Communication

Substitution Elasticities in the Large-Scale Manufacturing Industries of Pakistan: A Comment

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In the Spring 1981 edition of this *Review,* A.R. Kemal presented evidence on 'substitution elasticities', estimated over the period 1960—70, in sixteen two-digit manufacturing industries in Pakistan [8]. After applying several variants of the basic 'production function' approach, the author argued that the elasticities of substitution in Pakistan were generally low and/or insignificant and therefore a manifestation of Pakistan's 'technological dependence'; that these results are 'consistent' with similar estimates for other developing countries; and that because of the low value of the elasticities, the removal of factor price distortions, while necessary to the attainment of a more labour-intensive pattern of development, needs to be supported by policies aimed at subsidizing the developing of indigenous technologies.

While the above summary of the contents of A.R. Kemal's paper may seem innocuous enough, the actual reading of the paper has left me with a profound sense of unease and disagreement. This arises from several considerations: the use of a highly controversial data base without a warning to readers of its serious statistical pitfalls and without acknowledgement of *three* earlier *published* articles on the subject in this *Review;* the uncritical application of a 'production function' approach without setting out in clear, unequivocal terms the various assumptions that are crucial to sustaining it; the unnecessary preoccupation with the actual task of measurement rather than focusing on the implications of, and the interpretations that might be attached to, the results; the use of a selective, purposive sample of other developing-countries estimates to argue for the 'consistency' of the results; the failure to provide the reader with sufficient evidence on the statistical properties of the data used; and the failure to match the results with such evidence as exists on the movement of relative factor income shares in manufacturing and the 'bias' in technical change. I should, in this comment on Kemal's work, like to elaborate on these, and related, issues.

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The Data Base

Perhaps the most serious point of disagreement I have with the author relates to the quality of the data employed in his latest article and his supression of earlier published criticism of it. The present article is actually the fourth in a series of *studies (including a doctorate thesis)* that the author has written, all of which have been based on a single piece of work reported in [4]. In that article, the author presented the results of an exercise which purported to 'correct' the data contained in the published reports of Pakistan's Census of Manufacturing Industries for errors arising out of under-reporting and non-response, so as to produce a more 'consistent time series' for research purposes. In actual fact, the results of that endeavour, far from producing a 'consistent time series' on manufacturing activity, evoked a storm of controversy and strong disagreement over the methodology employed, the inappropriateness of the 'correction factors' used, and the sheer implausibility of the size and magnitude of quantum shifts in the estimates of key variables. These limitations were formally brought out in a forceful critique by Norbye [12], to which the author replied in [5], but not to Norbye's subsequent rebuttal [13]. Anyone who has read Norbye's comments, or looked carefully at "Consistent Time Series . . ." [4] itself, cannot but feel deeply sceptical of the value of that entire exercise, or be apprehensive of the strong possibility that, if used for serious analytical work, the data are bound to generate grossly misleading and spurious results. It is with similar misgivings that I view the author's latest contribution.

But what is, to my mind, far worse than using a highly controversial data base without even a formal caveat, which is both appropriate and necessary in any applied work, is that nowhere in the present article is there any reference to Norbye; and since the author had exchanged comments with Norbye in the pages of this *Review,* the decision to omit reference to such criticism was clearly a deliberate one. That is most unfortunate indeed. It is small wonder that, apart from a few coy remarks about the over- (under-) statement of costs (output), and the 'common problems' of census data, all the author has to say on the subject of his data is that they 'can confidently be used to estimate the elasticities of substitution' [4, p.6]. That is a totally unsustainable claim.

The Production Function

But no matter. The author is not to be deterred by methodological and statistical inexactitudes, nor by some dissenting comment which he obviously regards as being unworthy of acknowledgement, from the main purpose of his paper which is to fit several 'production functions' to his data so as to estimate the elasticity of substitution. This, I submit, is a primitive and unrealistic approach; it is also question-begging. While such an approach *might* be admissible in countries where the strong competitive and equilibrium conditions that underly it are realistic at

some tolerable level of approximation, the validity of its main postulates in a developing country like Pakistan cannot be sustained.[1] For there is little conformity between the actual experience of growth and transformation that Pakistan underwent during the 1960—70 period and the stylized notion of a 'production function', its strong competitive underpinnings (including perfect knowledge of past and future events), the smooth and predictable exogenous growth in factor supplies, etc., all of which are coordinated in a composite of equilibrium product and factor markets through an efficient price mechanism. In reality, once we allow, as indeed we must, for a real world of market imperfections and rigidities, for the unequal incidence of changing tastes and technologies and for differences in the growth elasticity of various industries, we find that the process of growth has typically been an unbalanced one: different sectors and industries experience widely different rates of growth of output and productivity as resources are continually allocated into new and expanding sectors in response to the profitability arising from the inducements associated with unbalanced growth, changes in per capita incomes, and the pattern of demand and output. In the words of a leading proponent of unbalanced growth, 'it is obvious that development *means* disturbing an equilibrium, upsetting a balance; the equilibrium of a stable society, the balance of forces that perpetuate the *status quo'* [16, pp. 170-171] (emphasis in the original). A developing country like Pakistan is thus more appropriately viewed as one which is in a *perpetual state of disequilibrium,* manifest in the continuous change in the composition of output as incomes rise, the differences in the flexibility associated with the rate of growth of factor supplies, critical inputs and decision-making ability, and the unequal flow of more efficient technologies through 'induced investment decisions' in new, expanding markets.

The author's only defence of the use of the 'production function' approach is based on the highly questionable assumption that since the *relative* degree of imperfection in factor- and product-markets remained broadly constant over the 1960—70 period, it is appropriate to proceed *as though* the economy behaved in a perfectly competitive manner. While I would be inclined to support the view that Pakistan still perhaps conformed to the workings of a labour-surplus economy despite some rise in real wages in the late '60s, it is totally incorrect to suggest that the 'distortions' in the product market remained similarly unaffected. The best manifestation of a shift in the magnitude of the distortion can be seen in the changing threshold and intensity of the movement in relative income shares during the 1960—70 period as the market environment, constituted by the expectations of the capitalists, was moulded by a *downward* modification in the convention governing the share of profits it was

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[1] I recognize that this issue has been extensively debated in the literature. There is very little I can possibly add to the arguments, so brilliantly and persuasively advanced by both sides, except to present briefly my own stand on the subject and emphasise the inappropriateness of the 'production function' approach in the context of a developing country.

considered appropriate to aim at [2]. This was sufficient not only to alter the relative degree of distortion in product- and factor-markets but, as a collorary to that, also to mark a major *displacement* in the movement in wage and profit incomes both in 1964 and in 1968. For Pakistan, there is certainly *no* support for the stylized 'constancy of relative income shares' so frequently and uncritically assumed in the equilibrium growth theory. Any 'production function' approach must therefore explicitly allow for the changing state of the magnitude of relative distortion in product- and factor-markets that occurred during the 1960–70 period.

Estimates of Substitution Elasticities

Section III of the author's article presents two tables embodying the results of his labours. I must confess that I felt a little embarrassed when I looked at the figures in these tables. The elasticity of substitution, based on alternative specifications, ranges from 0.0160 (and significant!) to *minus* 7.97; there is also an elasticity of *minus* 0.90 that is starred for its significance level. Small changes in the specification and returns-to-scale assumption lead to dramatic (and wholly implausible) changes in the magnitude of the elasticities and level of significance attached to them. Are we *really* expected to take all this seriously? Could this, perhaps, be a huge joke? An attempt to reduce econometrics *ad absurdum*?

International Comparisons

Section IV, however, goes further. Here the author compares his elasticities with those for a selected group of other developing countries and makes two comments: first, that the elasticities, most of which are low and/or insignificant in the case of Pakistan, are similarly low and/or insignificant in the other countries as well so that his results are 'consistent'; and second, that the low value of the elasticities for all these countries is a manifestation of their 'technological dependence'. The first statement is quite untrue; the second a *non sequitur*.

The first statement is untrue because, in actual fact, estimates of the elasticities of substitution based on international comparisons hardly yield either low or 'consistent' industry rankings. To the contrary, Morawetz finds that most studies (except, coincidentally, the ones chosen by Kemal for comparison), 'indicate that there does exist some scope for factor substitution . . .' [10, p. 12]. What is more, Morawetz finds — much to his discomfort — that the elasticities are seen to vary in an unsystematic and random way between different sectors and countries, with no consistency in the industry rankings based on the magnitude of the estimates. The general picture that emerges is one of disconcerting variability which is difficult to explain except in terms of the varying quality and appropriateness of the data and the (inappropriateness of the) estimation techniques employed.

The second point that Kemal makes is that the "low" (*sic*) elasticities are reflective of the high level of technological dependence in these countries. This is a *non sequitur* because — for Pakistan, at least — they could be equally due to the poor quality of the data, strong multicollinearity amongst the variables — a point on which, incidentally, the author is silent[2] —, the simultaneous equation bias, the use of an unrealistic and illegitimate estimation procedure, a confounding of the effects of inter- and intra-industry substitution possibilities because of the high level of aggregation chosen, the bias arising from varying capital-utilisation rates, the bias due to the exaggeration of the restriction on the value of the elasticity of substitution imposed by considering only the techniques actually chosen, and, as argued earlier, the bias caused by the failure to allow explicitly for market distortions. Nor does it follow that a high elasticity, on the other hand, reflects greater indigenous innovative abilities. Indeed, this is the first time that I have heard the proposition that the magnitude of the elasticity of substitution is inversely related to the level of 'technological dependence'.

The Elasticity of Substitution and Factor Shares

There is a further important element of inconsistency in all these estimates. If, as the author suggests, the aggregate industry-wide elasticity of substitution for Pakistan is *less* than unity, then, as we all know, with a positive rate of capital-deepening and neutral technical progress, labour's share in value added should have shown a tendency to *rise* over time. In actual fact, as I have demonstrated elsewhere [2], the share of wages and salaries in net manufacturing output in Pakistan during the 1958–71 period *fell* quite sharply, both in the aggregate and at varying rates in the different industries. How does one therefore reconcile these conflicting results?

The neo-classical explanation of this 'paradox' is that the decline in labour's share occurred in response to a strong 'bias' in technical change and that the magnitude of this 'bias' was large enough to *reverse* the rise in labour's share that would have occurred with capital-deepening and inelastic substituability taken together. But as the author's own work has shown [6], technical change has, on an average, been *neutral* in its incidence between labour and capital; it does not therefore enter into the determination of, nor provide an explanation for, the movement in relative income shares in Pakistan.[3]

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[2] For example, one estimating form (p.20) has wages, value added and time all in one equation; another (p.23) has wages, the capital-labour ratio and time; and a third equation (p.26) wages, the capital-labour ratio, employment and time! No matrix of correlation coefficients is reported; nor are there any tests for serial correlation.

[3] I should here like to warn the reader that the finding about neutrality of innovations is based on the same treacherous data base, i.e. Kemal [4], and should therefore be treated with appropriate caution. Nevertheless, it does seem very unlikely that a strong capital-augmenting bias in technical change explains the behaviour of relative income shares in Pakistan.

Capital Intensity in Pakistan's Manufacturing Sector

The final section of the author's paper opens with the rather bizarre statement that the capital-output ratio in Pakistan is amongst the 'highest in the world'. Perhaps this is a misprint. What the author probably meant to say was that the capital-labour ratio in Pakistan was amongst the highest in the world. But even here, the contention is open to critical scrutiny. I presume that the source of this statement is Khan [9], whose findings, made a decade ago, have become something of a matter of unquestioned faith. In any event, Khan never made the statement that the author ascribes to him. What he did say was that the level of capital intensity in *some* sectors of activity in Pakistan appeared to be higher than that obtaining in countries better endowed with capital. Moreover, Khan was careful to preface his admittedly 'startling' results with a number of qualificatory remarks. He conceded that international comparisons of the kind undertaken by him were fraught with difficulties and that, at best, he could only hope to achieve 'very general qualitative conclusions' [9, p. 241], because of the problems associated with exchange rate conversions, and differences in product mix, methods of estimation, capital utilisation rates, the quality of data, etc.

Now, while Khan's results have been widely quoted, a little reflection will show that his index of capital intensity for Pakistan was actually quite suspect. In that exercise, Khan undertook an elaborate adjustment procedure intended to revalue the book value of capital data reported in the Census of Manufacturing Industries to a figure more appropriate to its 'replacement cost', a procedure whose practical result was to *raise* the numerator of the index of capital intensity by varying amounts in the industries covered by the analysis. But he applied no similar correction factor to the capital stock data in the countries used for comparison, nor did he make any adjustment to his denominator — man-years. Yet, it is now well known that the 1962-63 Census data that Khan used as the source of his calculations was extremely poor, with an understatement of perhaps 40 percent [4]. Quite apart from the inherent problems of measurement which, taken together, would be sufficient to render any international comparison of capital intensity a rather tenuous exercise, Khan's own estimation procedure can be seen to embody a significant element of upward bias in it.

But, of course, in [4], the author is supposed to have corrected all these biases. It would be certainly interesting to compare the author's 'corrected' capital-labour ratio with the same countries used in Khan's study. Is the level of capital intensity in Pakistan — notwithstanding all the difficulties of comparability — still the 'highest in the world'?

Surely, even casual observation will show that the techniques of production used in Pakistan's large-scale manufacturing industries are, in fact, quite *labour*-intensive in the sense that one can easily imagine a more capital-intensive technique having been employed — especially in auxiliary operations such as materials movement, packing, storing, etc. — if those industries were operating in a more advanced

country? Surely, no one would *seriously* advance the proposition that the level of capital intensity in Pakistan's textile industry is (or ever was) *higher* than that obtaining in the United States with its advanced technology, fully-automated, mass production lines and, more recently, with its extensive use of lasers, computers and micro-processor-controlled operations?[4] That the level of capital intensity in Pakistan is (or was) high relative to that which might have been more appropriate to our 'factor endowments' is, perhaps, a fair statement. But to suggest that it is 'amongst the highest in the world' not only is quite fantastic, but imputes a degree of accuracy to the results that is *not* warranted by the data.

This brings me to what I regard as the principal explanation for differences in capital intensity between sectors and across countries. It has very little to do with 'distorted' factor prices, or, contrary to what Kemal would have us believe, an array of meaningless numbers that emerge from a bogus CES or VES 'production function' fitted to data of questionable accuracy. It is, rather, a reflection of fundamental differences in the *scale of activities;* the larger the scale of output, the higher the capital intensity associated with it. To be sure, there may be specific historic policy considerations and circumstances that are instrumental in securing a shift towards a more (or less) capital- (labour-) intensive direction: thus, the actual level of capital intensity appropriate to a given scale of output might be modified by such factors as the disproportionality between the scale of the imported technology and the initially small size of the domestic market,[5] the ease of access to information on technological alternatives or 'monopolized' technologies, the pattern of demand and the choice of production *commodities* that is dictated by it, the ability of those in charge of production to perceive and implement known labour- intensive techniques through *ex post* adaptive adjustments to basic processes already installed, etc. These factors, frequently mentioned in the literature, will certainly play some role in conditioning the outcome. But the evidence that exists — obtained partly from a survey I conducted in the large-scale manufacturing sector in 1980 [1] — points to the overwhelming importance of differences in scale and the volume of output in determining whether a particular activity will enjoy a high (or low) capital intensity or not.[6]

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[4] Moreover, one may ask why, if the level of capital intensity in Pakistan is actually higher than that obtaining in the US, the productivity of our labour is, perhaps, one-tenth the US level. The only explanation would be a degree of inefficiency in the personal attributes of our labour force (skills, education, age, etc.) that would be as incredible as the hypothesis to be proved.

Incidentally, I have worked out relative 'capital intensities' in a small sample of Pakistan and US manufacturing establishments based on the Census of Manufacturing Industries data on Pakistan for 1967-68 and data for the US reported in [11]. In terms of book value of fixed capital per man and the overvalued conversion rate of Rs. 4.76/$, in no sector is the differential between the US and Pakistan less than a factor of four, and the weighted average is closer to six. Quite sensibly, these differentials broadly corresponded to similar differentials in the productivity of labour.

[5] This is, of course, the familiar 'technological determinism' argument.

[6] A similar view, based on plant-level data, is also expressed in Pack [14], Morely & Smith [11], and Stewart [15].

Efficiency in Manufacturing

Scattered through the rest of the paper are a number of homely prescriptions and pedantic incrustations unsubstantiated with argument or solid empirical evidence. At one point (pp. 13-14), the author says that Pakistan should develop its own indigenous technologies — a worthy suggestion, but one made by someone who fails to understand that if Pakistan were able to do so, it would not perhaps be an underdeveloped country to begin with! Elsewhere, the author states that moving towards a more labour-intensive pattern of development would be a good thing because it would 'improve efficiency' (p. 7). Frankly, I am surprised that Kemal should again resort to the old 'inefficiency' argument when his entire doctorate thesis, as well as his 1979 article which was based on it [7], was devoted to arguing that, contrary to earlier findings, protection has served as an efficacious instrument in initiating dynamic learning effects in manufacturing industry, and that a substantial part of the very rapid growth in output during the 1960—70 period can be seen as a progressive 'working off' of an initially high cost disadvantage. But even if the author has now changed his mind, it is still insufficient to speak in general terms about inefficiencies; after all, it makes a great deal of difference what *kind* of inefficiency one has in mind. Has protection led to the establishment of industries which are non-competitive under all conditions (the so-called allocative inefficiency case)? Or, has it created industries that are X-inefficient (in which case production *could* be carried out at lower costs but fimrs voluntarily relax their search for cost-reducing innovations)? Or, is the author more concerned with the case where protection is exploited by paying monopoly wages and profits in the protected situation? As Bergsman argues, 'protection not only permits domestic production that *cannot* compete with imports, but also permits domestic production, at non-competitive costs, that *could* compete with imports if such competition were necessary for survival' [3, p. 411] (emphasis in the original). Since the allocative inefficiency case seems to make an unimportant contribution to the overall level of inefficiency in manufacturing [3], and since, according to my own work [2], differences in the levels of effective protection do not appear to have exerted an adverse influence on the capacity of industries to exploit the advantages of technical progress and increasing returns — advantages that accrue as a *by-product* of expansion in the scale of output, and *not* as some 'exogenous' time-trend variable in a 'production function' —, the inefficiency in Pakistan's manufacturing industry has largely manifested itself in the inter-industry structure of factor rewards. In that case, the author need not fret over the allocative inefficiency and X-inefficiency consequences of the system of 'incentives' facing the manufacturing sector: he should be more concerned with its adverse distributional consequences.

CONCLUSION

The article under discussion is a poor piece of scholarship. Without adequate warning to the reader or due acknowledgement, the author uses a data base that is fraught with grave statistical errors and biases, employs a wholly illegitimate estimation procedure, and arrives at a set of numbers which purport to measure the 'elasticity of substitution'. On obtaining unhelpful results, the author shifts from a pure neo-classical explanation of factor intensities to a structuralist one and then presents a purposive sample of estimates for other developing countries to support his arguments. I have attempted in this brief note to highlight the numerous qualificatory conditions that attach to the exercise and the rather simple-minded inferences that have been drawn from it. In the light of these considerations, I am constrained to conclude that the entire task is, at best, a seriously misleading one; at worst, it is completely vacuous.

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