as a 'myth', a futile exercise because the figures included in an investment budget could not be other than heterogeneous, tentative and imprecise. In fact, in Hirschman's words, 'the pretense of total, integrated economic planning could and often does coexist quite amicably with, and may serve to cover up, unregenerated total improvisation in the actual undertaking and carrying out of investment projects' (Hirschman 1971 [1954], 47)

Over time, Hirschman began to recognize that his many disparate observations and disagreements with the comprehensive, integrated approach of the *Consejo* headed by Currie actually resulted in a larger, coherent critique of what Hirschman described as a 'new orthodoxy' in development economics. In Hirschman's view, the comprehensive approach envisioned by Currie bore the same flaws as the theories that were quickly shaping development economics in those years. In a seminal 1943 article on the 'Problems of Industrialisation of Eastern and South-Eastern Europe', for example, Paul Rosenstein-Rodan had made the establishment of an industrial sector and the massive transfer of manpower from agriculture to industry the central point of his development strategy. The new industrial sector, Rosenstein-Rodan claimed, was to be considered 'like one huge firm or trust' (Rosenstein-Rodan 1943, 204). The underlying assumption was that only the concerted establishment of many industries, as though they were different departments of one single huge firm as opposed to isolated factories, would create the external economies and the demand necessary for the new industrial sector to thrive. As Rosenstein-Rodan put it, if unemployed agrarian workers

were taken from the land and put, not into one industry, but into a whole series of industries which produce the bulk of the goods on which the workers would spend their wages, what was not true in the case of one [isolated] shoe factory would become true in the case of a whole system of industries: it would create its own additional market (Rosenstein-Rodan 1943, 206)

Another prominent development economist, Ragnar Nurkse, thus concluded that 'a frontal attack of this sort – a wave of capital investments in a number of different

industries – can economically succeed while any substantial application of capital by an individual entrepreneur in any particular industry may be blocked or discouraged by the limitations of the pre-existing market’ (Nurkse 1962 [1953], 13). These theories were soon described as a ‘balanced-growth’ approach to development, because they envisioned a growth on many fronts, based on a fundamental complementarity between different sectors. A sector that would grow alone or too fast with respect to others, was, in this perspective, not only useless for the purpose of development but also doomed to fail.

Hirschman’s critique of these theories took full shape in his book *The Strategy of Economic Development*, published in 1958, two years after he and his family had returned to the United States from Colombia. In that book, Hirschman waged an attack against the balanced-growth approach:

My principal point is that the theory fails as a theory of *development*. Development presumably means the process of change of one type of economy into some other more advanced type. But such a process is given up as hopeless by the balanced growth theory which finds it difficult to visualize how the ‘underdevelopment equilibrium’ can be broken into at any point . . . The balanced growth theory reaches the conclusion that an entirely new, self-contained modern industrial economy must be superimposed on the stagnant and equally self-contained traditional sector. (Hirschman 1963b [1958], 51–52, emphasis in the original)

Since the key to development was the process of change starting from a condition of economic and social backwardness, the elements considered by balanced-growth proponents as obstacles – scarce capitals, imperfect markets, the inability to coordinate entrepreneurial decisions – became the basic elements of Hirschman’s analysis. The question, in other words, was not how to secure foreign aid, how to create new and efficient markets, or how to substitute centralized decision-making for imperfect entrepreneurial business decisions. On the contrary, Hirschman claimed, the question was how to foster investment decisions in a situation characterized by scarce capitals, imperfect markets and constrained entrepreneurial activity. Hence, Hirschman’s focus turned to the process of resource mobilization or, as he put it, on finding the ‘inducement mechanisms’ and the ‘hidden rationalities’ that made growth possible, and the ‘linkages’ that connected sectors or industries or firms; in a nutshell, the mechanisms that would facilitate additional investment in spite of the apparent shortcomings of an underdeveloped economic and institutional landscape. ‘Development depends not so much on finding optimal combinations for given resources and factors of production’, Hirschman wrote, ‘as on calling forth and listing for development purposes resources and abilities that are hidden, scattered, or badly utilized’ (Hirschman 1963b [1958], 5).

Hirschman’s radical critique of the balanced-growth approach was widely discussed and reviewed. *Strategy* quickly became a standard reference in development studies, earning his author fame and tenure at Columbia University. Perhaps most important, from a disciplinary perspective, is that the book addressed so directly the core of the early postwar development debate that it immediately became an essential and defining element of it. The balanced- versus unbalanced-growth dichotomy is a central theme of early development theories. As Paul Krugman once put it, the ‘glory days of high development theory’ span about 15 years, from the 1943 article by Rosenstein-Rodan to the publication of Hirschman’s *Strategy* in 1958 (Krugman 1994).

## DEVELOPMENT ECONOMICS IN THEORY AND PRACTICE

Whereas the balanced- versus unbalanced-growth debate, which Hirschman so insightfully contributed to shape, was central to the theory of development economics, its influence on the practice of development policies is less obvious. This apparent contradiction can be explained with the help of an episode from the years in Colombia: how the Currie mission, the following national planning committees, and Albert Hirschman discussed the opportunity of building an integrated steel plant.

At the end of World War II, domestic steel production in Colombia was regarded as an overriding strategic national interest, and the project for an integrated steel plant near the city of Paz del Río, in an internal, mountainous region, was at an advanced stage of implementation. The 1949 World Bank mission advised against the plant, both in terms of merit as a capital-intensive production requiring extremely high output, and in terms of location: the plant was isolated from the main lines of communication of the country, thus making it arduous and expensive to transport the output to the final destinations. Furthermore, this isolation would prevent the establishment of nearby plants that in principle could have harnessed the by-products of steel production (IBRD 1950, 423–425).

A document prepared by the Comité de Desarrollo Económico added that ‘the maximum progress achievable [for the years 1951–1955] is only possible if the amount devoted to each sector, for example transportation, agriculture, or industry, is proportional to its contribution to the overall Colombian economy during this period’.<sup>5</sup> The alternative uses of the funds needed for the integrated steel mill were also listed: the construction of a hydroelectric plant covering half of the production of electric power in Colombia in 1950; or thermoelectric plants; or 30 000 private houses; or the purchase of 266 diesel locomotives and the increase of roads and railways by about 500 km; or the construction of 23 000 rooms for hospitals, or new schools accommodating 900 000 students.<sup>6</sup> Hirschman’s critique addressed precisely this kind of reasoning, namely the idea that the development of a sector was to be proportional to the development of other sectors and that an investment plan was a zero-sum exercise. As noted above, Hirschman underscored the dynamic character of industrialization and thus the ‘backward’ and ‘forward’ linkages that a specific investment would have encouraged upstream or downstream from that initial investment. A steel industry, according to Hirschman, was not only a ‘national symbol’, as the Currie mission thought, but a veritable engine of regional and national industrialization: ‘Perhaps the underdeveloped countries are not so foolish and so exclusively prestige-motivated in attributing prime importance to this industry!’ (Hirschman 1963b [1958], 108).

In this polemic, both sides overlooked many important points of contact between their apparently opposed perspectives. The report of the World Bank argued that technical and geographic limits would weaken the impact of the prospective steel plant on the development of other industrial sectors. The steel plant was unlikely to leverage a ‘growing point’, which was considered the most desirable result for both the Currie

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<sup>5</sup> Comité de Desarrollo Económico, ‘Informe de la misión para el Comité. Fomento de una industria colombiana de acero’, draft, 15 December 1950, 15, Lauchlin B. Currie Papers, Duke University.

<sup>6</sup> *Ibid.*

mission and Hirschman, who dedicated an entire chapter of *Strategy* to growing points (Hirschman 1963b [1958], Ch. 10). When it became clear that the Colombian government was determined to build the steel plant despite the mission's opposition, Lauchlin Currie recommended developing it in stages. This would have limited the costs of the unfortunate location and allowed the business to gain strength despite this initial handicap. There would have been gradual growth as staff and workers acquired competencies and the plant itself gained a bigger market share. This reasoning was not too different from Hirschman's attempt to find 'hidden rationalities' in the process of growth. Thanks to these hidden rationalities, initial obstacles may play a positive role in the process of growth, as they help forge decisions and find new solutions. The mission and later the Comité tried to mediate between industrial and political motives, placing themselves at the center of a decision-making and problem-solving process. This is what Hirschman highlighted as the indispensable element (but also the rarest) for development: the ability to make entrepreneurial decisions.

A few perceptive commentators had noticed that despite the strong polemical tone of that early diatribe and its spillovers in the theoretical debate, the two seemingly irreconcilable approaches had many points of contact. As Amartya Sen put it, Hirschman seemed to be 'overstating' his case:

Controversies on 'balanced' versus 'unbalanced' growth tend to leave the readers . . . a little puzzled. Put in their native forms, both the doctrines look right; examined from the other's point of view, each looks totally inadequate . . . The 'balanced' and the 'unbalanced' growth doctrines have a considerable amount of common ground. (Sen 1960, 591–592)

For example, when Rosenstein-Rodan and Nurkse discussed how to mobilize 'disguised unemployment' in agriculture, they were in fact trying to bring up what Hirschman would have called 'hidden or badly utilized resources'.

It should be noted, however, that the years of 'high development theory', as Krugman labeled it, were pioneering years for development economics. The focus of this new and thriving discipline – how backward societies are to develop – was quickly becoming one of the top priorities of the international agenda. With the onset of the Cold War, international development and foreign aid became fundamental strategic questions. Within a nascent discipline quickly gaining global relevance, the debate soon became overheated. Opposing theories are natural elements of any field of human inquiry, and admittedly the 'balanced' and 'unbalanced' approaches highlighted different aspects of the development process. But in addition to intellectual disagreements, another key conflict was about what should be the overarching goal of the discipline and which approach would best incarnate this goal. As sociologist Robert K. Merton once noticed:

These controversies follow the classically identified course of social conflict . . . Since the conflict is public, it becomes a status battle more nearly than a search for truth . . . The consequent polarization leads each group . . . to respond largely to stereotyped versions of what is being done by the other . . . Not that these stereotypes have no basis in reality at all, but in the course of social conflict, they become self-confirming stereotypes. (Merton 1973 [1961], 55–56)<sup>7</sup>

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<sup>7</sup> For a thorough discussion of the early debates in development economics see Alacevich (2011b).

## NEW QUESTIONS AND THE SEARCH FOR A NOVEL PERSPECTIVE

As it happens, once the pioneering phase of a disciplinary field is over, the theoretical heat also diminishes. After the publication of *Strategy*, Hirschman himself quickly became less interested in high theoretical debates, shifting his focus instead to in-depth analysis of specific, highly contextualized processes of change. Hirschman's second and third books of what he would later describe as his 'development trilogy' (Hirschman 1995 [1994], vii) rested strongly on deep historical and context-specific analysis. How can this shift be explained?

After almost 20 years of expansion, the development field – and development economics with it – was undergoing a major transformation. In the pioneering days of the early postwar period, development policies were based on nothing more than trial and error, theories were only then starting to take shape, and observations and data were scant. As Lauchlin Currie, Hirschman's arch-enemy during the Colombian days, later pointed out:

there were no precedents for a mission of this sort [the 1949 mission to Colombia] 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, 30–31)

By the early 1960s, however, the broad generalizations and big concepts that had shaped development economics during its initial years were considered increasingly inadequate to explain the causes of, and remedies for, the economic and social underdevelopment of the so-called Third World countries, which were quickly growing in number due to the demise of the colonial empires. It was increasingly apparent that these countries presented different, specific traits and challenges. But the sense of inadequacy of development theories was also due to the confusing record, to say the least, of development policies implemented until then. The outcomes of foreign development assistance were not easily interpretable, and in many cases they offered evidence of failure: 'high development economics' apparently had not delivered what it had promised. A 1964 report by the Brookings Institution on foreign aid summarized this feeling of disorientation:

Aid is being extended, bilaterally and multilaterally, to 100 countries, democratic and authoritarian, allied and neutral, progressing and retrogressing, without satisfactory standards for evaluating competing claims, promoting particular strategies, harmonizing aid with other available instruments of policy, or appraising the results achieved. There is need for a better political, moral and economic framework within which to review our foreign aid programs and to prepare recommendations regarding their future.<sup>8</sup>

Building on the methodological lesson of his former colleague, the economic historian Alexander Gerschenkron, Hirschman was among the first development scholars to rec-

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<sup>8</sup> The Brookings Institution, Division of Foreign Policy Studies, 'Proposal for a Program of Studies Dealing with American Foreign Assistance', April 1964 (Revised), 5, Projects and Studies – Projects Evaluation – Professor Albert O. Hirschman – Volume 1, Operations policy files – Projects and studies – General Files – General 1946–1968, World Bank Group Archives (henceforth: World Bank Hirschman Folder, WBGA).

ognize the need for a different approach. Gerschenkron had shown that the process of economic development of continental European countries (the so-called ‘second-comers’ and ‘latecomers’), far from replicating the English model, had followed idiosyncratic paths of modernization depending on their institutional, political, financial and social resources, their relative backwardness and the timing and extent of their industrialization effort (Gerschenkron 1962, which collects articles dating back to the early 1950s). The lesson was also clear for the ‘very latecomers’ of the mid-twentieth century: no ‘one size fits all’ recipe or theory of development could provide all the answers to the problems of underdeveloped countries.

Hirschman was also critical of the increasing appeal that revolution seemed to have for Latin American intellectuals and policy-makers in search of a way to overcome social and economic underdevelopment and inequality. Hirschman thought that comprehensive plans on one side of the spectrum and revolutionary solutions on the other side were inadequate: by attempting to overcome underdevelopment in one big leap, they left out the complexities of the process of change from a backward or pre-revolutionary society to a modern or post-revolutionary one. Hirschman would later summarize his criticism to both approaches in his Foreword to the first book by Judith Tendler, a PhD student whom he was mentoring in the early 1960s (now Professor Emeritus of Political Economy at MIT):

Underdevelopment having been diagnosed as something so multifaceted, tangled, and deep-rooted, it was often concluded that the situation called for revolution, massive redistribution of wealth and power from the rich to the poor countries, or at least coordinated attack on pervasive backwardness through highly competent central planning.

But what if none of these *dei ex machina* are available to take matters properly in hand? What if the fortress of underdevelopment, just because it is so formidable, cannot be conquered by frontal assault? In that unfortunately quite common case, we need to know much more about ways in which the fortress can be surrounded, weakened by infiltration or subversion, and eventually taken by similar indirect tactics and processes. And I suggest that the major contribution to our knowledge of economic development must now come from detailed studies of such processes. (Hirschman 1968, vii–viii)

Judith Tendler’s book was a fine example of this new approach.<sup>9</sup> In it, Tendler discussed how hydro and thermal power had affected the Brazilian economic development of the 1950s and the 1960s in different ways. From this specific case, Tendler was able to build a solid and broader analysis of certain development dynamics of postwar Brazil. In Tendler’s words:

should the work therefore be properly identified as a narrow case study, with no pretension of relevance to more general questions of economic development? The answer . . . is in the negative, for this study is meant to illustrate the general thesis that technologies vary as to their political vulnerability, their ability to draw out and train competent talent, and their capacity to brook the coexistence of politically antagonistic institutions. The lesson of this particular case is not that hydro should be favored in developing countries, but that the technological configuration of a project, program, or economic activity is a valuable source of information in the study of opportunities for economic development. (Tendler 1968, 6; see also Tendler 1965)

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<sup>9</sup> For other examples, see Weaver (1968) and Echeverría (1969).



## THE POLITICAL ECONOMY OF DEVELOPMENT: *JOURNEYS TOWARD PROGRESS*

Of course this shift in perspective was also apparent in Hirschman's work. Historical narratives of specific economic problems in three Latin American countries make up two-thirds of his 1963 book, *Journeys toward Progress*. As he put it, 'the essence of this volume is in the flow of the three stories' about problems that had plagued Latin American countries for a long time, namely, drought in North-East Brazil, land reform in Colombia and inflation in Chile (Hirschman 1963a, 1). Hirschman's interest lay in how governments in Latin America would solve these problems and the related policy-making processes. 'The best method of looking for answers', he claimed, 'was to scrutinize the record of a few specific, documented, protracted, significant policy problems' (Hirschman 1963a, 2).

However idiosyncratic the record of specific problems, Hirschman was interested in finding elements that would improve the effectiveness of policy reform in the future. While he did not believe that any iron law governed social, economic and political dynamics, his research aimed at understanding the features underlying the policy-making processes that he had examined in his country studies. His focus on historical analysis, thus, was not a goal per se, but a way to achieve a broader understanding of political processes in Latin America. Hirschman's terminological choices are revealing of his awareness of the difficulties to generalize from specific cases: he predicted that his analysis would produce only 'tentative' and 'dispersed' findings, and stated that his goal was not to produce a theory of policy-making, but to sketch 'a Latin American "style" in handling, learning about, and moving toward the solution of large-scale policy problems' (Hirschman 1963a, 227).

Underdeveloped societies, Hirschman noted, are characterized by inadequate and ineffectual communication between citizens and the government. In such a situation, violence and mass protest are often the only way for long-neglected problems to get the attention of the government. Problems that are perceived as 'pressing' will often produce some reaction and attempt at solving them, but this is not necessarily based on a real understanding of the roots of the problem. 'Motivation', in other words, would have priority over 'understanding'. The effects of this dynamic are often visible in underdeveloped countries: certain grandiose attempts to attack big problems frontally, disregarding narrower but perhaps more manageable targets; the launch of comprehensive plans to solve all problems at once; the frequent establishment of new institutions dedicated to solve once and for all a certain problem; the excessive reliance on imported solutions that appear to be lifesavers only because they are 'foreign'; and finally, major policy shifts and ideological clashes.

Ideology, in particular, was a fundamental lens to understand the development debate in Latin America and the Latin American style of problem-solving and policy-making. Hirschman had specifically touched on the role of ideology in Latin America in the introductory chapter to his 1961 edited book *Latin American Issues* (Hirschman 1961). In this collection of essays on the problem of inflation in Latin America, Hirschman's attempt was to discuss the role of ideology in shaping the surrounding debate. As Jeremy Adelman put it, 'if economists were accustomed to thinking of themselves as outside societies looking in, like a doctor examining a patient, Hirschman turned them into the subject' (Adelman 2013, 369).

When Hirschman examined ideology and the other elements of policy-making in *Journeys*, he discovered that they presented several interesting and promising features. Those seemingly dysfunctional features of Latin American policy-making, far from being hopelessly negative, actually functioned as by-ways through which countries lacking a political system capable of ensuring a dialogue between the public and policy-makers became aware of certain problems and committed, perhaps unevenly, to their resolution. For example, comprehensive plans proved to be particularly useful to link neglected problems to a pressing problem already under the spotlight. Thus the ‘comprehensive plan’, the idea that no solution can come to any specific problem unless a whole set of issues is addressed in a coordinated way, became in Hirschman’s perspective ‘a generalized device for indirectly achieving recognition for the stepchild problems’ (Hirschman 1963a, 232). As mentioned above, Hirschman’s aversion to comprehensive development plans had prompted his harsh critique of the balanced-growth approach and his outline of an alternative, unbalanced approach in *The Strategy of Economic Growth*. But while in that case Hirschman was engaged in a theoretical debate on the mechanisms of industrial development in less-developed countries, in *Journeys* he was discussing a very different set of questions. The comprehensive plan, economically inefficient and useless, was – from the perspective of policy-making – an important strategic device. As pointless and rigid as a comprehensive plan might seem when the question was how to encourage economic entrepreneurship, it turned out to be rich and flexible when the question became how to bring about political reforms.

In the same vein, other characteristics of this ‘Latin American style’ may have positive if unexpected sides. Excessive reform promises, for example, are usually followed by meager results. Yet, a ‘utopian phase of policy-making’ often has the merit of generating legislation which, even though initially unenforced, nonetheless exists ‘on the books’. In a subsequent round of reform efforts, this dormant legislation will probably be enforced, thus securing the legal basis for policy initiatives that would have otherwise been inapplicable. Likewise, the mix of outright criticism of previous policies and blind faith in new approaches that characterizes the policy swings studied by Hirschman in his case studies had a number of positive spin-offs. First of all, the habit of considering all government attempts at reform as eventually doomed made opposition to them weaker than it might have been. In other words, the rhetoric of failure opened up spaces for actual reform. At the same time, the bombastic announcements of new policies – while likely destined, like previous efforts, for less than successful epilogues – had an energizing effect on policy-makers: a selective amnesia on previous failures made new attempts possible. Hirschman’s optimism seems excessive here, for the two effects of the rhetoric of failure and the rhetoric of success might as well produce outright reactionary results.

However, there was a method to this optimism. Hirschman’s 1963 book, after all, is an attempt at explaining the mechanisms of economic and social reform short of revolutionary events. It should be noted that revolution, in early 1960s Latin America, was a rather fashionable concept, its most successful example being the recent Cuban revolution. Hirschman did not deny the existence of situations, in Latin America and elsewhere, in which opposition to reform and concentration of power were so rigid that the only hope to bring about some change was through a revolution. However, his main interest lay in those cases where reform was feasible. In such cases, calls for revolution were unnecessary, and revolutionary intellectuals were analytically weak: instead of explaining social

change, they contented themselves with invoking a 'cataclysmic interlude between two static societies': the pre-revolutionary one, rotten and unjust and unresponsive to reform; and the post-revolutionary one, just and harmonious and no longer in need of further improvement. Similar to what Hirschman had already observed in his criticism of the balanced-growth approach, which he thought superimposed a developed society on a backward reality, he found this time that calls for revolution impeded the analysis of the truly interesting question: that is, how change happens. There was a lot to be explored in the space between effortless reforms at one extreme and revolutions at the other. Hirschman's study was a way to unravel material for a 'reformmonger's manual'. As he put it, 'perhaps it is time that such a text be written and offer some competition to the many handbooks on the techniques of revolutions, coups d'état, and guerrilla warfare' (Hirschman 1963a, 256).

Indeed, Hirschman's goal was 'to show how elements of both reform and revolution are present in the sequences of policy-making which we have studied' (Hirschman 1963a, 256), and from this perspective, violence itself could become an ingredient of reform. Colombian peasants, for instance, had often seized uncultivated land illegally and violently. But those eruptions of violence, far from leading to revolution, made it possible for reformist governments to legislate about land redistribution in ways that would have been unthinkable without the peasants' violent land seizures. Hirschman was amused by the 'highly disorderly sequence' the Colombians had invented: instead of the predictable two-step sequence from revolution to land redistribution within the framework of the new revolutionary legal order, Colombian peasants had first enacted decentralized and illegal redistribution, which was later legalized thanks to a reformist government (Hirschman 1963a, 260). More generally, Hirschman highlighted the enormous complexity of reformist action in Latin American countries and the mixed character of reform policies, for they appeared to be made of both non-antagonistic and antagonistic dynamics.

The whole book and the research upon which it was based, which Hirschman had conducted during multiple trips to Latin America with his wife, Sarah, and his colleague and friend, political scientist Charles E. Lindblom, was an uncompromising attempt at introducing public-policy analysis in the realm of development economics. Dissatisfied with the relegation of public decision-making to the role of mere 'preconditions' to economic growth, in *Journeys* Hirschman was determined to show how processes of decision-making and problem-solving play a decisive role at all stages of development. He explicitly connected this goal to his previous book, *Strategy*, in which he had investigated a variety of mechanisms able to generate entrepreneurial resources to advance economic development.

Hirschman's attempt at connecting economic and political analysis, perhaps not unexpectedly, was appreciated mostly by political scientists, whereas fellow economists advanced more than one criticism. Mancur Olson, Jr considered *Journeys* probably the best book published until then on the two-way relationship between economic development and policy-making (Olson 1965). Several economists, however, noted that the economic analysis of the three case studies was insufficient and at times unconvincing. As Dudley Seers put it in an otherwise positive review, 'it is one thing to say that economic theory needs to take account of political factors, another to leave economics very largely out of the picture' (Seers 1964, 158).

## THE POLITICAL ECONOMY OF PROJECT APPRAISAL: DEVELOPMENT PROJECTS OBSERVED

The research that resulted in Hirschman's 1967 book, *Development Projects Observed*, rested on the same methodological approach as *Journeys*. Although the book does not contain the stories of the individual projects studied by Hirschman, the research had an 'intensive concern with "cases"' and all the projects considered had 'an extended history'. 'Immersion in the particular,' Hirschman claimed, 'proved . . . essential for the catching of anything general' (Hirschman 1967, 3). The book was the result of Hirschman's collaboration with the World Bank, the Brookings Institution, the Ford Foundation and the Carnegie Corporation to study some general elements of project appraisal or, as he put it, 'to explore in detail the direct effects as well as the broad repercussions of a project on economy and society' and to reach 'some improvements in the process of project evaluation and selection'.<sup>10</sup> An additional purpose, after two books based exclusively on Latin American cases, was for Hirschman to broaden his expertise to Asia and Africa.

Hirschman selected 13 World Bank projects and travelled to Latin America, Asia, Southern Europe and Africa between July 1964 and August 1965 to study them in detail. Bank officers were enthusiastic: 'Probably for the first time', remarked a senior economist, 'the contemporary theory and practice of project appraisal in infra-structure will be subjected to a systematic *ex post* methodological scrutiny on a wide basis'.<sup>11</sup> This was all the more important since the World Bank had not yet established an internal and independent evaluation function.

Back from his travels, Hirschman circulated a memo with some preliminary observations. These focused on what he called 'Behavioral characteristics of development projects in different sectors'. As he put it: 'having learnt in fairly rapid succession about a wide variety of projects, I became alerted to the characteristic advantages or handicaps under which power projects, say, proceed as compared to irrigation projects'.<sup>12</sup> The principal aim of Hirschman's interim observations was clearly methodological. Far from addressing questions such as the economic return of World Bank loans or the traditional distinctions of infrastructure versus agricultural and industrial projects, or human versus physical capital, Hirschman focused on questions such as the degree of uncertainty in a project:

the element of the unknown, the uncertain and the unexpected which deflects projects from the originally chartered course is considerable in all projects. But it is far more important in some projects than in others and it may be of interest to the Bank to gain an approximate idea about the principal determinants of this uncertainty.<sup>13</sup>

Among these determinants, Hirschman listed the existence of visible linkages between

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<sup>10</sup> Albert O. Hirschman, 'A Study of Completed Investment Projects which Have Received Financial Support from the World Bank', June 1963, World Bank Hirschman Folders, Vol. 1, WBGA. This and the following documents are discussed in more detail in Alacevich (2014). See also Alacevich (2015).

<sup>11</sup> Dragoslav Avramovic to Department Heads, IBRD and IFC, 'Investment in Developing Countries – Effects, Expectations and Reality', 18 February 1964, World Bank Hirschman Folders, Vol. 1, WBGA.

<sup>12</sup> Albert O. Hirschman, 'A Study of Selected World Bank Projects – Some Interim Observations', August 1965, World Bank Hirschman Folders, Vol. 1, WBGA.

<sup>13</sup> *Ibid.*

the project's new supply and local demand, and the influence of social and political variables. In addition, Hirschman discussed the difficulty of calculating benefits and measuring results in several types of projects. This change in perspective called for a corresponding change in the Bank's behavior. The Bank, Hirschman wrote, should avoid the 'air of pat certainty' that emanated from the prospects of new projects, and instead expose the uncertainties underlying them. Moreover, the Bank should take into account the distributional and the social and political effects of its lending. Focusing only on the technical merits of a project, Hirschman concluded, was not enough.<sup>14</sup>

It is no wonder that Bank staff, initially delighted when Hirschman started his study, were instead upset when those first observations were circulated. The final outcome of Hirschman's research, his 1967 book, *Development Projects Observed*, did not help to defuse tensions. In it, Hirschman focused on the side-effects of projects and described project appraisal as the art of visualizing them. In his definition, side-effects were not just 'secondary effects': they were 'inputs essential to the realization of the project's principal effect and purpose'. They were equally essential for the project to mature into a long-lived endeavor (Hirschman 1967, 161). What was the difference, for instance, between a highway project and a railway project? A side-effect of investment in highways is that it develops the heavy motor vehicle industry and thus enhances entrepreneurship. But 'entrepreneurship means political power, which in turn means the ability to change the rules of the transportation game decisively in favor of the highways' (Hirschman 1967, 162). A secondary effect, in other words, may become a decisive element for the future of transportation policies in a given country.

With this methodological approach to project appraisal, it is not surprising that Hirschman believed that cost-benefit analysis was an excessively rigid process, hampered by too many arbitrary assumptions. The search for a yardstick to rank potential projects was, according to Hirschman, a futile exercise. He wondered:

How could it be expected that it is possible to rank development projects along a single scale by amalgamating all their varied dimensions into a single index when far simpler, everyday choices require the use of individual or collective judgment in the weighing of alternative objectives and in the trade-off between them? (Hirschman 1967, 179)

Hirschman was suspicious of cost-benefit analysis because he thought its allegedly 'scientific' index offered no useful tools for better policy-making. 'Each project', Hirschman remarked, '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, 186). Uncertainties and latitude (whether and how a project can be turned in one direction or another regardless of outside occurrences) condition a project's functioning and outcome, and they must be at the center of the appraising exercise. Hirschman's goal, however, was not to recommend all-encompassing appraisal criteria. Rather, his attempt was 'to provide project planners and operators with a large set of glasses with which to discern probable

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<sup>14</sup> Ibid.

lines of project behavior, in the expectation that the analysis of each individual project would require different and rather limited subsets of the full set of glasses which has been exhibited' (Hirschman 1967, 186).

As interesting and stimulating as Hirschman's approach was, it offered few elements for World Bank officers to make project appraisal operational. How could Hirschman's analysis be embedded in an organizational routine for internal evaluation? As a World Bank senior manager put it, the book:

is well written and contains a number of interesting observations. But by and large it does not contain any operationally useful analysis of the merits and priority of the particular projects observed by Professor Hirschman or of the kind of reshaping or rethinking of the projects which might have made them better. In short, I for one gained no significant new insights into the process of project preparation and evaluation.<sup>15</sup>

Hirschman's attempt to establish a qualitative approach to project appraisal was based on detailed historical reconstruction of the 'personal profiles' of projects as well as their larger political and social context. It aimed at underscoring 'the element of the unknown, the uncertain and the unexpected', as Hirschman put it, in order to understand what caused projects to change direction from their originally charted course. Finally, the goal was to assess the broader political, social and economic impact of a project. Hirschman's approach to project appraisal was a natural evolution of his previous work, which had underscored his increasing distance from the early debates in development theories. The apparent 'failure of several of the earlier ideas as *practical* policy solutions', as Tony Killick (1978, 27) put it, prompted Hirschman's detailed examination of the mechanisms of economic policy-making in *Journeys* and project appraisal in *Development Projects Observed*. But while Hirschman and the World Bank had agreed on what was needed, they ultimately disagreed on how to meet this need. Hirschman tried to transform the Bank's approach to project design, management and appraisal. The Bank, instead, expected Hirschman to make project design and management somehow more measurable, predictable and scalable. World Bank officers asked Hirschman to collaborate on an operational version of his book, but this ultimately did not see the light, primarily because of Hirschman's lack of interest in the project.<sup>16</sup>

When a few years later the World Bank established an Operations Evaluations function, Hirschman's work was virtually forgotten. Project appraisal and evaluation was solidly based on the cost-benefit analysis approach pioneered in the 1920s and 1930s for water-resources development and public investment activities and further developed in the 1960s and 1970s by a new wave of studies, such as those by Ian M.D. Little and James A. Mirrlees (1968, 1974) and Partha Dasgupta, Steven Marglin and Amartya Sen (1972). Although these studies did not ignore the role of uncertainty in project design

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<sup>15</sup> Richard H. Demuth to Mr. Robert E. Asher, 13 September 1966, World Bank Hirschman Folders, Vol. 2, WBGA.

<sup>16</sup> Herman G. van der Tak to Professor Albert O. Hirschman, 20 December 1966, World Bank Hirschman Folders, Vol. 2, WBGA; Herman G. van der Tak to Professor Albert O. Hirschman, 18 January 1967, World Bank Hirschman Folders, Vol. 2, WBGA; Albert O. Hirschman to Mr. Alexander Stevenson, 1 May 1967, World Bank Hirschman Folders, Vol. 2, WBGA; Alexander Stevenson to Professor Albert O. Hirschman, 18 May 1967, World Bank Hirschman Folders, Vol. 2, WBGA.

and appraisal, in practical matters they tended to conflate this term with what is usually meant by 'risk', that is, something subject to measurement. Hirschman, instead, following the dichotomy between 'risk' and 'uncertainty' put forth by Frank Knight, considered uncertainty impossible to measure. As he wrote in the early 1960s, 'it is clearly impossible to specify in advance the optimal doses of . . . various policies under different circumstances. The art of promoting economic development . . . consists, then, in acquiring a feeling for these doses' (Hirschman and Lindblom 1971 [1962], 83–84).

## THE CRISIS OF DEVELOPMENT ECONOMICS

*Development Projects Observed* was the last major work in Hirschman's opus on development. Although he would remain interested in development issues for his entire life and would publish further important essays on development questions, his subsequent book, *Exit, Voice and Loyalty* (1970), belonged to the broader social sciences. For its part, development economics was by then entering a process of decline. While the promise of rapid economic expansion had motivated an entire generation of economists between the 1940s and the 1950s, the failures of the subsequent development strategies shattered the foundations of the field. Hirschman's tone was particularly mournful in acknowledging the crisis of development economics:

Some twenty-five years later, that early optimism has largely evaporated, for a number of reasons. Growth, while substantial, has by no means overcome the division of the world into the rich 'north' and the underdeveloped 'south'. In the south itself, moreover, the fruits of growth have been divided more unevenly than had been anticipated. And there is another, often unacknowledged reason for the disenchantment: it looks increasingly as though the effort to achieve growth, whether or not successful, brings with it calamitous side effects in the political realm, from the loss of democratic liberties at the hand of authoritarian, repressive regimes to the wholesale violation of elementary human rights. (Hirschman 1981a [1979], 98–99)

Especially for a scholar like Albert Hirschman, whose work was guided by a reformist perspective and was equally distant from the opposing options of revolution and acceptance of the status quo, the blow was strong. Hirschman wrote that 'little light had been shed on the connections between economic growth and political disasters by my contemporaries' (Hirschman 1981a [1979], 99), but his own work was similarly exposed to this kind of critique, especially that celebration of reform-mongering that was *Journeys Toward Progress*. When Hirschman, in the early 1960s, was circulating the drafts of his country studies and later, when the book was published, several scholars noticed his excessive optimism. David Felix, reviewing the book, joked that while *The Strategy of Economic Development* might well be subtitled, paraphrasing a seminal article by W. Arthur Lewis, 'Economic Development with Unlimited Supplies of Labour, Foreign Exchange and Everything Else Except the Will to Invest', *Journeys Toward Progress* could be similarly be subtitled 'Social and Economic Reform with Unlimited Amounts of Time', for Hirschman's optimism rested on the judgment that after many false starts, the Brazilian, Chilean and Colombian governments would eventually focus on solving their long-lasting problems (Felix 1964, 202). The US Ambassador to Brazil, who read a draft

of Hirschman's analysis of the drought problem in the Nordeste, commented that things 'may well go worse before they go better'.<sup>17</sup>

## HIRSCHMAN'S DEVELOPMENT LEGACY

The analysis above seems to suggest that, after all, Hirschman's legacy in development economics has been rather limited. Nothing comparable to the pervasiveness of a Harrod–Domar model – rigid and mistaken though the foreign-aid policies based on that model were – could be mentioned in the case of Hirschman's legacy to development thinking. The balanced- versus unbalanced-growth approach diatribe defined the borders of early development economics but had only limited influence on the actual policies of development institutions. Even champions of one or the other approach, when confronted with the implementation of development projects, picked eclectically from seemingly incompatible theories. Thus, alleged balanced-growth supporters and comprehensive planners in Colombia thought in terms of linkages and inducement mechanisms *à la* Hirschman, while Hirschman praised the comprehensive approach of the regional development agency of Northern Brazil. Furthermore, Hirschman's *Journeys*, although highly stimulating, complex and interesting, was weak in terms of predicting future paths of reform in Latin America. Finally, the analytical recommendations in *Development Projects Observed* were quickly left behind by its major prospective client, the World Bank, and the whole field of project appraisal and operations evaluation all but forgot Hirschman's pioneering studies, focusing instead on cost–benefit analysis.

Yet, Hirschman has been a pioneer in many aspects of postwar development studies, and he has remained an influential thinker for development matters, if not in any particularly structured way – certainly not in a 'school', but surely from a methodological point of view. The crisis of development economics, paradoxically, may have helped.

Development economics as an autonomous disciplinary subfield no longer exists. In 1981, Hirschman made a very interesting, if biased, contribution to defining this discipline's specific identity. Development economics, he wrote, was defined by two connected claims. The first was the rejection of monoeconomics, that is, the claim that underdeveloped countries 'are set apart, through a number of specific economic characteristics common to them, from the advanced industrial countries', and thus traditional economic analysis, which has historically focused on economically advanced countries, must be significantly recast to deal with underdeveloped countries. The second was the mutual-benefits claim, that is, the view that both developed and developing countries can yield economic gains from mutual economic relations (Hirschman 1981b, 3).<sup>18</sup> In recent years, a prominent development scholar, Dani Rodrik, has argued instead that what matters is not to apply different theoretical frameworks to different realities such as developed and less-developed countries, but rather to apply different recipes based on the same economics. Ironically, for his 2007 book, unequivocally titled *One Economics, Many*

<sup>17</sup> Lincoln Gordon to Albert O. Hirschman, 3 September 1963, quoted in Adelman (2013, 380).

<sup>18</sup> This was a particularly restrictive definition, as it excluded from the territory of development economics structuralist and dependency theorists who argued that trade relations between developed and underdeveloped countries were disadvantageous for the less-developed countries.



*Recipes* (2007a), Rodrik was awarded the Albert O. Hirschman Prize, the highest award of the Social Science Research Council. But even though Rodrik may be at variance with Hirschman's old definition of development economics, his methodological approach is instead very much in line with Hirschman's thought. For example, Rodrik has consistently underscored the need for pragmatism, policy experimentation and deep knowledge about local contexts against general 'best practices' or rules of thumb. Similarly, he has privileged selective approaches to policy reform or – in his words – 'hitting the right targets and not doing everything at once' (Rodrik 2007b, 5).<sup>19</sup>

If development economics has disappeared as a discipline, the problem of development is still very much alive in public and scholarly debate. Liberated from identity questions, the development field is thriving again, and its approach is much more eclectic than before. Among the major features of the so-called new development economics, Dani Rodrik (2008, 27–28) has emphasized its being experimental (in the broad sense of 'a predisposition to find out what works through policy innovation'), based on monitoring and evaluation, aimed at selective and targeted reforms, focused on policy reforms and diagnostic rather than presumptive; all features that have a distinctive 'Hirschmanesque' flavor. In addition, it should be noted that Hirschman's approach has always had a strong influence on the Latin American development discourse (Adelman 2008; Ocampo 2008).

A central element of Hirschman's approach, of which current development scholarship is only partially aware, is the importance of the historical perspective. The importance of detailed historical analysis in *Journeys Toward Progress* and *Development Projects Observed*, as well as Hirschman's interest in reconstructing the 'personal profiles' of specific projects or questions, have been noted. But Hirschman's historical sensitivity was evident even in his most theoretical works. Sequences, inducement mechanisms, linkages are processes that unfold through time, and like all historical processes, they are intrinsically uncertain and open to unexpected shifts and turns and do not fit in any preordained framework. Hirschman highly valued historical analysis to discover perhaps hidden but nonetheless open possibilities for change and reform. As he put it in a 1980 talk:

Following in detail the process of a revolution gives us a strong feeling, as the structuralist approach does not, for the many might-have-beens of history . . . As a result, the event-minded historian is less likely than the sociologist to declare that, given such a structural condition, the outcome was preordained. [This] emphasis on the revolutionary process . . . in effect promises to restore a few degrees of freedom we were in danger of losing to the structuralists. (Hirschman 1986 [1980], 171–172)<sup>20</sup>

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<sup>19</sup> For Hirschman's affinity to other scholars in the development field, see Ellerman (2001a, 2001b).

<sup>20</sup> For one more testimony to Hirschman's sensitivity to the role of historical reconstruction, see his opinion of Marx's work: '*The Eighteenth Brumaire of Louis Bonaparte* was a particularly fine work. His historical books were much less orthodox than his economic ones . . . I like to understand how things happen, how change actually takes place' (Hirschman 1998, 67).

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