

**THE IRRELEVANCE OF METHODOLOGY AND  
THE ART OF THE POSSIBLE:  
READING SEN AND HIRSCHMAN**

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## **ABSTRACT**

Economic methodologists have observed that economists do not practice what they think their methodology is. Two positions follow from this. One insists on the need for 'better' practice in maintaining 'scientific' standard, while the other takes the literary turn. Following the second route we argue that appraisal of economic theories cannot be done by applying a general 'scientific method' apart from practice. Methodological conversations, which are shaped by various strategies taken by practitioners to persuade each other, can only be studied and improved by reading the most persuasive of the authors in the discipline. Writings of Albert Hirschman and Amartya Sen are chosen to be read following our approach.

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## Introduction

It now sounds odd that methodological debates within the economics profession in fact originated from a naive notion that methodology would tell us how to do economics *scientifically*. In the good old days of ‘economic science’, philosophers of science were believed to hold the key to how to do economics, even though certain groups outside of the mainstream economics (eg. Marxists, Austrians, Institutionalists) were seen following their respective methodological approaches that shared the view that methodology would provide a framework for launching fundamental critique of mainstream practice. What is common among the practitioners both within and outside of the mainstream is that both the groups have so far viewed methodology as offering a set of prescriptions concerning legitimate (‘scientific’ or ‘social-scientific’) practice. For them, the enterprise of methodology is therefore essentially normative, and based on philosophers’ attempts to justify knowledge claims. It is this notion of methodology that is critically reflected upon in this paper. From the title it may appear that our intention is to further strengthen the widely-held position that denigrates any explicit methodological analysis and commentary within contemporary mainstream economics<sup>1</sup>. It will become evident that this is not what ‘irrelevance of methodology’ is supposed to convey.

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1 It is often said that those who can, do, while those who cannot, discuss methodology. The whole enterprise of methodology is seen by many as something that can only divert attention from the real task of a social scientist, namely to solve substantial problems. Interestingly, many of the practising economists in India, who are ever so ready to dismiss contemporary western mainstream economics, share this view with the latter.

## **Methodology (with capital ‘M’)**

We begin with a useful distinction between two senses in which the word methodology may be used<sup>2</sup>. By Methodology (with capital ‘M’) we shall mean the normative enterprise of judging theories from a meta-theoretic position. Economists, who believe that claims about economic knowledge have no less epistemic status than claims about physical or biological phenomena, are positivists in the sense that they believe there is a universal framework in which all ‘scientific’ knowledge claims may be justified. This view of Methodology is quite different from the methodological (with small ‘m’) discourse that practising economists often engage in. Economists do not only construct models and conduct empirical tests, they also argue on what a good model should look like (Klamer, 1983, p238). These small methodological conversations lead to acceptance of certain arguments and rejection of others. But what is important is that it is impossible to know beforehand which arguments will prevail in the end.

While small methodological conversations are what economists do in seminars and conferences, Methodological pronouncements do not seem to have had much influence on what economists do. In this respect economists of mainstream variety are no less guilty than their friends in the ‘soft’ discipline of development economics. As a matter of fact, much of the interesting and influential work of economists would have to be rejected if we passed judgements on the basis of what economists believe their Methodology is<sup>3</sup>. On this point almost all the

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2 This is commonly shared by postpositivist writers like McCloskey, Klamer, Weintraub and others. This section has drawn heavily on Weintraub (1990).

3 Mark Blaug (1988) examines the work of John. R. Hicks with the aim of discovering whether a coherent Methodological position informs Hicks’s writings. Blaug is unable to find any coherent Methodological stance in Hicks’s work.

scholars of Methodology, whose positions are otherwise diametrically opposed to each other, would agree. Mark Blaug, who still argues in favour of falsificationism as the Methodological standard, observes that “economists fail consistently to practise what they preach”. Following this observation, while Blaug suggests that economists should try hard to satisfy the Methodological standard, others find problems inherent in falsificationist Methodology itself. In this paper our purpose is not to replace one Methodology with another. We rather ask the question: How sensible is it to hold on to a Methodology that prescribes a universal standard for assessing the merits of particular theories? Even when practising economists are seen engaged in methodological (with small ‘m’) conversations, can we really set a standard of conversation by any meta-theoretic consideration? In what follows, a brief account of the evolution of methodological thinking in economics is presented with particular emphasis on recent trends against foundationalist thinking, and then we make an attempt to articulate the view that appraisal of economic practice is a complex process of questioning and interpreting that cannot be based on any general “scientific method”. We argue that the requirement that arguments ought to follow certain fixed Methodological principles is rather misconstrued. We then put forward the position that methodological conversations, which are shaped by various strategies taken by practitioners to persuade each other, can only be studied and improved by reading the most persuasive of the authors in the discipline. The purpose here is not to give another clarion call for an integrated social science. What is more important is to take a stock of the building blocks that have already been assembled by practitioners with a variety of motivations. This brings us close to the approaches of economists like Albert Hirschman and Amartya Sen. As there is no sure foundation which would provide us a precise judgement about the worth of a particular piece of work in economics, it would be worthwhile to

study what economists actually do and not what they should do. The rich variety of actual practices of economists can be appreciated by taking a close look at some of the best practices. We undertake a study of two economists, Albert Hirschman and Amartya Sen, who have been quite successful in attracting and influencing their audiences. In particular, we attempt to analyse and compare their forms of argument which have resulted in the different kinds of success that these two writers on development have achieved, as far as persuasion is concerned.

### **What mainstream economists believe their Methodology is:**

The philosopher who has had the greatest influence on the Methodology of economics is Karl Popper. He is often invoked by economic Methodologists such as Mark Blaug and Terence Hutchison<sup>4</sup>. Popper's philosophy even influenced a major introductory textbook, Richard Lipsey's *An Introduction to Positive Economics*.

Throughout his work Popper has been concerned with what he calls "the problem of demarcation", that is, the problem of distinguishing science from non-science. Popper's answer to the problem of demarcation is that scientific theories are *falsifiable*. A statement is in principle falsifiable if it is logically inconsistent with some finite set of true or false observation reports. In other words, a theory is falsifiable if it is not guaranteed that it will pass all tests. There must be some possible tests or observations which, if obtained, would be evidence that the theory is false. Popper himself gives an example of a scientific statement: "All swans are white". This is a falsifiable statement since the observation of a non-white swan would establish its falsity (Popper, 1968, p27). Popper further refines this basic idea by distinguishing a class of statements which he calls "basic statements". Basic statements are such that we easily agree upon the truth status of these statements.

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4 Blaug (1980, 1992), Hutchison (1938, 1965).

Popper also emphasises an “asymmetry between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements” (Popper, *ibid*). A universal statement concerning an unbounded domain may be falsifiable but it will not be verifiable. That is, a universal statement is not deducible from any finite set of basic statements. For example, the basic statement “this swan is black” falsifies the universal statement “all swans are white”. But no set of observation reports verifies that “all swans are white” or “all swans are black”. It is not possible to verify any truly universal statement, but one can falsify it or verify its negation. (Hausman, 1992, Ch 10).

Blaug (1992) claims that

“modern economists do in fact subscribe to the methodology of falsificationism: despite some differences of opinion, particularly about the direct testing of fundamental assumptions, mainstream economists refuse to take any economic theory seriously if it does not venture to make definite predictions about economic events, and they ultimately judge economic theories in terms of their success in making accurate predictions” (p xiii).

At the same time he is critical of what economists actually do as distinct from what they say they do. Economists, according to him, pay lip service to notions like empirical content and falsifiability and deductive testing but do not actually put the Popperian rules for strenuous testing into practice (*ibid*). In other words, Blaug uses Popper’s Methodological rules to criticise economic practice.

But why Popper? To many economic methodologists, it is Popper who could salvage the ‘economic science’ by exposing the self-indulgence of economists who cared little about the empirical world. This motivated Terence Hutchison in the 1930s to bring Popper’s

demarcation criterion to the attention of economists. Hutchison's purpose was to show how much of what then passed for economic analysis was tautology masquerading as substantive propositions (de Marchi, 1988, p7).

These Methodological pronouncements apart, Popper constitutes a genuine puzzle for economic Methodologists. Popper's ideas are often referred to in disputes on what should be considered as "right practice", yet in actual practice deviations are the rule.

### **Why is falsificationism not practised?**

If Popper's ideas are of genuine value to economists, one would like to know why they are not being followed in actual practice. Why do economists not practise what they preach? Blaug would say "they don't try hard". His allegations against economic theorists are that they rarely formulate their theories in ways which facilitate testing, they carry out few tests, and they pay little attention to negative results. No sensible methodologist -- Popperian or otherwise -- would deny the fact that one important feature of good science is a serious concern with testing. Why then is testing so unimportant to economists? If it has something to do with the sociology of scientific practice one has to analyse the lapse in sociological terms. Although Methodologists like A. W. Coats have advocated that route, others have responded to the puzzle in a variety of ways.

Daniel Hausman criticises the "methodological schizophrenia that is characteristic of contemporary economics, whereby methodological doctrine and practice regularly contradict one another" (Hausman, 1992). But he provides a different answer to this puzzle. According to him, this schizophrenia is a symptom of the unsound philosophical premises underlying contemporary economic methodology. Hausman points out a number of difficulties with Popper's falsificationism as applied to



science. Even if it was free of these difficulties they would be of little or no value to economists. One can argue that the fact that economic theories are not logically falsifiable by themselves does not constitute a good argument against falsificationism because they can be incorporated into logically falsifiable test systems. But the same argument holds for theories of practically all disciplines, no matter how ‘unscientific’ they appear to be. If one wants to, there is little difficulty in taking any statement to be conveniently falsifiable (Hausman, 1988). Besides, economists are so little involved with testing because, either many are involved with non-empirical conceptual work, or, given the subject matter they deal with, they do not know enough to formulate good tests or to interpret the results of tests. It may be possible with discovery of better experimental techniques and more detailed knowledge (Hausman, 1992). This line of argument definitely shakes falsificationism to its roots and opens up the possibility of a plural approach.

### **Postpositivist tendencies**

Bruce Caldwell, in his *Beyond Positivism*, asks: How are we to do methodological work in the postpositivist era? He claims that falsificationism cannot be a recommendable Methodology since its structures are extremely demanding. What he advocates instead is “methodological pluralism”. He believes that the primary purpose of methodological work in economics is to enhance our *understanding* of what economic science is all about and, by so doing, to *improve* it (Caldwell, 1988). The critical appraisal of theories plays an essential role in methodological pluralism. But criticism is not undertaken for the purposes of either discovering or applying some universal criterion of demarcation. The role of the methodologist, according to Caldwell, is to discover the strengths and weaknesses of research programmes. This is of course a different endeavour from searching for a universal

norm for demarcation and goes beyond what is known as positivist epistemology. Against the advice of economists like Caldwell, who have paid attention to the history of philosophy over the last four decades or so, economists in general still use the positivist way of arguing. Interestingly, a naive positivism seems to have given economists the strength to carry on. But times are changing, as the editors of a volume titled *The Consequences of Economic Rhetoric* have noted. According to them, positivism is losing its grip on the collective consciousness of economists (Klamer, McCloskey and Solow, 1988). Now the economists are becoming interested in their language and their rhetorical devices. It now seems that, instead of making Methodological pronouncements, economists can do well if they pay attention to those scholars who have spent their professional lives thinking, writing, and talking about the subject.

The implications of postpositivist ideas in the Methodology of economics have been traced by McCloskey, Klamer, and others. McCloskey calls attention to the kind of positivism that informs appraisal of economic practices. McCloskey shows that the way economic theory and econometrics “work” is through the use of particular rhetorical strategies. For example, McCloskey points out that John Muth’s 1961 article on rational expectations, although written in a formalistic language, could be translated into simple nonscientific prose<sup>5</sup>. By this McCloskey

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5 I have a slight disagreement with McCloskey on this point. While it is true that some arguments can be translated from formal into non-formal language with little alteration of the substantive content, this is not always the case. Formalisation of a theory generally involves isolating some aspects of the theory to produce a highly idealised representation of it. In this process, the ambiguity of natural language gives way to clarity and precision, which is a good thing, but these qualities are gained only at the cost of losing much of the richness of natural language. Thus translation from one language to another is not without consequence -- it involves changes in the degrees of precision and richness. Thanks to Professor Krishnaji for pointing this out to me.

does not mean that formal language is useless. The scientific metaphors Muth uses are indeed necessary for persuading other economists of the importance of his arguments. For a serious rhetorician, scientific metaphors that the mainstream economists extensively use are not mere frills -- they constitute a serious figure of argument.

Before we move on, it may be worthwhile to clarify certain ambiguities present in the writings of researchers in economics, who seem to have been positively influenced by postmodernist thinking in other social science disciplines. Since most of them are trained primarily in quantitative techniques, they tend to think that the so-called 'qualitative' or ethnographic research methods would free their discipline from the shackles of positivism<sup>6</sup>. While it is true that the grip of positivism over other social science disciplines is fairly weak, and it is also true that the qualitative research methods are the most favourite ones among the practitioners in these disciplines, there is no necessary connection between the two. Qualitative research methods can be made consistent with positivist epistemology. A postpositivist position would instead see the two approaches as different means of persuasion, without making any claim of superiority of one over the other. Adding qualitative techniques to an economist's box of tools may produce more persuasive arguments; or it may not -- it all depends. The point that we are making is that it is impossible to know beforehand which methods are going to prove effective in persuading the members of the community of economists. This necessitates the study of their actual practice -- the study of how they actually persuade each other and the world. Appraisal of existing economic literature thus becomes an activity similar to literary criticism. Criticising economics in the literary sense of 'criticism' does

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6 This line of argument is presented in Shome et al (1996), and its refutation can be found in Chakraborty (1996) on which this paragraph is based.

not necessarily mean passing judgements on the 'truth value' of a particular piece of work. A criticism is a reading.

This literary turn in criticism of economic writings has transformed the way we used to think about economic Methodology. Now we are able to see that the logical structure of economic theories, criteria of appraisal or objects of study in conventional economic Methodology, are subsumed in a more comprehensive interpretive framework. Logical expressions and "facts" are only two of the many rhetorical devices that characterise economic discourse. Other rhetorical devices such as metaphor or "story" could as well be used to persuade the audience. McCloskey has gone a long way in pointing out the ubiquity of metaphors in economics. However, we still have much to learn about how the complex composition of arguments that the most persuasive of the authors employ to persuade others.

We take a slightly different route from McCloskey's. We view scholars in a particular way in this approach. They are seen essentially as social creatures whose aim is to live in a community with others. The boundaries of a typical community are in constant flux. An individual member of a community always experiences a tension between two contradictory motives -- solidarity within the community and a desire to transcend the boundaries. It is quite easy to see why a member of a certain community of scholars might feel indignant if a criticism comes from another community<sup>7</sup>. We feel that assessment of arguments cannot be made unless we confront the contexts as particulars. And they will best be confronted by a person who is experienced in that interpretive

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7 Frank Hahn's replies to critiques of general equilibrium theory is a case in point. He seems to be upset by the 'ignorance' that his critics display. He devotes his Cambridge inaugural lecture against Kaldor's earlier essay on the irrelevance of general equilibrium theory. See Hahn (1973) and Kaldor (1972).

community. Does it mean that such critical work can never come from someone who does not belong to that community? Not exactly. The idea of methodological conversation does not mean to promote isolation of one interpretive community from another. But it is important for an ‘outsider’ to get enough understanding of the language of conversation within a community. Conversations in one community can enter in an integral way in an internal critique in another, since knowledge of conversational practices (methodology, that is) in community X by community Y is as much a part of the internal reality of community Y as it is of community X<sup>8</sup>. This awareness is in fact necessary to check the tendency of narrow, temporary agreement among the members of a community to give rise to an immutable Methodological dogma. Both Sen and Hirschman remind us of the importance of guarding us against this tendency in economic practice.

### **Case studies: Albert Hirschman and Amartya Sen**

What is common in both Hirschman and Sen is that although they are anxious to show up inadequacies in existing theories, they are not typically concerned with replacement of these theories with radically different alternatives. However, while Sen sees himself “on the whole as a mainstream economist” (Sen, 1990, p259), trying to make mainstream economics take more note of such problems as poverty, famines, women’s question, etc, Hirschman sees himself as a “maverick who deliberately complicates the economic discourse”. It seems that Sen would more or less agree with Kuhn and others who have argued, it may be necessary for members of a scientific community to proceed as if the dominant paradigm is true until the time is ripe for its replacement.

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In an overall cultural context, Martha Nussbaum and Amartya Sen take a similar position. See Nussbaum, M. and A. Sen (1987).

And this seems to have worked well for him. On the reasons for Sen's 'deep impact', Michel De Vroey points out "it is not irrelevant that his critique of welfarist tradition in social choice theory should come from someone like him *who has also made important positive contributions to the field and who has thereby earned enough credentials to be allowed a critical stance, without being dismissed at once*" (De Vroey, 1991, italics ours). "Positive contributions to the field" is seen as conferring the right to criticise the field itself. This is somewhat close to our central position in this paper, which emphasises the role of what might be called 'internal criticism' or appraisal from within, or methodological conversation. Since, as we have already noted, there is no independent (or meta-theoretic) basis for appraisal, an economic theory or an economic argument is always appraised from within economics.

Hirschman deliberately avoids being identified with any theoretical tradition in economics. Paradoxically, although in general, economists want to know which school of thought someone belongs, resisting a label is also quite common among them. As Klamer points out, a label may sometimes become important in persuasion<sup>9</sup>. Unlike Sen, Hirschman may have suffered from an uncertainty in the community of economists because of his nebulosity in this regard.

Although Sen's own approach to the question of Methodology emphasises "heterogeneity and relevance" (Sen, 1991), it is strongly grounded on the cumulation of mainstream economic practice. While admitting the fact that much of the discontent about the mainstream

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9 According to Klamer, this belongs to the category of "meta-arguments". "Theoretical, empirical, epistemological, philosophical, commonsense arguments -- and still that is not all. Economists use even more arguments to sway their audience and get support for what they have to say. Commonly used tactics are to distinguish their own approach from others and to appropriate history. Meta arguments, the formal expression of such tactics, are statements about economists and their work." (Klamer, 1983, p249).

economic theorising is well grounded, he observes that quite a few critiques seem to take insufficient note of the particular nature of the exercises under attack. Some criticisms are based on an inadequate recognition of the diversity of motivations and concerns underlying different types of economic theory. He criticises the narrowness of prediction-centred Methodological approach on the obvious ground that there are other exercises - for example doing economic evaluation or providing adequate description -- which are as important as predictive theories. Sen makes a further point which is usually overlooked. As far as causal theories are concerned, the need to test them with empirical information is fairly universally accepted in principle by economic theorists. But there is a tendency to view testing as an ultimate rather than an immediate step to be undertaken.

“The complexity of many economic problems requires that conceptual and analytical issues be explored very substantially to understand what types of relationships might be involved. While the analyses *at this stage* are not meant for immediate testing and verification, they can greatly help cognitive assessment of the problems and the relations under examination. A great deal of economic theory involving causal relationships is, in fact, of this kind” (Sen, 1991, p7 ).

Sen’s own ‘entitlement’ approach to analyses of famines (Sen, 1981) can be seen in this light. Sen himself said he was offering “a general framework for analyzing famines rather than one particular hypothesis about their causation” (ibid, p162). De Waal (1990) in his criticism of the entitlement approach labels it as ‘entitlement theory’ and expects it to provide a general theory encompassing all the specific causes of entitlement failures. Since the entitlement approach is not a

model of famine causation or a comprehensive general theory, it is not meant for immediate testing and verification<sup>10</sup>. Sen's elaborate case studies of famines are clearly meant for defending his arguments which he advances to persuade his audience. And he seems to have achieved success in this endeavour even though there is no immediate testable implication (in the strict positivist sense) that follows from his approach. An approach, because of its very nature, cannot be 'tested' in the positivist sense. According to Osmani (1995), this approach-view is clearly presented in *Poverty and Famines*.

Sen's special strength lies in his ability to combine lucidity<sup>11</sup> and rigour. And thus he is able to attract interest far beyond fellow economists. The success of his book *Poverty and Famines*, for example, is not surprising given the appropriate combination of formal language and incisive case studies, even though it would be fair to say that most of Sen's arguments were not really new. His entitlement approach essentially extends the concept of the opportunity set in microeconomics, and the choice of abstract and formalistic style almost naturally follows, given the nature of his intended audience. Terms like 'endowment', 'exchange entitlement', 'E-mapping' are clearly aimed at persuading members of certain language community who are expected to appreciate the nuances involved in certain ways of conceptualising. In discussing such disturbing themes as coexistence of famines and unprecedented food stocks, too much of moral indignation may not be a good rhetorical strategy if universities and donor agencies are one's target audience. By choosing an appropriate style, Sen has no doubt achieved more success than most critics of mainstream economics in influencing audiences.

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10 On this and related issues see Gasper (1993).

11 Lucidity in style, we feel, is a matter of taste, rather than an absolute virtue. Sometimes obscurity plays a pragmatic function in attracting attention of a contemptuous audience.



It is meaningless to ask whether Hirschman is more successful than Sen in influencing others in the discipline. Both of them have made significant contributions to the fields of development and economic theory; and have held some of the most prestigious professorships in the western academic world. However, it would be fair to say that over time Hirschman has somewhat faded from the economics profession's sight, even though his recent writings still continue to bear traces of his skeptical, ironical and incisive cast of mind. His books have all along been praised highly by such eminent writers as Kenneth Arrow, John Galbraith, Charles Lindblom, and the like. At the same time, one does not fail to notice the increasingly formalistic mainstream economists' lack of interest in his writings. This is puzzling, given the admiration and respect he commands from many of the mainstream economists for his 'deep insight'. In what follows, we make an attempt to explain this apparent puzzle by taking a close look at Hirschman's style.

Hirschman's writings are typically characterised by a kind of "playful seriousness", a questioning and amused attitude (McPherson, 1986). As we have already noted, he sees himself as someone who "deliberately complicates economic discourse". "Like any virtue", he notes, "parsimony in theory construction can be overdone and something is sometimes to be gained by making things more complicated". All the 'complications' he wants to introduce in the economic discourse flow from the "complexity of human nature" which is disregarded by traditional theory. He does not, however, forget to add that this disregard was "for very good reasons". This addendum definitely has a pragmatic function in the art of winning over an audience convinced about the virtues of what Hirschman is calling 'traditional theory'. What he suggests is that the complications must be "spoonfed" back into the traditional framework. McPherson(1986) describes Hirschman's work metaphorically as one of "peering around the edges and through the

cracks in social scientific laws, to see what is being overlooked". This feature seems to have pervaded his writings, from his early work on 'unbalanced growth' through his recent intervention where he questions the behavioural foundations of economic theory (Hirschman, 1985). If McPherson's portrayal of Hirschman is correct, it at least partially explains why interest in Hirschman has not been sustained over the years, especially within the mainstream. The irony is that what McPherson views as the most remarkable feature of Hirschman's writing might have actually hindered spread of his influence. Hirschman himself does not quite agree with McPherson's portrayal of him as someone who is primarily interested in underlining what more systematic-minded economists have overlooked. According to him, "skepticism toward other people's claims to spectacular theoretical discoveries is, of course, not a particularly noteworthy trait" (Hirschman, 1994, p278). To be fair, one must admit that Hirschman has developed quite a few theoretical notions of his own, some of which have even found their place in the textbooks on development. But the very specific way that he does economics may be seen as responsible for the lack of interest shown by most of the systematic-minded economists.

Although it appears that both Sen and Hirschman want to strike a middle ground between opposing views, Hirschman is perhaps a great deal more, what might be phrased as 'against method'. When asked about his method, Hirschman replies "I think it is 80 per cent art and perhaps 20 per cent science or whatever you want to call it" (Swedberg, 1990). Hirschman's approach to economic methodology is essentially a logical extension of his approach to development issues. Hirschman differs from most of his fellow development economists, both leftists and rightists, in his position that progress can be achieved without premeditated goals and without prior knowledge of how to achieve these goals (Sanyal, 1994). Hirschman seems to believe that institutions come to define their

goals in a more precise way as a result of engaging in action, often without much knowledge about the possible consequences of their action. In an analogous way, one can argue that communities of scientists come to define their methodological standards as a result of engaging in conversation. In economics, conversations are often overshadowed by an apparent conflict between those who advocate rigorous and parsimonious models of social reality and others who emphasise pluralistic, narrative, or qualitative descriptions. While Sen takes a middle position between the two apparently opposing approaches, Hirschman is often criticised for his “rejection of the drive toward rigour” (Krugman, 1994, p40). In a highly provocative essay Paul Krugman (1994) blames Hirschman and Myrdal for helping precipitate the fall of what he calls “high development theory” that had deep influence on both economists and policy makers in the 1940s and 1950s. Krugman attributes it entirely to their stylistic and methodological stance. He writes,

Until their (i.e. Hirschman’s and Myrdal’s) books appeared, economists doing high development theory were trying to be good mainstream economists. They could not develop full formal models, but they got as close as they could, trying to keep up with the increasingly model-oriented mainstream. Myrdal and Hirschman abandoned this effort, and eventually took stands on principle against any effort to formalize their ideas. (Krugman, 1994, p 47)

Let us reiterate again, we are interested here not so much in what Hirschman wrote, although that certainly is important, but in how he made his ideas persuasive. What kinds of arguments does he prefer to deploy? What comes out of Krugman’s indictment is that Hirschman’s stylistic stance must have played a crucial role in his limited impact on

the mainstream economists. If economists are not persuaded, who is his implied reader then? Hirschman writes,

The fundamental bent of my writings has been to widen the limits of what is or is perceived to be possible, be it at the cost of lowering our ability, real or imaginary, to discern the probable. The nature of these persistent widening attempts -- or what I shall call my "possibilism" -- *varies with the public I am addressing*. (Hirschman, 1971, italics added).

What is to be noted here is that the nature of his possibilism varies with the public he is addressing. Clearly, he wants to maintain what might be called "cultural sensitivity", a sensitivity toward the culture of conversation of a community. But does one see a consistently defined audience in Hirschman's writing? That he is primarily an economist is evident. He has written extensively on the behaviour of firms and the functioning of national economies. The conceptual apparatus he often chooses to explain various issues on development belong largely to the discipline of economics. To a large extent Hirschman's use of the conceptual categories familiar to economists can be explained by his desire to communicate with other economists. As Sanyal observes, Hirschman is "an economist at heart, in that his 'natural' inclination is to explain any social phenomenon by drawing an analogy with the functioning of the market". But his preference for tools familiar to economists does not produce in him any sympathy toward what is pejoratively called "economic imperialism", that is, the practice of using economic tools to explain non-economic phenomena. In *Exit, Voice, and Loyalty*, he makes his position clear against using "the tools of one discipline for the purpose of annexing another". The economist has typically assumed that dissatisfaction with an organisation's product is

met by withdrawal of demand ('exit'), while the political scientist thinks rather of the protests possible within the organisation ('voice'). Hirschman argues that a comparative analysis of these two options and their interplay is likely to enrich both the disciplines.

Sen too is not very sympathetic to the so-called economic models of non-economic behaviour, as epitomised by Gary Becker's work. Both Sen and Hirschman hold more or less similar views on how to make cross-disciplinary communication without "imperialism" of any kind. If Sen takes up the task of building bridges between ethics and economics, Hirschman focuses on the connection between economics and politics in a novel way. In spite of these similarities, it is quite evident that they have very different kinds of impact on the academic community. The difference, we feel, is largely due to their stylistic and methodological preferences.

On a felicitation note Philippe Van Parijs comments,

"Sen is one of the very few people who are able to convey to economists, *in a language they find congenial*, those philosophical insights which they would be naive to ignore in discussing even the most concrete policy questions. He is also one of the very few people who are able to explain to philosophers, *in a language they can understand*, those elements of economic conversation which they would otherwise neglect." (van Parijs, 1990, italics ours).

Sen makes similar attempts elsewhere too. An extensive literature in development economics has grown since the 1970s that draws attention to neglected aspects of development such as quality of life, the fulfillment of basic needs, and so on. But welfare theorists typically sneer at these writings and treat them as nothing more than a bunch of ad-hoc

suggestions. This attitude is perhaps due to their Methodological belief that statements or suggestions should not just appeal to intuitions but also be structured and founded. Sen attributes this gulf between development economists and welfare theorists to the *different modes of arguing* that are used by the respective communities. In this context Sen makes an attempt to bridge another kind of gap, the gap between welfare theory and development economics. Instead of privileging one mode of argument over the other in an essentialist sense, he suggests that we have to compare and contrast the foundational features underlying the concern with quality of life, basic needs etc. with the informational foundations of the more traditional approaches used in welfare economics and moral philosophy, such as utilitarianism (Sen, 1984).

Hirschman observes that economists continue to identify scientific progress with the elimination of “exogenous” factors from their constructs, while political scientists use political categories only to explain political events. He takes up the “decentralised” activity of building bridges between economics and politics. One imagines that this must be an extremely useful way of looking at various social phenomena. But what sets Hirschman apart from Sen is the former’s impatience with the formal language that economists use in models. As a matter of fact, a kind of ambivalence surrounds Hirschman’s attitude toward formal tools in economics. According to the perspectives of internal criticism set forth in the last section, one must take sufficient note of the language of conversation pertaining to existing theories which one attempts to question and modify. Hirschman’s modifications often take the form of a combination of suggestive metaphor, anecdotes and analogies. We are not suggesting that metaphor has less epistemic status than models set in mathematical language. In fact, mathematical models are metaphors, and there is nothing defeatist in it if a modeler says so. What we want to emphasise instead is that in a community of academic economists, who

more or less agree on the persuasiveness of theoretical arguments set in mathematical models because of their repeated use, it is perhaps not a good strategy to use a language which is far removed from the practitioners of “normal science”.

### **Concluding remarks: Towards a more tolerant critical culture**

Rejection of a meta-theoretic perspective in economic methodology is likely to facilitate better communication among practitioners in social sciences in general and economics in particular. In spite of their differences on the need for formalism in economics, both Sen and Hirschman have shown their deep commitment against dogma, and they are remarkable in commanding admiration and respect from their fellow social scientists with a variety of intellectual orientations. “The typical economist”, McCloskey writes, is harshly if unreflectively dogmatic, demanding that students or colleagues be members in good standing of this or that church...Simple-mindedness accentuates dogmatism” (McCloskey, 1994). As Sen notes, “our ability to misrepresent or misunderstand what others are upto is quite striking” (Sen, 1991). Sen’s exquisite courtesy and respect for people who hold different views has in fact proven most productive in defence of his ideas. Separateness of large groups of citizens from one another seems to Hirschman too worrisome. He dramatises his point by saying “each group will at some point ask about the other, in utter puzzlement and often with mutual revulsion, “How did they get to be that way?”” (Hirschman, 1991). It is not rare to see economists using Methodology as a stick to beat up the members of the larger community.

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