

Classes, like Ethnic Groups, Are Imagined Communities: A Response to Rao

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# Classes, Like Ethnic Groups, Are Imagined Communities

## A Response to Rao

Ashutosh Varshney

THE application of class analysis to India's countryside is in need of an intellectual renewal. By repeatedly giving rise to, or participating in, the caste- and religion-based movements, rural India has not only continued to flout the core predictions and propositions of the standard class analysis, but more importantly for our purposes, the concept of class has also undergone serious changes in the last decade and a half. The key challenges to the concept of class, as we used to practise it the 1960s and 1970s, have come from the theories of collective action on the one hand and of ethnicity and nationalism on the other.

Theories of collective action argue that classes consist of individuals – hence it must, at the very least, be demonstrated why individuals would choose to act according to class, not individual, interests. This is an analytic imperative because individual and class interests can often clash.<sup>1</sup> A landless peasant may be hurt by the decision of the landlord to keep wages stagnant, but it does not follow that it is in his interests to join a union or party mobilising agricultural workers. From an individual perspective, class mobilisation can have a serious cost: the peasant may be dismissed altogether by the lord, and a dismissal may incalculably increase his misery which the resources of an agricultural union are typically unable to alleviate. Moreover, and this is the second part of the argument, if the mobilising union did succeed in getting wages raised for the landless class, the peasant would benefit anyway whether or not he participated in the agitation. We may have moral compunctions about such “free-riding” behaviour, but let us not conflate the normative and the empirical. The landless worker has an interest in the benefits of class action, not in its costs, and class benefits, if available, cannot easily be denied to the landless peasant, but costs of class mobilisation must be individually borne. A collective action problem thus exists. Behaviour according to class interest can, and does, take place, but it requires solving the collective action problem.

Given the constraints of space, I will not pursue this line of inquiry further. Wherever required, my book deals with puzzles of collective action in the countryside. I call attention to these points only to indicate that an *assumption* about the identity of class and

individual interests can no longer be maintained in the social sciences.

I would, instead, focus on the second line of reasoning that has emerged from the theories of ethnicity and nation-building. Though the primary purpose of these theories is to show that ethnic and national groups are imagined communities,<sup>2</sup> they have serious implications for the concept of class. Any collectivity that is larger than a village, a neighbourhood or a small organisation is an imagined community for it does not allow face-to-face intimacy. Thus, sectors of the economy and classes, like nations, are also imagined communities.<sup>3</sup> An individual does not “naturally” feel his class; such consciousness depends on political mobilisation, public policy, or other people's behaviour towards that individual. The implication is not that class analysis is irrelevant. All that is required is that classes not be seen as axiomatically central to politics or political economy but demonstrated to be actually so.

J Mohan Rao's vigorous attack<sup>4</sup> on my book, *Democracy, Development and the Countryside: Urban-Rural Struggles in India*<sup>5</sup> (DDC hereafter) is admittedly based on a class perspective, but he is somewhat blithely inattentive to the developments that have posed new questions for the concept of class. His critique is strikingly reminiscent of Ashok Mitra's claims in *Terms of Trade and Class Relations*.<sup>6</sup> The latter was a fine tract for its times, but the times have changed. The neat simplicity of a class-theoretic world, as we used to know it, has been shaken by the resilience of non-class economic and political actors, and by the corresponding theoretical trajectories in the social sciences.

Though Rao's attack on my book is wide-ranging, one can identify, in the order of theoretical importance, four basic disputes.

(i) *Should classes or sectors be the unit of analysis?* Rao agrees that in the 1970s and 1980s the urban-rural divide was a highly charged and visible issue in Indian politics. But he argues that a sectoral construction of politics does not require the sector as a unit of analysis, for India's countryside is internally divided between classes. The sectoral movements, Rao argues, were only apparently sectoral but, in reality, class-biased and against the have-nots. I do conceptualise

internal rural divisions, but primarily in terms of caste and religion which, according to Rao, contradicts my sectoral argument.

(ii) *Are caste and religious identities more basic than class considerations in rural India?* The caste- and religion-based concept of intra-rural divisions, argues Rao, is also wrong. The pull of caste and religious identities, DDC argues, is blocking the further growth of rural, sectoral power in India. A lower versus upper caste construction of politics splits rural India because castes, whether upper or lower, cut across the town and countryside. Large-scale caste mobilisation, thus, undermines sectoral movements, and the same, I argue, is true of religious mobilisation. Class, Rao argues, captures intra-rural divisions better than caste or religion, and should be given primacy.

(iii) *How should one theorise the relationship between class power and state behaviour?* “The first three-quarters of the book”, Rao contends, “holds no surprises” (p 1743), contributing nothing to what we already know about India's agricultural policy. Rao has in mind parts of the book where I make a distinction between power as articulated in society and power as expressed within the state institutions, and explain public policy and state behaviour as an outcome more of the latter, less of the former.

(iv) *Can my return index meaningfully capture returns to farming?* Rao believes that my index for measuring whether farm returns have gone up or down since 1970-71 is seriously flawed.

### CLASSES AND SECTORS

Rao misreads the basic analytic purpose of my book. DDC is not about the internal workings of the countryside. It is about the impact of the countryside on state behaviour/economic policy. That being so, my key questions required “going inside the state”, not going inside the countryside. The latter is relevant only insofar as it is necessary to understand the former, not in and of itself. Why?

The way an analyst slices up the empirical universe depends on what the analyst is asking. For example, my current project deals with Hindu-Muslim relations in six Indian towns, three peaceful and three violent. The key puzzle of the project is: why are Hindus and Muslims able to live peacefully in some towns but not in others? For this question, the Indian state or national politics – which is dissected in DDC – provides only the context within which Hindu-Muslim relations in the various towns (and the selected neighbourhoods within each town) are examined. The book, as a result, has a mode of narration and analysis that is highly town-based. The question posed made going

inside the neighbourhoods and towns necessary.

The sectoral abstraction in DDC is not meant to deny that divisive issues do not exist in rural India. Indeed, as already indicated, a principal argument of the book is that divisive identities of caste and religion cannot easily be overwhelmed by an economic emphasis on urban-rural issues. For *Bharat* (the villages) to exercise more pressure on India (the cities) than it already has, the villagers must increasingly feel that being rural is more important than being a member of caste and religious communities. Rao believes that the very recognition of intra-rural contradictions, even if wrongly conceptualised in terms of caste and religion, invalidates the sectoral category called the countryside.

Do disputes within a nation – between various castes, between language groups, between religious groups, or indeed between classes – mean that the concept of a nation is vacuous? Do contradictions within a class – between, let us say, upper and middle peasantry, or workers in the organised and unorganised sectors – mean that the concept of class is of no use? By themselves, internal dissensions within classes or nations do not make the concept of class or nation irrelevant. They simply complicate the analysis of classes and nations, presenting analytic difficulties that must be encountered but not requiring that the concept be abandoned.

In most empirical research about macro tendencies, large abstractions are unavoidable. Thus, the dispute between Rao and me is in effect about whether, in understanding the urban-rural struggles in India, a sectoral abstraction is more analytically helpful than a class abstraction, for both are conceptual abstractions and neither is self-evidently real. Which abstraction makes greater empirical sense, and why?

Politics is one way of answering this question. We can ask: which construction of their interests – caste-based, religious, class-based, sectoral – sways the masses most? For reasons already outlined, all of these constructs are imagined communities for they are not small groups of face-to-face intimacies. That they are imagined, of course, does not make them any less powerful, for imaginations can be awfully serious determinants of economic and political action. The point simply is that an artefact – a class, a caste, a nation, a sector – must be transformed into a subjective experience. Large-scale transformations of this kind are typically brought about by politics. Through state policy and/or political mobilisation, political leaders seek to construct coalitions of various small units in order to create a larger community which, to begin with, exists only in imagination. That a class-in-itself has little meaning for politics until it becomes a class-

for-itself was the earlier way of stating what I have just said. A similar distinction can be drawn between a nation-in-itself and nation-for-itself, or a countryside-in-itself and a countryside-for-itself.

Just as Indian nationalists attacked the British as a target that would unite India as a nation despite internal dissensions, a large number of India's politicians, though not all, in the 1970s and 1980s looked for issues that would make the external, urban target more politically salient than the intra-village dissensions. There have also been politicians who sought to convince the rural masses that class was the most appropriate prism through which they should interpret their experiences. Compared to the construction of a rural constituency by politicians since the 1970s – pushing the state for higher crop prices, input subsidies, and credit relief – class-based politicians have fared badly. Beyond West Bengal and partially Kerala (see below), class-based politics has been quite inadequate in putting together large rural coalitions.

What does the greater ability of rural politicians to generate an agrarian, as opposed to a class, pressure in the polity indicate? One argument can be that sectoral constructions simply have greater resonance with rural people's lives than class constructions. That, according to Rao, is not true. There is, he seems to suggest, a second method of demonstrating the centrality of class: a sectoral construction may carry greater political power than a class construction, but economically speaking, sectoral politics has class-differentiated results, and therefore what appears sectoral is, in actuality, class-based. "A little class analysis would have gone a long distance", says Rao, in showing that "efforts to push up producer subsidies not only benefit (the rural haves) here and now but, in so far as such subsidies serve to fiscally detract from public investment, in the future as well" (p 1744). Contrariwise, "the rural majority here and now are hurt by higher food prices" (p 1743).

Chapter 5 of DDC ('Organising the Countryside in the 1980s') anticipates this argument. It accepts that higher agricultural prices and subsidies benefit some classes (the large and middle peasants) more than others (small and marginal peasants). On three grounds, however, it argues that this result alone cannot constitute a basis for claiming that the sectoral movement is driven by a class-bias. I will come to the special case of landless agricultural labour separately.

The first counter-argument is purely methodological. Rao commits what may be called a functionalist fallacy. He reads the motivations underlying a movement from an argument about its consequences. By the methodological standards of the 1980s and 1990s, functionalism is inadmissible in the social sciences unless some specific

conditions are met.<sup>7</sup> According to one such condition, it should be demonstrated that the observed consequences were anticipated in the original motivations of the actors. Motivations of the participants should be established independently, not imputed from consequences. The latter allows too much sloppiness in the social sciences.

Secondly, larger farmers may indeed benefit more from price and input subsidies than the smaller farmers, but what turns on that? An unequal benefit does not mean that those who receive less would not support, or profit from, the movement for higher prices and subsidies. For what may be more important for the smaller farmers is how much better they are today compared to yesterday, not how much better off someone else is at the present moment. There is no compelling evidence to show that the smaller farmers view their welfare more in terms of how bigger farmers are doing, as opposed to what their own conditions were until some time back. If anything, the available evidence suggests that every addition of benefit, however tiny, makes greater sense to the small farmer than to the bigger farmer (DDC, pp 128-29).

Thirdly, much of the class-based argument depends entirely on an economic understanding of interests. Even if smaller farmers do not derive as much direct economic benefit as the larger farmers do, the sense of empowerment that a movement provides makes their life much easier *vis-a-vis* the bureaucracy. In several places (Tamil Nadu, 1971; Punjab, 1973; Karnataka, 1983; Uttar Pradesh, 1987, 1993), the rural movements arose as a protest against the bureaucracy tampering with power and input supplies. This benefit is actually more important for the smaller farmer than for his richer counterpart. Landlords and rich farmers, given their resources and standing, can individually negotiate with the bureaucrat. Standing alone, the smaller farmer is quite helpless; organised as a collectivity, he wields more power. A strictly economic calculus of class benefits typically ignores this, even though evidence for such motivations can be shown to exist (DDC, pp 131-32).

DDC does maintain, however, that while one can talk with considerable certainty about the sectoral interests of the landed peasantry, large and small, and show that both benefit from sectoral demands, it is hard to be so confident about the landless agricultural labourers. Are they hurt by higher agricultural, especially food, prices?

Rao has no doubt that they are, which makes neither conceptual nor empirical sense. The green revolution strategy combined producer price incentives and new technology to increase production. Both Rao and I agree that a strategy that relied more on expanding the regional spread of

yield-increasing technology, and less on producer price incentives, would be better for the landless poor for it would increase employment as well as make food cheaper. From this, however, it does not follow, as Rao concludes and I do not, that higher agricultural prices inevitably hurt the poor. The welfare effect of higher producer prices on the landless depends not only on the higher consumer prices of food (which an increase in producer prices, in the absence of offsetting consumer subsidies, would typically trigger), but also on the employment created via higher production and the impact on wage rates. How exactly these three factors work themselves out – food prices, employment, wages – is an empirical question. Regional variations are likely to exist.

Consider Punjab. In 1965-66, about 31.5 per cent of the rural population was estimated to be below the poverty line, consisting mainly of the landless. By 1970-71, this proportion had dropped to 17.5 per cent and by 1990-91, still further to 3.45 per cent.<sup>8</sup> From Bihar, however, declines as large have not been reported.

Evidence from outside India further complicates the empirical picture. Studies of Malaysia show that the first phase of the green revolution (1967-74) led to remarkable rises in welfare all around: "it is a rare peasant today who does not eat rice twice a day", reported James Scott from Sedaka in 1974.<sup>9</sup> However, the introduction of combine-harvesters, by far the single largest labour-displacing machine in agriculture, created agricultural unemployment by the late 1970s. Within a decade, peasant fortunes declined.

No such complexity enters Rao's arguments about the landless. He is deductively sure that the poor are hurt, but the claim is entirely premised on the argument that higher food prices hurt the landless poor, without taking into account the other effects. A similar reasoning is applied to all net buyers of foodgrain, including small peasants. Deductive reasoning based on one effect alone is not enough to ascertain which way the results would go for the poor,<sup>10</sup> making unambiguous judgments about their welfare nearly impossible. As it turns out, the case studies of who participates in price movements do reflect this ambiguity, showing, that some landless workers support the movement while others oppose it.<sup>11</sup>

#### CLASSES AND IDENTITY GROUPS

Let me now turn to the relative salience of class, caste and religion in rural India. My argument about caste and religious identities weakening a potentially greater rural (sectoral) pressure in the polity is wrong-headed, thinks Rao, because "people who have identical economic interests *vis-a-vis* other classes in society may unite to press their joint economic claims notwithstanding

their separate non-economic identities" (p 1745).

They indeed may, but do they? How often? Can we step beyond what may happen and see what has actually happened? Even in Kerala, where class-based politicians have considerable electoral success, it can be argued that if the fit between the *ezhava* caste and the rural poor had not been so good between the 1930s and 1940s, class mobilisation would have made little headway. Class politics was inserted into the campaign for caste-based social justice.<sup>12</sup> To this day, the *ezhavas* continue to be the principal base of the CPI(M): People of similar class-positions, if nair, have gone on the whole with the Congress; if Christian, with Kerala Congress; if Muslim, with the Muslim League.<sup>13</sup>

If caste and religion have not been displaced in Kerala where class-based mobilisation has achieved some success, what can we say about the rest of the country? A class analyst could still argue that the vibrancy of religious and caste politics in Kerala (or elsewhere) is a fault of the strategies deployed by Marxist politicians, not a comment on "the reality". Such a position, if taken, would not resonate well with what we know from the field, and will only invite the charge that we are arrogant ivory-tower theorists, who believe we know the realities better than the best organisers do. In a disarmingly candid statement, E M S Namboodiripad has recently admitted that the inability of the decades-long class mobilisation in Kerala to overwhelm the religious divisions of the state may be rather more rooted in historical realities than Marxists had expected.<sup>14</sup> As people who spend less time in the field organising, let us not second-guess the most towering, tireless, lifelong mobiliser of the working classes in independent India on what the "realities" are.

Rao further argues that my hypothesis about the greater power of caste or religious factors *vis-a-vis* the sectoral construction of economic interests is "unfalsifiable". This is a surprising claim. The argument is not only testable, but DDC also presents the following evidence in support (pp 186-88): (i) the sectoral, peasant agitation in Punjab led by BKU gathered increasing strength till the mid-1980s, forcing many concessions on the government, but the religious mobilisation triggered by the Sikh insurgency after the mid-1980s literally wiped out the sectoral movement; (ii) when, in the campaign for the 1991 general elections, three different constructions of India's basic conflicts were presented to the electorate as competing choices for the future map of Indian politics – the urban-rural divide led by Devi Lal, the upper versus lower caste construction led by V P Singh, and a Hindu versus Muslim construction by L K Advani – the latter two

literally consumed the first; and (iii) by far the most popular sectoral movement of the 1980s, led by Sharad Joshi in Maharashtra, had by 1990 come under great pressure as communalism tore into the heart of Maharashtra politics and Shiv Sena began to erode Joshi's base. What had merely raised its head in 1990 has by now gone quite far. Sharad Joshi was a candidate from two seats in the 1993 assembly elections in Maharashtra and lost both. In the 1996 parliamentary elections, Sharad Joshi fought from Nanded, widely viewed as a stronghold of his peasant organisation. While the Shiv Sena-BJP candidate polled 1,73,366 votes, Joshi got a mere 71,407. A communal Shiv Sena-BJP alliance appears to have fatally wounded Joshi's organisation and political platform.

If such evidence does not present a falsifiability test for the greater pull of caste and religion, I am not sure what does.<sup>15</sup> Had Rao been more familiar with India's political universe, he would have both seen that a falsifiability test exists, and that it has been met. Caste-politics and religious nationalism are fighting it out for national prominence today, pushing class-based, or sectoral, politics to the margins, except in small quarters of India.

#### CLASS POWER AND THE STATE

Like his understanding of class, Rao's comments on the relationship between classes and state policy also have a touch of theoretical obsolescence. Even according to Marxists, Marxist theory of the state was reductionist in the 1960s: state behaviour used to be directly deduced from the correlation of class forces in the economy or society. Therefore, reacting in the 1970s, Nicos Poulantzas developed a theory of the relative autonomy of the state.<sup>16</sup> In liberal circles, too, the state theory was reductionist until the 1960s, except that state behaviour was "read off" from interest groups, not from classes, and interests, unlike classes, were considered cross-cutting, which thereby gave permanent political advantage to no particular interest groups. By the 1970s, liberal state theory, too, had moved in the direction of "bringing the state back in", giving up reducing state behaviour from societal groups and pressures.<sup>17</sup>

Rao seems unperturbed, unconvinced, or unmindful. He sees nothing analytical or new in the first three quarters of DDC, in which I track down the history of struggles over agricultural policy within the state institutions and provide an explanation for the outcomes that emerged, refusing to infer state policy and behaviour from conflicts in society. As far as I know, no political economy book on India, with the exception of Francine Frankel's work in the 1970s,<sup>18</sup> has gone inside the state institutions to analyse the struggles

conducted over economic policy. Frankel and I, however, apply different conceptual and methodological prisms to the same empirical site. A second widely read treatise by Pranab Bardhan identifies three dominant classes – the landlords, the bourgeoisie and the bureaucrats – but Bardhan derives state policy from the presumed interests of these classes and the equilibria they generate.<sup>19</sup> He does not investigate battles over economic policy within the state institutions.

Rao's theory of public policy and state behaviour is straightforward. India's state, he says, has been in the grip of the "rural haves" on agricultural policy:

The influence of large landowners on pricing policy must be seen to be of a price with their influence on various other policies...Long before the rise of the headline-grabbing lobby oriented toward price policy, the rural haves had given a good account (though not public display) of their ability to defend their interests against those of the rural have-nots. Both land reform and tenancy reform were scuttled by their money, muscle and access to government machinery (p 1744).

Even as a description, let alone theory, the claim that the small class of "rural haves" is behind the failure of land reforms as well as the relative success of price movements is highly dubious. What does the term "rural haves" mean? If it means both landlords and the so-called middle peasantry, then it can be shown that the former benefited from blocking land reforms, but the latter were either hardly touched by land reforms (if their farm sizes were below the size-ceilings), or they were the beneficiaries of reforms (if they were substantial tenants of the absentee landlords). Thus, even if their interests are similar on agricultural prices today, they were not on land reforms. Moreover, whether the interests of landlords and middle peasants are similar or conflictual, we still have to show how these interests are played out inside the state institutions if we wish to understand state policy. Should one theorise about state policy without asking what happens inside the state institutions?

Once we go directly to the institutions of policy-making, we notice, as DDC shows, the significance of ideas and economic theories. The power of large landowners in civil society notwithstanding, their biggest adversary inside the state in the 1950s was the development theory of the time. The latter, in its search for quick industrialisation, looked primarily at the macro-economic consequences of agricultural prices. Not until did the landowners penetrate national parliament and the policy-making institutions, was lasting political legitimacy given to the micro-economic view of agricultural prices. The macro view has traditionally been hostile to the idea of producer price incentives; such incentives,

in contrast, constitute the centrepiece of the micro-theory of prices. By now, scholars have sought to bridge the macro- and micro-theories of agricultural prices by importing the notion of border prices in agricultural policy debates.<sup>20</sup> Between the early 1950s and late 1970s, however, the macro and micro perspectives on agricultural prices were strikingly at odds. Because they constitute the argumentative arsenal of economic bureaucracies, international and national, economic theories matter in public policy. Inferring public policy from class power in civil society is simply wrong.

#### THE RETURN INDEX

Rao also critiques my farm returns index, which I deploy to measure whether farm returns over time have gone up or down. Using the agriculture-industry terms of trade data to derive farm returns, DDC argues, is misleading because the terms of trade only report relative prices, not costs. Depending on costs, farm returns can go up even when agricultural prices decline relative to industry. This is especially true in periods of technical change, when increases in yield reduce unit costs.

There are two ways of factoring in costs. The ideal formula would be:  $R_t$  (farm returns) =  $(P-C)Y$  (where  $P$  is output price,  $C$  unit cost, and  $Y$  yield per acre). Let me call it Formula 1. However, to measure real as opposed to nominal returns, this formula requires a price deflator. As the fierce debate over the appropriate price deflator for the agricultural sector indicates, there is no uniquely acceptable deflator available in the economic literature.<sup>21</sup> It is not a problem scholars of political economy can easily solve. Until economists settle the debate, we must willy-nilly go for the second best.

A second-best solution was available because my argument did not require measurement of exact returns, only directionality of returns. Whether farm returns were going up or down as a result of the rising agrarian pressure in the polity was all I had to find out. I, therefore, constructed a return index, which I wrote as  $R=(P/C)Y$  (where  $R$  is return index). Let me call it Formula 2, which eliminated the problem of finding an appropriate price deflator, for  $P$ , a nominal magnitude, was being divided by  $C$ , another nominal magnitude, and could, if multiplied by yields ( $Y$ ), indicate directionality. On the basis of this index, I found that farm returns were neither uniformly increasing or decreasing. They went up or down, depending on the crop and the state.

Rao has two objections to my proposal. First, my index is a mere "proximate estimate of land rents obtained after material and all labour inputs have been deducted" (p 1744). Second, depending on the relative movements

of  $P$ ,  $C$  and the appropriate price deflator, the magnitude and "even the sign of changes in  $R$ " (Formula 2) may "differ from those of  $R_t$ " (Formula 1).

The first objection does not make sense.  $C$  in Formula 2 is *not* equal to what the government of India calls total costs (or Cost  $C$ ), which include operational costs as well as land rents. It only includes operational costs (or what the government calls Cost  $A$ ),<sup>22</sup> as all graphs on farm returns in my book repeatedly make clear (pp 162-64). I did not use Cost  $C$  on purpose. If we include land rents as costs, we run into a serious conceptual problem. Are land rents price-determining or price-determined? Or, to use David Ricardo's famous formulation, is price of corn high because the price of corn land is high, or is the price of corn land high because the price of corn is high? The latter was true, argued Ricardo, because land supply was relatively fixed. Land rents, thus, can be seen as "pure rents" on an inelastically supplied factor of production. For this reason,  $C$  in my formula does not include rents over which, then, the government would provide a margin in its administered support price ( $P$ ). That I "cannot be faulted for supposing that what is good for the pure rentier is also good for the poorest rural labourer" (p 1744) is, thus, an ideologically overdetermined conclusion. We can conduct arguments without such ideological one-upmanship. Class analysts, I suspect, are not the only people who care for the poor!

The second objection, in principle, has greater substance, though it is not clear what its overall impact is. It is true that the directionality of returns based on a deflated ( $P-C$ ) and of those based on ( $P/C$ ) can differ. But that will be true if and only if the proportionate change in  $P$  is greater than proportionate change in  $C$ , but the absolute change in  $C$  is greater than the absolute change in  $P$ . My sense of the cost and price data for 1970-90 is that this is a most unlikely condition. If Rao wants to show that it is not only conceptually possible but also empirically true, he must investigate the publicly available data for 20 years. Is Rao

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prepared to do that? All of us perhaps will learn.

In conclusion, let me summarise the main thrust of my response. In political economy, if not in economic theory, simple-minded deductions can be highly vacuous, and if combined with ideological certitudes, they can also misdirect and unduly polarise debate. Deductions and polemics are no substitute for careful empirical work. Our conceptual work must mesh with the real world.

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## Import Tariffs as Strategic Policy Signals

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IN reply to my note in this journal (1995) on import protection and exports, Marjit and Sarkar (1995) have come out with a technical point of optimal pricing rule of monopoly instead of addressing my main contentions. In this note I would like to restate my main contentions and take this opportunity to put forward an interesting point of using import tariffs as strategic policy signals towards disciplining domestic producers.

Vast theoretical and empirical literature on industrial organisation and trade in imperfect competition framework shows that analysis of exports should not be based on piecemeal approaches because any single trade or industrial policy has strong trade-offs. High levels of import protection could work against export promotion if it causes long-run domestic market power and makes domestic producers highly X-inefficient in production. In the case of Japan and South Korea, import protection was combined with a set of industrial policies under an effective institutional mechanism of 'carrot and stick' policies.

My second contention is a technical one and has interesting implications in terms of using import tariffs as strategic policy signals. According to Marjit and Sarkar, at a domestic price of  $P_w(1+t)$ , where  $P_w$  is world price and  $t$  is import tariff, whole of the domestic market will be catered to by the domestic monopolist. At any given time, if the domestic producer follows the price of  $P_w(1+t)$ , the domestic market structure is no more a monopoly as imports also face the same price. At price  $P_w(1+t)$ , both domestic producer and imports share the domestic market.

One way the domestic monopolist can be shown to cater to the whole of domestic market at price  $P_w(1+t)$  is by introducing time element into the model. This is illustrated as follows. Let us take two time periods 1 and 2. At period 1, the domestic policy gives a signal to the domestic monopolist that at a future time period 2, import tariffs will be reduced. This prompts the domestic producer to commit a capacity or sales in the domestic market as an entry deterrence strategy towards blocking the entry of imports at period 2 even at the price of  $P_w(1+t)$ . In this case, exports will decline if the cost curve remains unchanged between the two periods. The interesting point here is that cost curves might shift down as a response to the policy signal of reduction in import tariffs. An

excellent empirical example of this possibility is given in a recent book by Jacobsson and Alam (1994). By comparing the Indian and South Korean hydraulic excavators industry, they point out that "...while the Indian government liberalised its licensing policy in the 1980s to allow for a greater level of domestic competition, the Korean policy meant a restriction in the field to only two producers. Second, while in the case of India, the protection from imports seemed indefinite, the Korean government clearly set a limit on the protection." In other words, restriction of entry into Korean industry allowed firms to realise economies of scale advantage in exports and the threat of import competition made them make systematic technological efforts to remain highly competitive. In other words, the South Korean policy was able to combine trade and industrial policies quite effectively. It was able to use import protection levels as an effective strategic signalling towards disciplining domestic producers. The policy in India led to the opposite results: the delicensing led to excess entry and to suboptimal scales and the policy signal of indefinite import protection to inefficiency [Patibandla 1995a]. If export promotion is the policy objective, it has to be pursued by an effective combination of a set of trade and industrial policies by minimising the negative trade-offs, rather than by piecemeal approaches.

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