

Albert Hirschman, Development Economics, and the Social Sciences

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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, 1963

Albert Hirschman’s scholarly trajectory was characterized by—in fact, built on—working across disciplines. As Cass Sunstein wrote a few months after his death, Hirschman is principally known for four remarkable books, all published after 1970, which have become classics in the social sciences: *Exit, Voice, and Loyalty* (1970), *The Passions and the Interests* (1977), *Shifting Involvements* (1982), and *The Rhetoric of Reaction* (1991).¹ As it happens, however, Hirschman’s first professional identity in the 1950s and 1960s was that of a development economist. For fifteen years, between 1952 and 1967, Hirschman worked almost exclusively on development issues, producing a vast number of articles and three books: *The Strategy of Economic Development* (1958), *Journeys toward Progress* (1963), and *Development Projects Observed* (1967). His contribution to the early development debate had such a great significance that in fact he is considered a founding father of postwar development economics. Although Hirschman’s development experience is far from neglected, there is nonetheless a tendency to focus on his subsequent work.

A simple and quite persuasive explanation of this is that the publication of *Exit, Voice, and Loyalty* consecrated its author as one of the great social scientists of the postwar period.² Moreover, even though *Exit, Voice, and Loyalty* originated from an

¹ Cass R. Sunstein, “An Original Thinker of Our Time”, *The New York Review of Books*, May 23, 2013.

² Mere ignorance of those writings is not an adequate explanation. Sunstein, for example, is obviously aware of Hirschman’s earlier phase, for he recently prefaced a new edition of a “development” book by Hirschman; see Hirschman 2015 [1967].

unresolved problem in *Development Projects Observed*, its publication inaugurated a new phase in Hirschman's intellectual trajectory. Although his interest in development issues never faded, it is only with the 1970 book and the subsequent move to the Institute for Advanced Studies in Princeton that Hirschman acquired that aura of Renaissance man, impossible to confine within the fences of a single disciplinary field, for which he has long been admired. As Jeremy Adelman described it, *Exit, Voice, and Loyalty* was "a hyphen linking an 'early' Hirschman concerned with economic development in Latin America to a 'later' Hirschman working from a broadened palette" (Adelman 2013, p. 442).

A second explanation for the apparent neglect of the "early" Hirschman focuses instead on the limits of Hirschman's development thought. Although they recognize the importance of his intuitions on development issues, several commentators have also highlighted the fundamental failure of Hirschman's broader intellectual project, that is, to design a new economic analysis for the less developed countries that was conceptually different, in a number of crucial respects, from mainstream economics—which Hirschman called the rejection of "monoeconomics".

This was, for example, Paul Krugman's criticism, who recuperated many analyses of early development economists to shape the "new economic geography" of the 1990s, but at the same time criticized Hirschman and his colleagues for their inability to transform intuitions into formal models. As Krugman put it, "During the years when high development theory flourished, the leading practitioners failed to turn their intuitive insights into clear-cut models that could serve as the core of an enduring discipline" (Krugman 1993, p. 26; see also Krugman 1994).

In this paper, I have a twofold goal. First, I plan to discuss how Hirschman navigated the problems of development economics as they unfolded in the 1950s and the 1960s. Actually, Hirschman was well aware of the limits of the field he had contributed to establishing. He saw those limits, however, not as much in the lack of formal modelization, as in the excessively narrow limits of much development analysis and policies—hence, his conviction that unbalanced processes should be

brought back into the picture (actually, to the center of it); that the study of political reform should become an essential element of the study of economic change; and that project evaluation should be broadened to consider side effects, as well as direct social and political consequences.

Like many of his colleagues, Hirschman later recognized the demise of development economics as a distinct disciplinary field (Hirschman 1981). As we will see, however, if it is true that the broader goal of rejecting mono-economics failed, it was because of deep changes in the institutional and international scenario. Insofar as the lack of formal models can be considered an explanation for the marginalization of Hirschman's work, as it is indeed possible to argue for the case of *Development Projects Observed*, the question should nonetheless be framed as a matter of how successfully to embed routines for the production of knowledge within an organization, rather than of how to write a mathematical model. In other words, Hirschman's was a problem of institutional culture, not of economic language (although obviously the two overlap).

My second goal is to examine what remains valid in Hirschman's development analyses, despite the demise of development economics as a distinctive discipline. First, as I will argue, important strands of current development thinking—which, though no longer organized in a distinct subdiscipline, is still very much alive—are recuperating the main messages of Hirschman's work. Second, at a broader level, Hirschman development studies remain an important example of methodological lucidity for the social science in general, in particular for that fundamental question that is comprised under the rubric of “theories of social change”.

A most fertile feature of Hirschman's contribution is his reliance on detailed historical analysis. This may seem somehow counterintuitive, considering the increasing fashion for modelling in the postwar era. And yet, this apparent discrepancy disappears when one considers that, for Hirschman, historical analysis and abstract stylization complement each other, resulting in a rather peculiar marriage of theoretical discussion and context-specific study. Historical analysis, in

Hirschman's hands, makes it possible to tailor models to specific situations and make these situations intelligible. At times, moreover, historical analysis constitutes the primary element from which to extract mid-level generalizations that help explain political and cultural regularities and processes of change. Because of this attention to Hirschman's use of historical analysis, I will give relatively less importance to Hirschman's highly acclaimed theoretical development masterpiece (*Strategy*), and will focus instead on his subsequent works: the praised-but-neglected *Journeys*, and the altogether unsuccessful (but long selling!) *Development Projects Observed*.

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Hirschman's formative years in development were as momentous as they were serendipitous. In 1952, when he moved to Colombia as an economic expert for the World Bank, he was, in his own words, "without any prior knowledge of, or reading about, economic development".³ He joined a small group of foreign experts who had lived in Colombia for many years and had been part of a groundbreaking World Bank mission to that country in 1949. The mission produced a voluminous report on the social and economic conditions of Colombia, which recommended a comprehensive development plan to lift the country from the vicious circle of poverty in which it seemed to be locked. The idea, in a nutshell, was that "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 other sector unaffected . . . Poverty, ill health, ignorance, lack of ambition, low productivity are not only concomitants—they actually reinforce and perpetuate one

³ Albert O. Hirschman, "A Dissenter's Confession: 'The Strategy of Economic Development' Revisited", in Gerald M. Meier and Dudley Seers (eds.), *Pioneers in Development*, New York: Oxford University Press, 1984, pp. 87-111, quotation from p. 88.

another” (Currie 1950). During his Colombian years, Hirschman felt increasingly at variance with this comprehensive approach—by then known as “balanced-growth approach”—mainly because he had serious doubts that it could actually work in practice.⁴ In 1958, he waged a frontal attack against it in his *The Strategy of Economic Development*. “Development”, Hirschman wrote, “presumably means the process of change of one type of economy into some other more advanced type”, whereas “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”. The balanced growth theory seemed incapable of explaining the evolutionary process of economic growth, and thus, Hirschman concluded, it “fails as a theory of *development*”.⁵

Hirschman’s focus was instead on what enables change. He highlighted what he called the “inducement mechanisms” and the “linkages” that can help entrepreneurship and growth spread from one sector of the economy to another. Importantly, Hirschman highlighted the positive role that intersectoral and interregional unbalances can have as growth-inducing mechanisms—hence the term of “unbalanced-growth” for this kind of analysis.⁶ He was not searching for the missing ingredients of a well-balanced development recipe, but, as he put it, for “resources and abilities that are hidden, scattered, or badly utilized”.⁷ In other

⁴ For a discussion of the World Bank 1949 mission to Colombia, Hirschman’s Colombian years, and early development economics, see Alacevich 2009. For an analysis of the whole group of early World Bank “general survey missions” and the broader development debate, see Alacevich 2011a.

⁵ Albert O. Hirschman, *The Strategy of Economic Development*, New Haven: Yale University Press, 1958, pp. 51-2, emphasis in the original.

⁶ Hirschman was not the only critic of the balanced-growth, nor the only proponent of the new unbalanced-growth approach. In 1958, Paul P. Streeten claimed very much the same as Hirschman’s; see Streeten 1958.

⁷ Albert O. Hirschman, *The Strategy of Economic Development*, New Haven: Yale University Press, 1958, p. 5.

words, this was not as much about transferring financial resources to a country, as it was about mobilizing resources already available but hidden.

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Strategy was hailed by many as a refreshing change of perspective in the early development debate, and even critics could not dismiss it. The book became one of those rare publications that have the power to frame a whole debate.

Whereas the balanced- vs. unbalanced-growth debate that Hirschman so insightfully contributed to shape was central to the *theory* of development economics, its influence on the *practice* of development policies, however, is less obvious. In fact, applied development policies in the field were not as mutually incompatible as the theories from which they arose: as it happens, supporters of balanced-growth reasoned in terms of sequences of development and inducement mechanisms, which we would now consider a quintessentially “Hirschmanian” perspective. And Hirschman, for his part, was not monolithically critical of comprehensive plans: in his subsequent book, *Journeys toward Progress*, he underscored the usefulness of large, comprehensive plans to “smuggle” reformist policies through the legislative process.

In practical terms, Hirschman’s heterodoxy appeared to be less radical than it was in theory. 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”. As Sen concluded, “The

‘balanced’ and the ‘unbalanced’ growth doctrines have a considerable amount of common ground” (Sen 1960, pp. 591-592).⁸

As I have argued elsewhere, the heat of the debate was due not as much to simple intellectual or policy disagreements, as to the fact that development economics was still a young discipline. The conflict, thus, revolved around its very identity, specifically around which doctrine would be recognized as its theoretical kernel—hence Hirschman’s accusation that the balanced-growth approach failed “as a theory of development”. Students of social sciences highlighted how these controversies reproduce typical rules of engagement of social conflict. As sociologist Robert K. Merton once noticed, “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], pp. 55–56).⁹

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As it happens, once the pioneering phase of a disciplinary field is over, theoretical diatribes also diminish. After the publication of *Strategy*, Hirschman himself quickly became less interested in high theoretical debates, shifting instead his focus to in-depth analysis of specific, highly contextualized processes of change. Hirschman’s second and third installments of his “development trilogy”—as he once called it—

⁸ Amartya K. Sen, “Review of *The Strategy of Economic Development*, by A. O. Hirschman; *The Struggle for a Higher Standard of Living: The Problems of the Underdeveloped Countries*, by W. Brand; and *Public Enterprise and Economic Development*, by A. H. Hanson”, *Economic Journal*, 70 (September 1960), pp. 590-4, quotation from pp. 591-2.

⁹ For a thorough discussion of the early debates in development economics see Alacevich 2011b.

were strongly grounded in deep historical and context-specific analysis.¹⁰ How can we explain this shift?

After almost twenty 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. 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 to, 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. 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.¹¹

¹⁰ See Hirschman 1994, p. vii.

¹¹ The Brookings Institution, Division of Foreign Policy Studies, “Proposal for a Program of Studies Dealing with American Foreign Assistance”, April 1964 (Revised), p. 5, Projects and Studies -

Building on the methodological lesson of his former colleague at Berkeley, the economic historian Alexander Gerschenkron, Hirschman was among the first development scholars to recognize 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 “late comers”), far from replicating the English model, had followed idiosyncratic paths of industrialization 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 clear also for the “very late comers” of the mid-twentieth century: no “one size fits all” recipe or theory of development could provide all answers to the problems of underdeveloped countries.

Hirschman was also critical of the increasing appeal that revolution seemed to have on Latin American intellectuals and policy-makers in search of a way to overcome social and economic underdevelopment and inequality. Hirschman considered huge, over-arching planning and revolutionary solutions as two sides of the same (flawed) coin: 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:

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.

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).

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, pp. vii-viii).

Judith Tendler's book was a fine example of this new approach.¹² 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". Through a historical reconstruction of policy debates in Brazil, Tendler's book investigated how the "technological configuration" of an economic activity shaped opportunities for the country's economic development (Tendler 1968, p. 6; see also Tendler 1965).

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¹² For other examples, see Weaver 1968 and Echeverría 1969.

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 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 1963, p. 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", claimed Hirschman, "was to scrutinize the record of a few specific, documented, protracted, significant policy problems" (Hirschman 1963, p. 2).

However idiosyncratic the record of specific problems was, 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 reveal his awareness of the difficulties of generalizing 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 1963, p. 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 felt 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 less developed 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 specifically the Latin American style of problem-solving and policy-making. Hirschman had discussed 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 analyze 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, p. 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 that lacked 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 1963, p. 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 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 decisions.

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 spinoffs. 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, to less than successful epilogues, had an energizing effect on policy-makers: a selective amnesia on previous failures made new attempts possible.

One must recognize that Hirschman’s optimism seemed at times excessive, for the two effects of the rhetoric of failure and the rhetoric of success might as well produce outright reactionary results. And yet, there was a method in this optimism.

In the end, Hirschman's 1963 book 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 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 lasting 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 were content to invoke 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 to 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 1963, p. 256).

Indeed, Hirschman's goal was "to show how elements of both reform and revolution are present in the sequences of policy-making" which he had studied (Hirschman 1963, p. 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 1963, p. 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 on 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 mere role of “pre-condition” to economic growth, Hirschman was determined to show how processes of decision-making and problem-solving play a decisive role in all phases of a process 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 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, p. 158).

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The research that resulted in Hirschman's 1967 book, *Development Projects Observed*, rested on the same methodological approach of *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, p. 3). The book was the result of Hirschman's collaboration with the World Bank, 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".¹³ An additional purpose, after two books exclusively based on Latin American cases, was for Hirschman to broaden his expertise to Asia and Africa. Hirschman selected thirteen 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".¹⁴ This was all the more important, since the World Bank had not yet established an internal and independent evaluation function.

¹³ 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 more in detail in Alacevich 2014. For a discussion of the birth of an evaluation function at the Bank and of Hirschman's role (or lack thereof) in this process, see Alacevich 2014; 2015; and 2017.

¹⁴ Dragoslav Avramovic to Department Heads, IBRD and IFC, "Investment in Developing Countries – Effects, Expectations and Reality", February 18, 1964, World Bank Hirschman Folders, Vol. 1, WBGA, italics in the original.

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”.¹⁵

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, say, infrastructure vs. agricultural and industrial projects, or human vs. 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”.¹⁶ Among these determinants, Hirschman listed the existence of visible linkages between 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 expose instead 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.¹⁷

¹⁵ Albert O. Hirschman, “A Study of Selected World Bank Projects – Some Interim Observations,” August 1965, World Bank Hirschman Folders, Vol. 1, WBGA.

¹⁶ *Ibid.*

¹⁷ *Ibid.*

It is no wonder that Bank staff, initially delighted when Hirschman started his study, was instead upset when those first observations were circulated. The final outcome of Hirschman's research, his 1967 book *Development Projects Observed*, did not help 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, p. 161). What was the difference, for instance, between a highway project and a railway one? 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, p. 162). An apparently secondary effect, in other words, may become a decisive element for the future of transportation policies in a given country.

Cost-benefit analysis, which in those years was becoming increasingly fashionable, seemed to Hirschman 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. "How could it be expected," he wondered, "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, p. 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, p. 186,

emphasis in the original). Uncertainties and latitude (if 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 appraisal exercise.

As interesting and stimulating as Hirschman's approach was, it offered few elements for World Bank officers to make project appraisal operational. In particular, it was difficult for them to see how Hirschman's analysis could be embedded in the organizational routines 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".¹⁸

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 "failure of several of the earlier ideas as *practical* policy solutions," as Tony Killick put it (Killick 1978, p. 27, italics in the original), prompted Hirschman's detailed examination of the mechanisms of economic policy-making in

¹⁸ Richard H. Demuth to Mr. Robert E. Asher, September 13, 1966, World Bank Hirschman Folders, Vol. 2, WBGA.

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.¹⁹

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 for the Organisation for Economic Co-operation and Development (1968; 1974) and Partha Dasgupta, Steven Marglin and Amartya Sen for the United Nations Industrial Development Organization (1972). Although these studies did not ignore the role of uncertainty in project design 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,

¹⁹ Herman G. van der Tak to Professor Albert O. Hirschman, December 20, 1966, World Bank Hirschman Folders, Vol. 2, WBGA; Herman G. van der Tak to Professor Albert O. Hirschman, January 18, World Bank Hirschman Folders, Vol. 2, WBGA; Albert O. Hirschman to Mr. Alexander Stevenson, May 1, 1967, World Bank Hirschman Folders, Vol. 2, WBGA; Alexander Stevenson to Professor Albert O. Hirschman, May 18, 1967, World Bank Hirschman Folders, Vol. 2, WBGA.

then, in acquiring a feeling for these doses” (Hirschman and Lindblom 1971 [1962], pp. 83-84).

Hirschman, in sum, recognized that the uncertainty intrinsic in the nature of development projects constituted an unavoidable question, to be addressed on a case by case basis. Accordingly, he refrained from recommending all-encompassing appraisal criteria. Rather, as he put it, his attempt was “to provide project planners and operators with a large set of glasses with which to discern probable 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, p. 186).

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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 field of social science. 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 1981 [1979], pp. 98-99).

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 1981 [1979], p. 99), but his own work was similarly exposed to this kind of critique, especially that celebration of reformmongering 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). The U.S. 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”.²⁰

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²⁰ Lincoln Gordon to Albert O. Hirschman, September 3, 1963, quoted in Adelman 2013, p. 380.

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 of *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, not only was Hirschman a pioneer of postwar development studies, he has remained an influential thinker for development matters, if not in any particularly structured way—certainly not in a “school”—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. Hirschman's contribution to the definition of this discipline's specific identity is interesting, if biased. Development economics, he wrote, was defined by two connected claims. The first was the rejection of mono-economics. According to Hirschman, 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 1981, p. 3).²¹

In recent years, a prominent development scholar, Dani Rodrik, argued instead that what matters is not the different theoretical constructs for 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 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, p. 5).²²

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, pp. 27-28) has emphasized its being

²¹ 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 less developed countries.

²² For Hirschman's affinity to other scholars in the development field, see Ellerman 2001a and 2001b.

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.²³

A central element of Hirschman’s approach, of which current development scholarship is only partially aware, is the importance of historical perspective. We have noticed the crucial role of detailed historical analysis in *Journeys and Development Projects Observed*, and Hirschman’s interest in reconstructing the “personal profiles” of specific projects or questions. But Hirschman’s historical sensitivity was evident even in his most theoretical works. Sequences, inducement mechanisms, linkages, are elements of 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 pre-ordained 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], pp. 171-172).²⁴

²³ In addition, it should be noticed that Hirschman’s approach has always had a strong influence on the Latin American development discourse (Adelman 2008; Ocampo 2008).

²⁴ 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, p. 67).

Hirschman's plea in favor of historical analysis against the structuralist position is related to a broader methodological discussion that periodically emerges about the analysis of the processes of social change. As Raymond Boudon observed, the inference of "laws of development" from specific analyses and comparative studies, far from enriching our understanding of the process of social change, in fact impoverishes it, as "disorder" is unduly expunged from the analysis (Boudon 1986). As Boudon writes, it would be more useful to admit that "alleged *laws* are actually *ideal models* that are prone to appear roughly realized only in *specific* cases without it being possible to define in any precise way the boundaries of their realm of validity" (Boudon 1986, p. 105 of the Italian translation; emphasis in the original). Exogenous factors occupy too important a place in the processes of social change to be neglected. Hence, Hirschman relied heavily on historical analysis and had little patience for strictly endogenous explanations of economic underdevelopment, such as the vicious circle of poverty. All too often, these strictly endogenous explanations of underdevelopment called for strictly exogenous solutions, such as foreign aid designed, explained to locals, and implemented, by foreign experts.

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This brings us to a final question I would like to address in the conclusion of this paper, that is, the moral dimension of development economics as it emerges from Hirschman's writings as a development economist. To be sure, in his earlier work only rarely did Hirschman address this subject, and, even in those rare cases, only cursorily. His observations on morality and the social sciences belong to a later period. And yet, Hirschman's approach had, and still retains, an important moral stance as far as development policies are concerned.²⁵

²⁵ The following paragraphs are slightly revised excerpts from Alacevich 2015, pp. 187-190.

In the conclusions of *Development Projects Observed*, as we have seen, Hirschman insisted on the “uniqueness” of each project and the impossibility of detecting a set of criteria uniformly applicable to all projects. Years later, he admitted that his “Principle of the Hiding Hand”—which he had originally called the “Theory of Providential Ignorance”—could not be, strictly speaking, operationally useful.²⁶ In fact, Hirschman’s goal was much more ambitious than preparing a textbook on project appraisal. Rather, he tried to include uncertainty and limited rationality in the World Bank’s epistemology. By choosing this difficult path, he condemned his book to irrelevance, but he went to the core of a question that is central in today’s debates on international organizations and foreign aid: development knowledge is intrinsically uncertain, and “standard” solutions, especially if based on the experience of “advanced” countries, often produce disappointing results.

The task of development agencies is different from that of other bureaucracies (that is, bureaucracies not focused on development) because of the many limits, in terms of knowledge, imposed on them. Judith Tendler has vividly summarized these kinds of difficulties for a development agency:

Like other public works entities, for example, [a development agency] has to spend money on highways and housing. But the typical highway or low-income housing project it finances will turn out wrong if done “by the book.” The highway will fall apart long before the end of its service life because of lack of maintenance and vehicle load control practices in the aid-recipient country. Poor people may move into the housing project only at gunpoint, because that way of living seems so alien. Yet the [development] agency is held accountable if the highway falls apart or if the housing project remains unoccupied. At the same time, the agency may be just as censured for using procedures that strayed, in the interest of innovation, from standard technical norms (Tendler 1975, p. 9).

²⁶ See Hirschman 1994, p. xvii.

In other words, the information is often scant, previous experience only partially useful, and routine responses may not work as well as for other organizations. Well established problem-solving techniques may prove insufficient when exported to less-developed countries, and the challenges faced by a development agency are often of a novel type and need first to be properly understood. “Development assistance,” Tandler concludes, “involves not only ‘doing.’ An essential portion of it has to do with learning” (Tandler 1975, p. 9).

The implications of this are far-reaching. Development agencies and the developing countries they seek to assist have much more in common than is usually understood. Furthermore, as Hirschman often remarked in his writings, foreign “experts” often lack the contextual knowledge and the sense of what works and what doesn’t that locals have. This is why Hirschman, in the first report he circulated at the Bank after returning from his travels, criticized “the air of pat certainty” of the Bank’s project prospects. Yet, development agencies build their authority and reputation on their supposed expertise in the matters of development assistance (and this is why, after Hirschman circulated his report, feedback notes by World Bank staff were in many cases more resentful than usual). Development agencies transfer financial assets, but in recent years they have also increasingly perceived themselves as repositories of knowledge. At a time when bilateral programs and sovereign funds are reducing the relative financial importance of multilateral banks, these development agencies insist on knowledge as their core business—and, in fact, recent World Bank presidents have presented their institution as a “Knowledge Bank” or, more recently, as a “Solutions Bank.”

There is thus an intrinsic tension between the role of development agencies in development assistance and the nature of the knowledge available for this task. Knowledge capital is different from financial capital: it is not stock that can be simply transferred from country to country or from a development agency to a client country. The uncertainties intrinsic to the development task and the unique characteristics of each project make development knowledge both an input to *and* a

product of the task itself. Hirschman's insistence on the role of the uncertain and the unexpected, and on the importance of historical and local knowledge, are not only useful insights for scholars and practitioners. They are a reminder that a humble and dubitative attitude toward development knowledge is a constitutive element of that knowledge itself.

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