IPA [redacted] 80055-13 Editorial Committee Mtgs. Corresp. 1970

[redacted]

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Files

DATE: December 16, 1970

FROM: Judy Maguire

SUBJECT: Editorial Committee Meeting

A meeting of the Editorial Committee was held on December 8, at 3:30 p.m. Present were Mr. Please (Chairman), Mr. Silcock (Editor), Mr. Balassa, Mr. Baldwin, Mr. Gulhati, Mr. Haq, Mr. Pryor, and Miss Maguire (Secretary).

1. A Study of Road User Charges in Central America: Anthony Churchill and others

was submitted to the committee for preliminary consideration. The committee agreed that the paper was a competent and interesting study which would be a most useful complement to the Walters paper. It seemed to be the consensus that, after specified revisions had been made, the paper would be most suitable for publication as an Occasional Paper. It was agreed that the paper should be sent to an outside assessor for comment. Professor Vickery was mentioned as a suitable candidate.

The editor agreed to discuss suggested revisions with the author.

a) Among other points it was suggested that the relationship between investment benefits and pricing policies should be more fully explained. Also in this section the difference between economic and financial analysis needed to be more clearly brought out.

b) The question was raised as to whether the congestion charge was actually valid. A suggestion was made that much of the congestion was shown by the study to be due to poor administration; once the administrative changes necessary for the new program were put into effect, the congestion might disappear.

c) In some instances it was thought that additional disclaimers as to the reliability of the figures would be advisable.

d) It was proposed that some brief indication be given toward the end as to: how the proposed changes would affect congestion, how these would affect urban transport and industry costs, and what would be the income distribution effects.

e) The validity of the idea of a "subsistance corridor" was questioned. It was felt that this issue needed some additional explanation.

2. The editor gave an interim report on work being done on the Thias/Carnoy paper Cost-Benefit Analysis in Education: A Case Study on Kenya.

Thus far chapters 2, 4 and 8 have been removed, chapters 1 and 10 changed slightly, and chapters 3, 5 and 6 condensed. Chapter 7 is now in the annex, and where possible some of the other annex material has been left out. It is hoped that the revisions will be completed and the manuscript in the hands of

the editor by the first of the year.

3. The editor gave a brief synopsis of the report he made on reviews given the Occasional Papers. It was noted that we seem to have had better notice in the technical rather than the economic journals. It was thought that this was probably due to the rather specialized nature of the majority of these papers. A suggestion was made that we might send a set of the papers to the leading economic journals in hopes that these might be reviewed as a series. The question then arose as to what was in fact the benefit in seeking reviews. If this benefit were publicity, this might just as well be obtained by a series of judiciously placed advertisements. If we were concerned with critical feedback, did we seek this from the academic community, or from practicioners who might find these studies more directly useful?

It was agreed that work being done within the Bank would profit from all the exposure possible. Comment should, therefore, be sought from every available source. With this in view, the editor suggested that some sort of committee might be set up to oversee a better distribution of the papers.

4. Status of forthcoming papers.

The Costs and Benefits of Family Planning Programs: George C. Zaidan

The Economic Benefit of Road Transport Projects: van der Tak/Ray

are in the final stages of editing, and should be sent to The Johns Hopkins Press by the end of the year. A number of potential papers have been submitted to the editor. Several of these should be available for consideration by the committee after the first of the year. These include particularly:

"Analysis of Investment in Electric Power" H.D. Jacoby

"Evaluation of Employment Prospects in Less Developed Countries" S.K. Singh

"The Economic Regulation of the Road Transport Industry" Conrad J. Oort

"Development Plans and Planning: A Survey of Bibliographies" A. Schumacher

cc: Members of the Editorial Committee
Mr. Kamarck, Mr. Stevenson, Mr. Lowther
Mr. Pryor, Mr. Hoffman
Mr. Chenery
Division Chiefs

INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: All Senior Economic Staff (See Distribution) **DATE:** December 11, 1970

FROM: Hollis B. Chenery HBC

SUBJECT: Senior Economic Staff Meeting

Beginning December 18, 1970 the Friday morning Senior Economic Staff Meeting will start at 10:00 a.m. It will continue to be held in Room D556.

Economics Department	Economic Program Department	Development Research Center
Messrs.: Kamarck, A.M.	Lerdau, E.	Goreux, L.
Henderson, P.D.	Beier, G.	
Stevenson, A.	Carter, N.	
Balassa, B.	Hulley, J.	
Bohr, K.	Levy, E.	
de Weille, J.	Please, S.	
Egbert, A.	Sacchetti, U.	
Elz, D.	Saxe, J.	
Hawkins, E.K.		
Holland, E.		
Hughes, H.		
Macone, A.		
McPheeters, R.		
Reutlinger, S.		
Silcock, T.H.		
Singh, S.		
Tiemann, A.E.		
Westebbe, R.		

* In addition to this list, anyone who is temporarily acting as a Division Chief, is also invited.

Editorial Committee on Dec 8, 1970 (TUES)

at 3:30

in D-520

D-322	~~D-400~~	Stanley Please	~~x 4081~~ 3951	
		T.H. Silcock		
C-308	~~C-307~~	George Baldwin	x 2635	YES
	D-450	Bela Balassa	x 2781	YES
	C-706	Barend A. de Vries	x 4526	No, leaving for Paris.
	D-822	Ravi Gulhati	x 5323	YES — sent material Dec. 4, 1970
	D-640	Mahbub ul Haq	x 5133	YES — sent material Dec. 4, 1970
Jan 11'71	~~D-441~~	~~P.D. Henderson~~	~~x 3435~~	YES
D-435	~~A-831~~	John A. Holsen	~~x 4908~~ 3604	BRAZIL
	A-210	Benjamin B. King	x 4482	? Back on 7th.
	D-545	Shlomo Reutlinger	x 2141	Tokyo Back on 14th
	D-1123	Michael Hoffman	x 2173	Never attends
	D-921	Donald J. Pryor	x 2530	YES

Miss Judy Maguire

OFFICE MEMORANDUM

TO: Members of the Editorial Committee **DATE:** December 4, 1970

FROM: T. H. Silcock

SUBJECT: Editorial Committee Meeting

A meeting will be held in D-560 on Tuesday December 8 at 3:30.

The agenda will be:

1. Preliminary consideration for publication as an Occasional Paper of "A Study of Road User Charges in Central America", of which members have already been sent a copy.

2. Reviews of Occasional Papers: comments on the Editor's report.

3. Report by the Editor on completed work.

4. Interim report on H. Thias and M. Carnoy "Cost-Benefit Analysis in Education".

5. Relation of Editorial Committee to proposed committee on country reports.

6. Other manuscripts under consideration.

cc: Mr. Hoffman
Mr. Pryor

Mr. Kamarck, Mr. Stevenson, Mr. Lowther

FORM No. 61.EC
(7.65)

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT

INTERNATIONAL DEVELOPMENT ASSOCIATION

D-34

CONFIDENTIAL

ECONOMIC COMMITTEE

EC/O/70 - 150

MEMORANDUM

November 27, 1970

TO: Members of Economic Committee

FROM: Andrew M. Kamarck

SUBJECT: Editorial Committee

The following have agreed to serve on the Editorial Committee through September 30, 1971:

Stanley Please (Chairman)
T. H. Silcock (Editor)
George Baldwin
Bela Balassa
Barend A. de Vries
Ravi Gulhati
Mahbub ul Haq
~~P. D. Henderson~~
John A. Holsen
Benjamin B. King
Shlomo Reutlinger

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Andrew M. Kamarck DATE: November 18, 1970

FROM: T. H. Silcock

SUBJECT: Editorial Committee

The term of the Editorial Committee expired on October 1, 1970. The following have been asked and (except for Mr. King who is abroad) have agreed to serve in the period ending September 30, 1971:

Stanley Please, (Chairman)
T.H. Silcock, (Editor)
George Baldwin
Bela Balassa
Barend A. de Vries
Ravi Gulhati
Mahbub ul Haq
P. D. Henderson
John A. Holsen
Benjamin B. King
Shlomo Reutlinger

THS/be

Betty,

We can relax about this.

Chaffey tells me it has gone to all Editorial Ctee members & to the widest of the three distributions of Economic Committee papers. The distribution lists are no longer printed, for any papers.

NS

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: November 18, 1970

FROM: T.H. Silcock

SUBJECT:

Enclosed please find a copy of A Study of Road User Charges in Central America, to which I hope the committee can give preliminary consideration at the next meeting, which we hope to hold about the end of the month.

I am also enclosing a note on the reviews of the Occasional Papers, of which we have made an analysis in this office. The committee might like to consider whether this note should be forwarded to the Publications Committee as it stands, or whether it wishes for any further work to be done.

I have been informed by Messrs Thias and Carnoy that there is a chance they will have the re-draft of their paper ready for consideration for the meeting.

3 Enclosures: copy of Road User Charges in Central America
Comments on this paper
copy of Reviews of Occasional Papers

cc: Mr. Hoffman
Mr. Pryor

Messrs Kamarck, Stevenson, Lowther

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee

DATE: November 18, 1970

FROM: T. H. Silcock

SUBJECT: Reviews of Occasional Papers

The Johns Hopkins Press sends out over 100 copies of each Occasional Paper for review. The slip which they enclose asks for copies of any reviews to be returned to the Press, and all such copies are filed in the Bank's Department of Information and Public Affairs. No search is made for reviews that are not returned to the Johns Hopkins Press and they have informed us that it is quite possible that copies are reviewed without any notification to the Press. It would probably be worthwhile asking any economist in the Bank who sees a review of an Occasional Paper in one of the less likely journals to notify this office.

The Johns Hopkins Press considers that the response is reasonably good by modern standards. The proportion is certainly rather low, but this is because the number sent out was initially very high when the series was new and The Johns Hopkins Press has continued to use the same list without questioning it. I have now cut the list down, excluding at least those journals which do not carry book reviews, but were originally circulated in the hope that they would write a note about the new series.

The following are brief notes about reviews of the individual books: (it should be noted that we are likely to receive more reviews of all papers after No. 6 because of normal time lags)

No. 1. The Economic Choice between Hydroelectric and Thermal Power Developments

Coverage was pretty wide, but most of it did little more than refer to the title and author with a brief reference to the new series. There was only one really favorable review which appeared in The South African Journal of Economics. The review in Economica was very critical.

The total number received was 38, of which 8 were 10 lines or less.

No. 2. Quantification of Road User Savings

There were fewer reviews of this paper as was to be expected, but the paper was much more favorably received. We had two very favorable reviews in Economica and The Journal of Political Economy, and several others that were distinctly warm. There were also three very favorable reviews in transport or engineering journals. Virtually no comment was hostile, and in terms of total review coverage this was probably the

most successful of all the papers.

The total number received was 29, of which 9 were 10 lines or less.

No. 3. The Export Experience of Developing Countries

This was very fully summarized in Development Digest and was also the first Occasional Paper to be noted in the Economic Journal. There was only one enthusiastic review which appeared in Dutch in Maandschrift Economie. The range of notices for this paper was disappointingly narrow. There was little adverse comment of any kind, but the paper was largely unnoticed.

The total number received was 15, of which 3 were 10 lines or less.

No. 4. Sector and Project Planning in Transportation

This also was summarized in Development Digest, although the summary was a little less satisfactory. There was one moderately enthusiastic review in Dutch in Tijdschrift Voor Economie. Other notices were again not numerous but there was little hostile criticism.

The total number received was 10, of which 4 were 10 lines or less.

No. 5. The Economics of Road User Charges

It is surprising that this very substantial paper received so few reviews and yet has sold so well. There was one fairly enthusiastic review in the Journal of Economic Literature, which poked fun in a quiet way at the Bank's Preface elaborately hedging against commitment to Walters' very orthodox views.

The total number received was 6, of which 3 were 10 lines or less.

No. 6. Notes on the Mechanics of Growth and Debt

Again, the number of reviews was very disappointing. The Economic Journal again gives merely a 'books received' notice, but the brief notice is enthusiastic about the clarity and novelty of this monograph.

The total number received was 4, of which 1 was less than 10 lines.

No. 7. Reappraisal of a Road Project in Iran

This was rather better reviewed than the two previous ones. The Manchester School was quite enthusiastic as was the Engineering Economist. Some of the others were reasonably warm and there was no adverse comment. The number however was rather disappointing.

The total number received was 10, of which 1 was less than 10 lines.

No. 8. Automotive Industries in Developing Countries

Reviews still seem to be coming in about this paper, but the record is disappointing so far; the Manchester School gave it a fairly good review but the Economic Journal wrote a rather critical note, all the other journals that noticed it so far have given it no more than a brief factual notice.

The total number received was 9, of which 1 was less than 10 lines.

No. 9. Manufacture of Heavy Electrical Equipment in Developing Countries

So far the only notice that goes beyond a brief factual description is one in Italian in L'Industria - Rivista di Economia Politica, which is moderately enthusiastic. No doubt more reviews will be received later.

Of the10 reviews received, 3 were 10 lines or less.

No. 10. Techniques for Project Appraisal under Uncertainty

Reviews are just beginning to come in about this paper. Two out of three have been mildly favorable.

General comment. We seem, on the whole, to receive rather more notice in the technical and semi-technical press than in the economic journals. It is probably to be expected that the number of reviews should decline as the series becomes more established, but there seems no pronounced trend other than this general decline in the number of reviews.

We have had rather more hostile comment from economists than is altogether acceptable. This usually takes the form of criticizing

the papers for oversimplification. Presumably this is a fairly natural reaction of academics to relatively practical papers which can hardly afford to be as sophisticated and avant garde as economic journals might wish.

Perhaps on the whole we do better with the papers that have a more practical orientation such as Numbers 2 and 7, but the distinction is not very marked and some quite favorable reviews have been given to the relatively theoretical papers.

I discussed the matter with Mr. Brain of The Johns Hopkins Press and one of his recommendations is that we should try to make the cover a little more interesting as this would possibly attract rather more notice, giving at least wider coverage if not better technical reviewing. I am not convinced that a more attractive cover would help much. However, I am impressed with the argument that we need to establish the separate identity of each paper as a book that merits review. I have therefore asked for suggestions from the Press for varying the colors of the cover while retaining the existing format in other respects. I think that in part we do suffer from being treated as half way to a journal and making the books of different color might help.

cc: Mr. Hoffman
Mr. Pryor

Messrs Kamarck, Stevenson, Lowther

Mr. Chenery

Form No. 57 INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: November 18, 1970

FROM: T.H. Silcock

SUBJECT: A Study of Road User Charges in Central America

This is an appropriate type of study for an Occasional Paper and is suitable in length. Its style is fairly practical and it is clearly a follow-up of the Walters' paper The Economics of Road User Charges, which has been one of our most widely used publications. On these grounds it seems a reasonably good candidate for publication as an Occasional Paper but it needs substantial improvements before it can be published. The Committee is asked to consider whether the improvements outlined below would make it appropriate for publication as an Occasional Paper and whether it wishes to suggest any other changes.

In some degree the hand of the Committee has been forced because although the paper has not yet been considered for publication or formally submitted, letters have already been sent by the Bank to Central American governments stating that the Bank is ready to publish this as an Occasional Paper. Perhaps the Committee would care to comment on the appropriateness of anyone in the Bank telling an outside authority that we are now ready to go ahead with the publishing of a study as one of the Bank's Occasional Papers without prior reference to the Editorial Committee.

Personally I have no objection to tentative approaches being made to governments at an early stage so as to save the author doing unnecessary work if permission would not be granted, but I hope the Committee will insist that this particular form of words will not be used unless it has first been consulted.

The Chairman has asked me to give an opinion on this study. I do not feel very well qualified in transport economics and know nothing of Central America. My only basis for judgment is the evidence of the paper itself.

It shows evidence of a high level of analytical competence and also of a great deal of practical work in collecting material. I confess to some nervousness about the reliability of the data, simply because there is a good deal of evidence that the manuscript was not very carefully checked for consistency and reliability in minor matters. It was however sent in at a time when the author was about to leave for a long period abroad and has not since been revised by him and I hope the Committee will not take too hostile a view on this account. It is fully possible that the substantive material is reliable and that these minor matters will be attended to when the work is formally submitted.

The following changes seem to me to be necessary to make it fit for publication. I am not including minor editorial changes since I am assuming that it will be carefully revised.

Chapter 2: It has been suggested that this should be transferred to an appendix, but I do not agree with this. I feel that we need Chapter 2 in its present position, except that some of the tables might reasonably be transferred to an appendix. I feel that more discussion here of such issues as whether road taxes are essential for revenue purposes on other grounds than those of "paying for the roads" might be appropriate; also some of the hints about the different effects of different taxes, at the margin, on the behavior of road users might be expanded instead of giving so much emphasis to average incidence; after all, one of the points emphasized in the study is the underutilization of the main rural roads.

Chapter 4: Part of the Introduction and a few of the key facts from this chapter should be included in Chapter 2 and the remainder put in an appendix.

Chapter 5: The development model appears to need more explanation; in particular the subsistance corridor is not adequately explained. This is a most important chapter and needs careful revision as there are many points (apart from the very poor preparation for reproduction) which make it difficult to follow.

Chapter 6: This chapter is also very important and rather too long for the balance of the paper as a whole. I would suggest that A and B and part of C could reasonably go into one chapter and the remainder into another and that this would improve the balance of the work as a whole.

Chapter 7: Most of B and C in this chapter can be transferred to an appendix.

My impression is that the urban section of this paper is better and more interesting than the rural section, although no doubt more work has been done on the rural section. It would however be desirable in the initial theoretical part to give a little more emphasis to the urban section of the work. Apart from this however it seems that the chief thing that the paper needs is rearrangement, roughly along the lines indicated, some clarification of the theory, and a great deal of careful revision.

cc: Mr. Hoffman
Mr. Pryor

Messrs Kamarck, Stevenson, Lowther

FORM NO. 61.EC
(7-65)

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT
.....
INTERNATIONAL DEVELOPMENT ASSOCIATION

D334

CONFIDENTIAL

ECONOMIC COMMITTEE

EC/O/70 - 150

November 27, 1970

MEMORANDUM

TO: Members of Economic Committee

FROM: Andrew M. Kamarck

SUBJECT: Editorial Committee

The following have agreed to serve on the Editorial Committee through September 30, 1971:

Stanley Please (Chairman)
T. H. Silcock (Editor)
George Baldwin
Bela Balassa
Barend A. de Vries
Ravi Gulhati
Mahbub ul Haq
P. D. Henderson
John A. Holsen
Benjamin B. King
Shlomo Reutlinger

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. P.D. Henderson DATE: November 17, 1970

FROM: T. H. Silcock

SUBJECT: Arrangements for the Editorial Committee

We spoke briefly today about the Editorial Committee. The role of this committee will need to be changed, partly because there are now two economics departments instead of one, partly because a separate committee will be needed when we begin publishing country reports, and partly because the practice according to which we have been working -- and which works very well -- differs from the formal definition in the Organization Manual (4.04, No. 2), since we do not in fact involve the Economic Committee in our communication with the Publications Committee, but report directly to Mr. Kamarck who, as Deputy Chairman of the Economic Committee and a member of the Publications Committee, passes our recommendations on directly to the Publications Committee.

I have discussed this with Mr. Kamarck. Clearly Mr. Chenery will be involved in any changes, but there are many more urgent matters that need decisions. This can be handled quite adequately for the next six months. Mr. Please, the Chairman of the Editorial Committee is in one Economics Department, I am in the other. We can continue to report to you. There is no urgent reason for change if you are willing to let it continue like this. In a few months I will submit a memorandum to you about the relation of the Editorial Committee for the Occasional Papers and the ad hoc committee (that I propose to discuss with Mr. Lerdau for the publication of country reports), both to one another and to the Departments and the Publications Committee.

For the present I recommend that we leave this as it is. Mr. Kamarck will invite the existing and new members of the Editorial Committee to serve for another year. Some time after you have succeeded him -- and before September 30 when the new Committee's term expires -- we can put up new suggestions to Mr. Chenery. I put this to you now only because it requires your willingness to continue a not very satisfactory structure for a few more months, until the dust settles.

cc: Mr. Kamarck
Mr. Please

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: October 27, 1970

FROM: T. H. Silcock

SUBJECT:

The enclosed revised versions of the Procedure for Selection and Review of Manuscripts and the two Guidelines take account of the criticisms expressed in the last meeting. The amended paragraphs have been marked in the margin to facilitate comparison.

If any member would like further discussion a further meeting will be arranged. Minor verbal amendments could be done by correspondence.

Mr. Please has suggested, in relation to para. 3 of the Procedure, that we could often use young, highly qualified professionals, recently recruited to the Bank, as assessors, and that in asking Division Chiefs to nominate them we could suggest this. I fully concur with this suggestion.

Unless any comments are received, I hope we may use these revised guidelines. The note to all economists will be delayed until the present review of research by Mr. Balassa is completed.

cc: Mr. Hoffman
Mr. Pryor

Mr. Kamarck
Mr. Stevenson
Mr. Lowther

En sure No. 1.

Procedure for Selection and Review of Manuscripts for Occasional Papers

1. A note setting out the function of the Occasional Papers, and the character of the manuscripts that we would consider publishing, should be sent to every professional staff member in the Bank, outlining the procedure for submitting a manuscript. It should be made clear that any manuscripts that fit the general description of the Occasional Papers would be welcome, and that we have no intention of recommending for publication only material from the Economics Department. The Committee is willing to consider manuscripts at an early stage and advise on further work that needs to be done. At a later stage the manuscript should be formally submitted for the Committee's approval and recommendation to the Publications Committee. At this stage the author is expected to have the manuscript as near perfect as he can make it, even though further editing will normally be needed.

2. Wherever a manuscript is being typed specially for submission as an Occasional Paper, the secretary should be instructed to get in touch with Mrs. Easter so that so far as possible the style of typing could conform to the requirements of The Johns Hopkins Press, since these differ in a number of minor respects from normal Bank procedures.

3. Manuscripts should be sent to the Editorial Office through the Division Chief who would take necessary steps to clear with the Head of the Department. Detailed technical assessments are not expected from Division Chiefs, though naturally they would advise against publication if they considered a manuscript to be obviously unsatisfactory or against Bank policy. Division Chiefs would, however, be asked to recommend an assessor to assist the Committee, preferably from another division, but a specialist in the same general area of study. His name should not, in the first instance, be revealed to the author, in case he preferred to give a confidential assessment.

4. We will refer to the assessor, sending guidelines so as to achieve uniform standards, and put his report and the manuscript before the Editorial Committee. The Committee would itself assess the manuscript on its own merits, supplemented by specialist assistance from the assessor, and by any other written assessments sent in by those who had read it.

5. In general any report provisionally approved for publication at this preliminary meeting would be sent by the Editor to a further assessor, preferably outside the Bank. Changes recommended by the internal assessor should not normally be made until the external assessor's report is received.

6. Every manuscript must contain, as an appendix, a complete list of every reference cited, with full bibliographical details. Material consulted but not cited may also be included at the author's discretion.

7. The author of any manuscript will in future be required to prepare an index. The Editor will be prepared to advise on the methods to be used, but it is undesirable for this work to be delegated. It is a part of the book and should be related organically to its data, analysis and argument.

Guidelines for Review of Manuscripts in Editorial Committee

The following set of questions is designed to focus the attention of members of the Committee on agreed criteria, so as to narrow the range of discussion and achieve more uniform assessments. It is meant as a check-list and guide to criteria, not as a means of voting or precluding other discussion.

1. Operational relevance. Does the manuscript deal with a topic that has practical relevance for people and institutions involved in development?

2. Relation to existing literature. Does the manuscript contribute new evidence or material on a topic which is being actively discussed in the literature? If not, does it show sufficient novelty or originality in its own field to merit publication without this advantage?

3. Appropriate form of publication. Is the quantity, structure and content of the material such that it should be published as:

a. A full-length book published by a commercial publisher or University press?

b. One or more articles in professional journals?

c. A blue-cover mimeographed Bank report?

d. A Bank Occasional Paper?

or e. It should not be published at all?

4. Additions and deletions. Does the manuscript appear to you to have

a. Little or no material that should be boiled down or omitted, or alternatively expanded?

b. Major sections or chapters that could be omitted with improvements in focus and readability?

c. Several points or sections that require expansion or fuller explanation?

d. A need for both expansions and deletions, or extensive rewriting?

5. Clarity and readability. How good is the writing?

a. Does the manuscript need major editing even to achieve acceptable style, grammar and syntax?

b. Do the main points fail to come through clearly? Does it need more attention to the connections, the key sentences and paragraphs and the conclusions?

c. Are there excessive differences between the levels of professional skill assumed in different parts of the manuscript? (E.g. elaborate explanations of well-known economic principles in one part, assumption of advanced knowledge in others.)

d. Is the style clear, convincing and consistent, needing relatively little editing?

6. Technical terms. Bearing in mind that the readers of the Occasional Papers are probably mostly general practicing economists, but that we are not mainly dealing with a list of regular subscribers, and can sometimes cater for a more specialist audience, we may ask

a. Do the technical terms used suggest that the manuscript is directed to a more specialist audience than general practicing economists? If so is there justification both in the difficulty of the subject matter and in its relation to the Bank to justify publication in the Occasional Papers series?

b. Does the author use plain English wherever technical terms contribute nothing in rigor, clarity or economy?

c. Are technical terms, that are outside the normal range of a general practicing economist, clearly defined and consistently used (except in specialist studies -- see above)?

7. Mathematics. How is the mathematical reasoning handled?

a. Are the main arguments in the manuscript capable of being expressed, with only minor losses of rigor and generality, in plain English?

b. Where mathematics are needed in the text, are the assumptions -- particularly where they have policy implications -- clearly and fairly set out?

c. Are the symbols logically designed, as easy to handle as possible, and clearly defined in an easily located place?

d. Is the mathematics handled with a degree of rigor appropriate to the work in hand, and consistent throughout the manuscript?

8. Correctness and Validity of Conclusions. Do you believe that

a. The author's arguments are thoroughly convincing, scholarly and as rigorous as the evidence allows?

b. The author makes a reasonable case for his conclusions, i.e. such that at least some qualified economists are likely to be convinced by it?

c. The conclusions are not well supported by the evidence and analysis, and readers are likely to judge the work as seriously lacking in professional competence?

9. Assessment. This will depend on whether the manuscript is being given preliminary or final consideration. If the consideration is preliminary, does the manuscript show sufficient relevance and promise, and is it sufficiently appropriate in form and substance, to justify recommending further work along the lines indicated in earlier sections or in other ways for publication as an Occasional Paper? If not, do you recommend an attempt to have it published in some other form?

If the consideration is final, should the manuscript be recommended to the Publications Committee as it is, and if approved published with no more than normal editing? Or should it be accepted conditionally, subject to agreement to make some minor changes? Or should it be rejected?

Enclosure No. 3.

Guidelines for Assessors

You have agreed to act as an assessor for the attached manuscript. This note is to assist you in helping the Editorial Committee achieve uniform standards.

Occasional Papers are monographs, with a practical orientation and arising out of this Bank's activities. Since the Bank encourages publication of such material either in technical journals or as books produced by university or commercial presses, an Occasional Paper should usually be a study that needs to be presented as a whole, is too long for a single journal article and is too short for a full-length book.

There may be special circumstances, related to the practical orientation of the work or its relevance to an international market, which might lead to a manuscript being published as an Occasional Paper when (on grounds of length and structure alone) it might be suitable as a book or a group of articles; but our standards should in no circumstances be lower than those of a good university press or an internationally known technical journal.

The criteria by which the Editorial Committee assesses manuscripts are set out in the attached guidelines which we have adopted for our own use. You are particularly asked to give us your help in assessing the relation of this manuscript to the current special literature in its field, its use of special technical terms, its handling of mathematical reasoning and the level of its scholarship and cogency of its arguments.

We ask you to give your assessments with complete candor, and to regard yourself as our agent. Your name has not been given to the author and we shall keep it confidential unless you prefer to engage in discussion with him. The Committee may, at its discretion, ask him to comment on extracts from your assessment provided anonymously.

If you consider that, with manageable improvements, the manuscript should be published, we should welcome your suggestions for such improvements, and if we decide to proceed we shall pass them on to the author.

The Committee much appreciates your willingness to undertake this task for us.

MISS Z. [illegible]

FORM NO. 57 INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: October 13, 1970

FROM: T. H. Silcock

SUBJECT: Taxation and Earmarking in Developing Countries"

Mr. Eklund sent this reply to Mr. Balassa's comments of July 30 the day before he left the Bank. It is circulated for information. Mr. Balassa has told me that he does not intend to reply.

cc: Mr. Kamarck, Mr. Stevenson, Mr. Lowther

Mr. Pryor
Mr. Hoffman

OFFICE MEMORANDUM

TO: Mr. Bela Balassa

DATE: September 30, 1970

FROM: Per Eklund

SUBJECT: Your comments on my study "Taxation and Earmarking in Developing Countries".

1. General Comments

1. In your review of my study, you make a number of comments which I cannot permit to remain unanswered. Your three main points are:

i) that it conveys the impression that the author has a "cause" to defend.

ii) that there is a need of more case studies; and your principal suggestion

iii) that you do not see much usefulness in Chapters II and II. "Some of their major conclusions are open to doubt, and they are not necessary for establishing the usefulness of earmarking in certain situations".

2. To begin with, I leave it to others to judge whether I have a "cause" to defend in my study. If I have a "cause" to propagate, it is to emphasize that much of the criticisms of earmarking in textbooks and elsewhere have been based on a much too simplistic analysis; i.e. a lack of understanding of the complex reality of fiscal decisionmaking.

Secondly, in your paragraph 1 and in paragraph 4, you refer to the cost aspects of earmarking. Of course, earmarking has "costs" attached to it, just like other fiscal measures; justified at one point in time, but not necessarily indefinitely. These "costs" and the required trade-off between costs and benefits are referred to in the study: in the introduction, in the main body, in the case study on the Philippines, and in the conclusion. Therefore, your presumption in paragraph 4 that the cost aspects are not discussed is simply not correct; moreover, I completely fail to see the logic of the assertion that costs and benefits of earmarking must be discussed in the same chapter. If the implication is that this leads to a deliberate discounting of the costs, I can only refer you, inter alia, to the final paragraph of the summary.

3. In regard to case studies and empirical material, it is self-evident in any empirical work that more material is always desirable. The relevant question is whether there is sufficient material to enable tentative conclusions to be drawn. You do not refer to the cross-section analysis of 18 countries which have earmarking for highways, and 19 non-earmarking countries; and a time series analysis of five countries. A different opinion on this aspect of the study is the one expressed by the Bank's previous editor, Mr. Latimer, who considered the study to be "thorough and a model of good documentation" despite other reservations he may have had about it.

1/ Memorandum from Hugh Latimer to Stanley Please, "Earmarking and Taxation"- Per Eklund, October 15, 1969.

4. You are in doubt as to the usefulness of Chapters II and III: ("Budget Policy and Earmarking of Taxes", and "Comparison of an Actual System of Parliamentary Democracy with the Lindahl Theory", respectively). Chapter II sets out the two main theories of fiscal behavior, the simplest of which is the ability to pay approach, which, as is well-known, leaves the pattern and level of expenditures as given. The second theory, the benefit theory of taxation, in contrast, attempts to see decisions on taxes and expenditures and the linkage between them, reflecting and being determined by the environment. As you know, among economists the benefit theory of taxation has given rise to considerable controversy. But no-one has offered a better theory of public expenditure that the Lindahl theory, therefore this theory is worthy of our interest. There has also been considerable progress in rehabilitating and reconstructing Lindahl's solution, through Samuelson's clear statement of the theory of consumption externalities (public goods) and through Johansen's reformulation of the Lindahl solution.1/

5. Chapter III sets out the divergencies which arise, when the Lindahl theory is compared with an actual system of parliamentary democracy. The cumulative effect of these divergencies leads to the conclusion that the fiscal optimum will not be fully achieved in actual parliamentary democracies. Still the analogy between budget determination and price formulation in the market is useful. I argue that these divergencies become magnified, when the theory is confronted with conditions in developing countries. In brief, therefore, the message is that when the "flaws" between theory and reality widen, the importance of instruments such as earmarking, and other policies, which raise voters' - taxpayers' satisfaction, increases.

6. In fact, nothing could be easier for me than to agree with you that the theory in these two chapters is not necessary to establish the usefulness of earmarking in certain situations. But, your comments, give me the impression that you consider earmarking simply a pragmatic matter; whereas, on the contrary there are important theoretical issues involved, which must not be hidden. Your query as to the usefulness of these two chapters suggests that you have failed to see the important theoretical issues which a study of earmarking - if it is to have any ambition - must consider. Stanley Please put it:

> "Essentially these theoretical issues are analagous to those in the historic Chamberlin/Robinson revolution which dealt with supply problems. This revolution got us away from the previous polarized and obviously unrealistic assumptions of perfect competition and "pure monopoly". They were replaced by Chamberlin's analysis in terms of "groups" of varying sizes (duopoly, eligopoly, etc.). What now needs to happen on the demand side is to get away from the polarized assumptions of individual goods on the one hand and public goods on the other and to introduce the notion of

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1/ Another very recent contribution proving the existence of a Lindahl equilibrium is Duncan K. Foley, "Lindahl's solution and the Core of an Economy with Public Goods". Econometrica, Vol. 38, No. 1, January 1970, p. 66.

"group goods" - geographical groups, social groups, age groups, etc. Essentially, this is a large part of what lies behind the earmarking analysis".1/

Indeed, the major flaw of the Lindahl theory - the lack of assured revelation of voters' preferences for public goods is mitigated, when we start to analyze "group goods", i.e., goods with few externalities for outside groups. In this context, the Lindahl theory and the necessity of joint tax-expenditure decisions - implicit or explicit (formal) earmarking - becomes more realistic. In sum, I firmly reject your assertion that Chapters II and III lack usefulness and that their major policy conclusions are open to doubt.

2. More Detailed Comments

7. To turn to the particular points you have raised, the purpose of my presentation of the Lindahl theory - explicitly set out in the Annex to Chapter II - was not to present and repeat the mathematical proof that the Lindahl solution satisfies the conditions for a Pareto-optimum. In my study, there are enough references, I would think, to P. A. Samuelson and to Leif Johansen's mathematical formulation for me to object to your statement that there is "no proof that a stable equilibrium will be reached". However, let me pursue your point in paragraph 2. "As is well-known, bilateral monopoly and isolated barter do not lead to a Pareto-optimum or for that matter to a stable equilibrium". The analogy between ordinary market economics and the public goods case, with which we are concerned, is not that simple. In the former case, as is well-known, when we increase the number of sellers of a homogenous product indefinitely, you pass from monopoly through indeterminate oligopoly and can hope to reach a determinate competitive equilibrium in the limit. Whenever the indifference applies, there will be only one price in the market for the commodity. In contrast in the Lindahl model, it would not help, if there were a greater number of parties involved; for each additional group we are compelled to introduce one more distribution ratio. For this reason, a perfectly valid assumption , in theory, is to assume that each party has power and ability to 'defend its own interest'; another matter is that in the real world I think we both concede that the situation is different.

8. There is no disagreement between us: when the Lindahl theory is confronted with reality the major imperfection is that individuals-groups are expected to reveal their preferences in situations in which such a revelation would be to their disadvantage. (Your paragraph 3). My subsequent statement that "a fuller revelation of preferences will take place", when taken in the context it appears, is a qualification to this general case.

1/ Memorandum to Mr. Hugh Latimer from Stanley Please, October 29, 1969. "Earmarking and Taxation" - Per Eklund.

9. In your paragraph 5, you state that the presumption that in the absence of earmarking less than the optimal amount would be spent is not universally valid. I agree that this may be the case, but then in the first instance earmarking was not necessary either. The general case is that earmarking increases taxable capacity; cooperative solutions to preference revelation are more easily reached. The situation you depict in your last sentence in paragraph 5 is not clear to me.

10. In paragraph 6, you infer that the example in Annex I leads to the conclusion that "the vote should be taken on "budgetary packages" rather than on individual issues which leads us away from earmarking". I fail to see this. The basic issue is not a distinction between "individual issues" and "packages", but that voting groups must consider the cost - the tax burden - jointly with expenditure decisions. It is true that in parliamentary democracies, voters do choose among "packages" presented by the various political parties rather than voting on individual issues. The complexity and multipurpose of many public expenditures has made an evaluation and weighing of costs and benefits of expenditure difficult. The political leaders or the representative group spokesment, therefore, have to fill the function of evaluating experts (see my page 20, paragraph 48). But there is certainly no guarantee that this is an optimal procedure: expenditure decisions are made annually as part of the routine of preparing the government's budget, in theory the total burden of taxes must be consistent with these decisions. To exemplify, Charles E. Lindblom has stated that in the case of the U.S. system, appropriations should be taken out of the present budgetary process so that in a feasible way appropriations can be linked very tightly to basic legislation. (See page 21, paragraph 49).

11. In regard to a minority group, "it will make little difference if there is an earmarking or not and the potential threat of the breakup of the majority exists equally in the two cases" (your paragraph 7). The argument in this case rests on the grounds that there is a distinction between an "assurance" by a majority safeguarding the interest of a minority group, not written into the law and one written into the law. In other words, there is a distinction between decisions made annually as part of the routine of preparing the government's budget, and those written into the law.

12. In the same paragraph (7), I think you mistakenly state that you do not see that earmarking would be substitute for decentralized decisionmaking on the regional level, while earmarking in the state budgets of the U.S. and Switzerland can hardly be explained by their federal structure. At a given point in time earmarking, accommodation of group interests, is likely to lead to more optimal tax-expenditure decisions, and similarly a delegation of authority to regional units will increase taxpayers satisfaction. There are abundant references to this linkage, in my study (page 22, and Chapter V) and in the literature (Leif Johansen, see also for instance W. Arthur Lewis "Decentralization thus raises

taxable capacity"[1]) that I am somewhat surprised at your statements. Let me refer to what I say on page 23, paragraph 54:

> "Earmarking and decentralization may be treated as substitutes, but they must not be looked upon as mutually exclusive. From this point of view, one may note that in federally administered countries like the U.S.A., Brazil and Switzerland, earmarking is frequent. Both the decentralization of governmental policy-making and earmarking point to the fact that special interests including geographical interests have been accommodated".

13. In your paragraph 8, you accept my point about majority coalitions, but your main query is whether at a low stage of development, people would be more individualistic and hence the society more heterogenous? To make my point more clear, let me refer to Richard Goode's listing of the conditions necessary, for a successful use of the personal income tax: a fairly high degree of economic development, but in addition they emphasize honest maintenance of records, honest administration and voluntary compliance on the part of the taxpayers. (See page 8, paragraph 18). I think you must agree with me that the personal income tax has not proved itself appropriate at a low stage of economic and social development. Merely on this ground, can we avoid the conclusion that this reflects a close relationship between a more individualistic and heterogenous society and a lower stage of development?

cc: Messrs. Kamarck
Stevenson
Please
Editorial Committee

PEklund/hf

1/ W. Arthur Lewis, "Comment in Agricultural Development and Economic Growth". Ed. by Hermer M. Southworth and Bruce F. Johnston, Cornell 1967, p. 494.

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Files

DATE: September 29, 1970

FROM: Judy Maguire

SUBJECT: Editorial Committee Meeting

1. A meeting of the Editorial Committee was held on September 15, at 3:30 p.m. Present were Mr. Please (Chairman), Mr. Silcock (Editor), Mr. Balassa, Mr. Baldwin, Mr. Holsen, Mr. B.B. King, Mr. Pryor and Miss Maguire (Secretary).

2. Mr. Please said that the recent death of Samuel Lipkowitz represented a very real personal and professional loss for all members of the Editorial Committee. Mr. Lipkowitz was an active participant in the Committee's work, and his assistance will be very greatly missed.

3. The meeting of the committee was devoted to a discussion of the guidelines proposed by Mr. Silcock for the selection and review of manuscripts.

4. The initial reaction of those attending was that in their view these guidelines were excessively stringent, and would tend to discourage authors from submitting their manuscripts for consideration. One feeling was that the primary purpose of the Occasional Papers series is not the promotion of individuals, but the exposure to outside professional criticism of the economic work undertaken within the Bank. Furthermore there was frequently little individual incentive for staff members to have their work published, and the committee ought therefore to adopt a more positive attitude in order to stimulate a flow of potential papers. On submission these papers need not be considered as finished pieces but as drafts which, when "doctored", would be suitable for publication. The Editor explained that he regarded it as part of his function to try to see manuscripts at an early stage and to encourage publication, but he did not believe that a more formal procedure of selection or rejection at some point would be discouraging and he felt that it would help to encourage high standards.

5. It was generally agreed that a mechanism should be set up for preliminary and informal consideration of manuscripts. At this stage the committee could either indicate that the manuscript was not suitable for publication in the Occasional Papers series or encourage the author to do further work on it. It could also indicate particular changes that it would regard as necessary before it could recommend the manuscript to the Publications Committee. The committee felt that authors needed some encouragement to undertake further work, even though there could be no guarantee by the committee that the author's work would be acceptable.

6. The committee expressed some concern at the apparent intention of the proposed guidelines to place an excessive dependence on a single assessor. It felt that the committee should itself be left free to come to its decisions after its own study of the work. The Editor explained that he had not intended

the assessor's role to replace that of independent study by the committee. It was expected simply that he would give an impartial and expert finding, particularly on the more technical aspects where he could be assumed to be more familiar with specialist literature. The committee suggested that anonymity of the assessor might not be necessary within the Bank but it was agreed that it should be offered to him and left to his own decision. The committee did not feel that there had been any real difficulty in the past in obtaining critical reviews. There was objection to the idea in the guideline proposals that additional supporting or critical material would not be circulated to the committee. On the contrary it was felt that assessments should be solicited from many different quarters. The committee felt that in the past a number of varied opinions on certain papers had been a valuable aid in its own assessment of a manuscript.

7. Finally it was decided that a very real effort would have to be made to search out ideas for future papers. It was noted that particular attention should be paid to those areas -- i.e. applied economics -- where the Bank has a comparative advantage. Staff members should be made aware that the committee is interested in manuscripts not only from the Economics Department but from all parts of the Bank.

Distribution: Members of the Editorial Committee
Mr. Pryor, Mr. Hoffman

Messrs. Kamarck, Stevenson, Lowther

Mr. Chenery

Division Chiefs

Editorial Committee meeting Tuesday, September 15, in Room D-560 at 3:30 p.m.

ON 1971 COMMITTEE

To discuss -- see Agenda

D-400	Stanley Please	x 4081		
D-534	T.H. Silcock	x 2482		
C-307	George Baldwin	x 2635	YES	YES
D-450	Bela Balassa	x 2781	YES	Yes
G-1038	Barend A. de Vries	x 3625	YES	Yes
~~D-441~~	~~P.D. Henderson~~ GULHATI / HAQ	x 3435	Henderson in Moscow!	Yes
A-831	John A. Holsen	x 4908	YES	Yes
A-210	Benjamin B. King	x 4482	YES	
~~C-305~~	~~Samuel Lipkowitz~~	~~x 4605~~		
D-545	~~Herman G. van der Tak~~ Reutlinger	x 2141	out for a year	Reutlinger
D-536	Rena Zafiriou	x 1481	Yes	
D-1123	Michael L. Hoffman	x 2173	Never attends - goes to Pryor instead	
D-921	Donald J. Pryor	x 2530	YES.	

TO ADD: Ravi GULHATI

REUTLINGER

Mahbub ul HAQ

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

Judy

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: September 2, 1970

FROM: T. H. Silcock

SUBJECT: Editorial Committee Meeting

A meeting of the Editorial Committee will be held on Tuesday, September 15, in Room D-560 at 3:30 p.m.

No business on manuscripts is pending at present. Attached is the agenda for the meeting.

1 Enclosure

THS:be

Mr. P.D. Henderson August 25, 1970

T. H. Silcock

Proposed Agenda for Editorial Committee

Before you went away I suggested that we should have a meeting on these administrative changes to be followed in dealing with manuscripts. In view of the fact that you will be here only a week, I think it will probably be better to wait for the meeting until Mr. Please returns, but I would prefer to get the notice out as soon as possible. May I discuss this draft agenda with you and then send it out? I propose to fix a date for the meeting as soon as possible after Mr. Please returns, so as to get this done before the Annual Meeting. I have already sent this to Mr. Stevenson and he has no comments and says I can go ahead with it.

1 Encl.

Agenda for Editorial Committee

1. Procedure for selection and review of manuscripts for Occasional Papers. (Encl. No. 1.)

2. Guidelines for review of manuscripts in Editorial Committee. (Encl. No. 2)

3. Guidelines for assessors. (Encl. No. 3.)

4. Pre-editing and editing.

We in the editorial office welcome the help of divisional editors who often insure that we receive a manuscript in much better condition than would be possible without them. However, we should like a ruling that once a manuscript is submitted, the divisional editor's role is complete. Editors must try to negotiate with authors, to achieve greater clarity and consistency, and these negotiations are made unnecessarily difficult if divisional editors also -- in effect -- claim proprietary rights. We must emphasize that any corrections of galleys must be negotiated directly with us by the author.

5. Correction of galleys and page proofs.

We propose to notify authors that every change made in the galley by an author costs the Bank more than a dollar for every line. Normally an author is allowed a limited number free, and is required to pay for all those in excess out of his own pocket. Since our authors are economists, and accustomed to making imaginary trade offs, we propose to tell them the price and ask them to make a conscientious effort to judge all galley corrections as if they had to pay for them personally. For the present we are not proposing any further sanction.

Page proofs are sent to the author only for information. Corrections are very costly at this stage. Our attention should be called to any actual error, but we reserve the right to eliminate it in the way that will incur least cost, or even to leave it if the cost of correction, in our opinion, ourweighs the seriousness of the error.

6. Circulation of correspondence.

Enclosure No. 1.

Procedure for Selection and Review of Manuscripts for Occasional Papers

1. A note setting out the function of the Occasional Papers, and the character of the manuscripts that we would consider publishing, should be sent to every professional staff member in the Bank, outlining the procedure for submitting a manuscript. It should be made clear that any manuscripts that fit the general description of the Occasional Papers would be welcome, and that we have no intention of publishing material only from the Economics Department. We should also indicate that we would expect formal submission of a manuscript as near perfect as the author can make it. It should be a manuscript that he would not be ashamed to have printed and published exactly as it stands.

2. Wherever a manuscript is being typed specially for submission as an Occasional Paper the secretary should be instructed to get in touch with Mrs. Easter so that so far as possible the style of typing could conform to the requirements of The Johns Hopkins Press, since these differ in a number of minor respects from normal Bank procedures.

3. Manuscripts should be sent to the Editorial Office through the Division Chief who would take necessary steps to clear with the Head of the Department. Detailed technical assessments are not expected from Division Chiefs, though naturally they would advise against publication if they considered a manuscript to be obviously unsatisfactory or against Bank policy. Division Chiefs would, however, be requested to help us with the selection of an assessor. This should be preferably someone in a different division, and his identity should not be revealed to the author. He should be a specialist in the same general field whomwe could approach for a confidential technical assessment of the professional value of the manuscript.

4. We will refer to the assessor, sending guidelines so as to achieve uniform standards, and put his report and the manuscript before the Editorial Committee. In general other supporting or critical material will not be circulated. An exception could be made for comments by the Division Chief on possible usefulness of the material to practical work within the Bank. The aim, however, would be to have the report assessed on the basis of its own merits and an assessor's report.

5. In general any report provisionally approved for publication at this preliminary meeting would be sent by the Editor to a further assessor, preferably outside the Bank. Changes recommended by the internal assessor should not normally be made until the external assessor's report is received.

6. The Committee will then decide whether to reject or to ask for any revisions for publication. At the Committee's discretion, extracts from the assessors' reports may be sent to the author, but their identity should be kept confidential. We must inform the assessors that their anonymity will be respected.

7. Every manuscript must contain, as an appendix, a complete list of every reference cited, with full bibliographical details. Material consulted but not cited may also be included at the author's discretion.

8. The author of any manuscript will in future be required to prepare an index. The Editor will be prepared to advise on the methods to be used, but it is undesirable for this work to be delegated. It is a part of the book and should be related organically to its data, analysis and argument.

Enclosure No. 2.

Guidelines for Review of Manuscripts in Editorial Committee

The following set of questions is designed to focus the attention of members of the Committee on agreed criteria, so as to narrow the range of discussion and achieve more uniform assessments. It was prepared by Mr. Baldwin and developed and amended by the editor. Further discussion will be welcome.

1. Operational relevance. Does the manuscript deal with a topic that has practical relevance for people and institutions involved in development?

Yes No Borderline

2. Relation to existing literature. Does the manuscript contribute new evidence or material on a topic which is being actively discussed in the literature? Yes No If not, does it show sufficient novelty or originality in its own field to merit publication without this advantage?
Yes No

3. Appropriate form of publication. Is the quantity, structure and content of the material such that it should be published as:

a. A full-length book published by a commercial publisher or University press?

b. One or more articles in professional journals?

c. A blue-cover mimeographed Bank report?

d. A Bank Occasional Paper?

or e. It should not be published at all?

4. Additions and deletions. Does the manuscript appear to you to have

a. Little or no material that should be boiled down or omitted, or alternatively expanded?

b. Major sections or chapters that could be omitted with improvements in focus and readability?

c. Several points or sections that require expansion or fuller explanation?

d. A need for both expansions and deletions, or extensive rewriting?

5. Clarity and readability. How good is the writing?

a. Does the manuscript need major editing even to achieve acceptable style, grammar and syntax?

b. Do the main points fail to come through clearly? Does it need more attention to the connections, the key sentences and paragraphs and the conclusions?

c. Are there excessive differences between the levels of professional skill assumed in different parts of the manuscript? (E.g. elaborate explanations of well-known economic principles in one part, assumption of advanced knowledge in others.)

d. Is the style clear, convincing and consistent, needing relatively little editing?

6. Technical terms. Bearing in mind that the readers of the Occasional Papers are probably mostly general practicing economists, but that we are not mainly dealing with a list of regular subscribers, and can sometimes cater for a more specialist audience, we may ask

a. Do the technical terms used suggest that the manuscript is directed to a more specialist audience than general practicing economists? If so is there justification both in the difficulty of the subject matter and in its relation to the Bank to justify publication in the Occasional Papers series?

b. Does the author use plain English wherever technical terms contribute nothing in rigor, clarity or economy?

c. Are technical terms, that are outside the normal range of a general practicing economist, clearly defined and consistently used (except in specialist studies -- see above)?

7. Mathematics. How is the mathematical reasoning handled?

a. Are the main arguments in the manuscript capable of being expressed, with only minor losses of rigor and generality, in plain English?

b. Where mathematics are needed in the text, are the assumptions -- particularly where they have policy implications -- clearly and fairly set out?

c. Are the symbols logically designed, as easy to handle as possible, and clearly defined in an easily located place?

d. Is the mathematics handled with a degree of rigor appropriate to the work in hand, and consistent throughout the manuscript?

8. Correctness and Validity of Conclusions. Do you believe that

a. The author's arguments are thoroughly convincing, scholarly and as rigorous as the evidence allows?

b. The author makes a reasonable case for his conclusions, i.e. such that at least some qualified economists are likely to be convinced by it?

c. The conclusions are not well supported by the evidence and analysis, and readers are likely to judge the work as seriously lacking in professional competence?

d. The conclusions are clearly at variance with Bank policy and likely to be a source of embarrassment to the Bank?

9. Assessment. My preliminary net judgment is as follows:

a. For publication as an Occasional Paper with no more than normal editing.

b. For conditional acceptance as an Occasional Paper, subject to agreement to make some minor changes.

c. Against publication as an Occasional Paper, but for Bank or outside publication in some other form.

d. Against encouragement of publication in any form.

Enclosure No. 3.

Guidelines for Assessors

You have agreed to act as an assessor for the attached manuscript. This note is to assist you in helping the Editorial Committee achieve uniform standards.

Occasional Papers are monographs, with a practical orientation and arising out of this Bank's activities. Since the Bank encourages publication of such material either in technical journals or as books produced by university or commercial presses, an Occasional Paper should usually be a study that needs to be presented as a whole, is too long for a single journal article and is too short for a full-length book.

There may be special circumstances, related to the practical orientation of the work or its relevance to an international market, which might lead to a manuscript being published as an Occasional Paper when (on grounds of length and structure alone) it might be suitable as a book or a group of articles; but our standards should in no circumstances be lower than those of a good university press or an internationally known technical journal.

The criteria by which the Editorial Committee assesses manuscripts are set out in the attached guidelines which we have adopted for our own use. You are particularly asked to give us your help in assessing the relation of this manuscript to the current special literature in its field, its use of special technical terms; its handling of mathematical reasoning and the level of its scholarship and cogency of its arguments.

We ask you to give your assessments with complete candor, and to regard yourself as our agent. Your name will not be revealed to the author, though the Committee may, at its discretion, ask him to comment on extracts from your assessment provided anonymously.

If you consider that, with manageable improvements, the manuscripts should be published, we should welcome your suggestions for such improvements, and if we decide to proceed we shall pass them on to the author.

The Committee much appreciates your willingness to undertake this task for us.

Judy.

FORM NO. 57 | INTERNATIONAL DEVE. MENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Files

DATE: August 7, 1970

FROM: Judy Maguire

SUBJECT: Editorial Committee Meeting

A meeting of the Editorial Committee was held on July 28, at 3:30 p.m. Present were Mr. Baldwin (Acting Chairman), Mr. Silcock (Editor), Mr. Balassa, Mr. Lipkowitz, Mr. van der Tak, Miss Zafiriou, Mr. Pryor and Miss Maguire (Secretary).

Mr. Baldwin opened the meeting with the suggestion that some time be given to a discussion of the purpose behind the Occasional Papers series, and to the consideration of the criteria which are to be applied in the selection of possible future papers. He felt that there should be clearly defined objective standards by which a paper might be measured, and offered the following as a sample of those which might be included.

1) Appropriateness for the Occasional Papers format. Are there more appropriate outlets for the type of audience the paper is likely to attract?

2) Readability, clarity and interest.

3) Professional respectability.

4) The paper's demonstrated relevance for policy in the development field.

5) Its attitude toward the Bank and member countries.

6) The element of novelty. Does it reveal a new insight into a particular problem?

7) Does the use of unnecessary jargon or mathematics limit the audience to which it might appeal?

Mr. Silcock added that it was hoped that an entire session could be devoted to these questions some time in the near future. It was agreed that such a session would be extremely valuable, especially if a memorandum on the subject could be circulated to the committee before the meeting.

Cost-Benefit Analysis in Education: A Case Study on Kenya.
Thias/Carnoy

In view of the fact that a great deal of correspondence was stimulated by the seminar and committee meeting at which this paper was first discussed, it was thought advisable that the committee be apprised of recent developments.

It was agreed that the question of the technical validity of certain sections

of the paper was largely irrelevant. It would be sufficient to publish the paper with a summary description of the objections which had been raised to the methodology used.

It was reported that the authors agreed to make the changes requested by the committee as a condition of publication. These changes include toning down the policy as well as the statistical claims, and deleting or cutting down specified chapters and sections. There was some concern voiced as to whether the authors had in fact been given a detailed accounting of the changes which the committee expected to be made. It was decided that by August 14 those members of the committee who were interested should submit their criticisms and recommendations to the authors. The authors could then in turn either make an interim report to the committee, or could work directly on a revised draft. If there were any complications involving the revised version it was suggested a subcommittee might be set up to deal with these.

Taxation and Earmarking in Developing Countries.
Per Eklund

This paper was submitted for consideration for publication as a World Bank Staff Occasional Paper. The initial reaction of the committee was that the paper had some potential, but that it would need considerable work before any further steps could be taken. It was suggested that the Occasional Papers series might not be the proper outlet for a paper such as this, and that it would be more suitable for a revision as a journal article. It was decided that though on the whole members were against publication as an Occasional Paper in its present form, no definite decision should be taken until Mr. Please had had the opportunity to comment. We should, in any event, not publish without a favorable report from an outside assessor, and if we consulted one it was agreed that this person should be someone unknown to the author.

Country Studies

Mr. Silcock told the committee that he was in the process of preparing a memorandum incorporating his suggestions for publication of the material available in the country reports. A brief outline of his proposals was presented and members gave their comments.

cc: Members of the Editorial Committee
Messrs Pryor, Hoffman
Mr. Friedman
Messrs Kamarck, Stevenson, Lowther
Division Chiefs

JM:be

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: July 31, 1970

FROM: T. H. Silcock

SUBJECT: Taxation and Earmarking in Developing Countries: Per Eklund

The attached note, to which Mr. Balassa referred at the Editorial Committee Meeting on July 28, is circulated at his request.

1 Enclosure

cc: Mr. Pryor
Mr. Hoffman
Mr. Lowther (for info)

OFFICE MEMORANDUM

TO: Mr. Andrew M. Kamarck DATE: July 30, 1970

FROM: Bela Balassa

SUBJECT: Comments on Per Eklund, "Taxation and Earmarking in Developing Countries"

1. Mr. Eklund presents a case for earmarking in developing countries. The study provides much interesting material but it conveys the impression that the author has a cause to defend. This impression is strengthened by the separation of the discussion of the benefits (Ch.IV-Ch.VII) and the costs (Ch.VIII) of earmarking. In so doing, Eklund has neglected to mention some of the deficiencies of earmarking that are related to the points made in its favor, especially in Chapter VI. Combining Chapters IV, V, VI, and VIII, with a discussion of the advantages and disadvantages of earmarking under each heading would provide a better balance. It would further be desirable to expand the rather brief case study on the Philippines and to provide more case studies. But my principal suggestion is one of surgery: I do not see much usefulness in Chapters II and III. The chapters are poorly written, some of their major conclusions are open to doubt, and they are not necessary for establishing the usefulness of earmarking in certain situations. In the following, I will concentrate on these chapters.

2. I find it difficult to accept the view that bargaining will lead to the Pareto-optimal value of (h) that reflects the distribution of the fiscal burden between two groups. As Eklund correctly notes, this situation is comparable "to a market situation of bilateral monopoly or of isolated barter" (p.13). As is well-known, bilateral monopoly and isolated barter do not lead to a Pareto optimum or, for that matter, to a stable equilibrium. The same conclusion applies to the distribution of the fiscal burden since each group will try to get more public goods for less of a reduction in private expenditure. Eklund does not provide a solution to this problem by stating: "In order to reach the solution point P, through negotiating and bargaining, it is necessary to assume that each party has power and ability 'to defend its own interest'" (Annex Ch.II, p.4). Nor do I see the similarity to the case in "ordinary welfare economics" where prices are given. At any rate, in bilateral monopoly, the fixity of prices does not assure a Pareto-optimal solution.

3. Eklund himself notes that "a major criticism of the theory has ... arisen because it can be demonstrated that ... the revelation of preferences which the Lindahl model assumes, is going to suffer from imperfections" (Annex to Ch.II, p.6). Subsequently, however, he rather disingenuously argues that in the case of public goods "a fuller revelation of preferences will take place" (ibid, p.7). I would rather agree with the statement made in Ch.III that "consumers will be reluctant to reveal their preferences" and that "there is an incentive for /the consumer/ to understate his preferences" (p.17). At any rate, Eklund provides no proof that a stable equilibrium would be reached if preferences were known.

4. Eklund further suggests that Pareto-optimality cannot be guaranteed by majority voting (p.19) and "that explicit assigning or earmarking may be necessary to achieve Pareto-optimality" (Annex to Ch.II, p.8). He fails to note, however, that earmarking may not only lead away from a Pareto optimum but also reduce welfare as compared to a situation obtainable under majority rule. For one thing, earmarking constrains particular expenditures to revenues derived from particular taxes; for another, it provides incentives for overspending.

5. The presumption is also made that, in the absence of earmarking, less than the optimal amount would be spent. In this connection, Eklund approvingly quotes Lindahl's statement according to which "if Parliament were presented only with the totals of the tax bill and of public services ... there is only one way to avoid a contraction of public services harmful to all; each must undertake to pay a greater share than the other towards the cost of those services which each finds most useful" (p.19). Such a presumption is not generally valid, however. Thus, earmarking would restrict spending if minority groups had strong preferences for certain types of expenditures to the exclusion of others while combining several items would command a majority.

6. The example in Annex I, too, leads to the conclusion that the vote should be taken on "budgetary packages" rather than on individual issues which leads us away from earmarking. Indeed, in a parliamentary democracy the voters do choose among "packages" presented by the various political parties rather than voting on individual issues. The financing of state expenditures in the United States provides an exception but one may question if this is the optimal procedure (cf. the recent rejection of bond issues destined to improve education). In this connection, I may add that the one-paragraph criticism of the U.S. Federal budgetary system is gratuitous and out of place in the paper.

7. As regards minority groups, Eklund asserts that "an earmarking arrangement would be both less unstable and more credible and therefore a more desirable form of contract" (p.22). I do not see the logic of the argument; for the minority group it will make little difference if there is earmarking or not and the potential threat of the breakup of the majority exists equally in the two cases. Nor do I see that earmarking would be a substitute for decentralized decisionmaking on the regional level, while earmarking in the state budgets of the U.S. and Switzerland can hardly be explained by their federal structure.

8. Turning to the applicability of the theoretical model to real world situations, Eklund advances some additional arguments in favor of earmarking in developing countries, such as the existence of unstable majority coalitions, political interference in the administration, and administrative inefficiency. The first point is well-taken although I find it difficult to accept the view that at a low stage of development people would be more individualistic and hence the society more heterogeneous. In turn, political interference in administration and administrative inefficiencies do not necessarily speak for earmarking; earmarking, too, creates vested interests and inefficiencies may be greater in the handling of earmarked revenue than elsewhere. At any rate, the considerations noted here can be introduced without reference to Lindahl's theory.

c.c. Messrs. Stevenson
Eklund
Editorial Committee
BBalassa:alj

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: July 30, 1970

FROM: T.H. Silcock

SUBJECT: Cost-Benefit Analysis in Education: A Case Study on Kenya: Thias/Carnoy

The attached notes were received too late for yesterday's meeting, and are circulated for your attention.

2 Enclosures: Note from Mr. Thias) dated July 28, 1970
Note from Mr. Holm)

cc: Mr. Hoffman
Mr. Pryor
Messrs Kamarck, Stevenson, Lowther (for info)

INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. T.H. Silcock

DATE: July 28, 1970

FROM: H.H. Thias

SUBJECT: Mr. Shourie's Memo of July 22, 1970

1. Mr. Shourie's note of July 22, 1970 has been brought to my attention. I read it and feel obliged - although this involves an undesirable loss of time - to provide a short reply. For the matter of simplicity, let me deal with Mr. Shourie's points in chronological order:

2. In para. 3a, Mr. Shourie complains about our disqualifying his suggestion of adding other explanatory variables as "freak economic theory". This is not exactly what happened. In his earlier memo of June 24 Mr. Shourie had said that the R^2's (sc. of our equations 9.5 - 9.13) "would have been equally high if we had related employment to, say, exports, imports, a simple time variable and any of a host of other variables", and we have denounced the "say, exports, imports" suggestion as freak. To conclude from this specific example that we oppose the inclusion of any additional variables in the model is a rather daring generalization. Indeed, in equations 9.11, 9.12 and 9.13 we did attempt to trace the institutional and structural changes Mr. Shourie refers to by introducing a dummy variable that distinguishes between pre- and post- independence observations.

3. In para. 3b Mr. Shourie deals with the problem of collinearity. He develops a series of equations (employment as a function of "real wages", of sectoral GDP, and of both "real wages" and sectoral GDP) to show that certain regression coefficients have changed "very dramatically". (I might add in parentheses that my interest is not so much in whether a change is dramatical or not but rather in whether it is statistically significant - a point on which Mr. Shourie does not elaborate). However, I fail to see why this would make the regression coefficients "highly suspect". What becomes doubtful, as Mr. Shourie has clearly pointed out in the first para. on p. 2, is an "attempt to partition the variance of the dependent variable between collinear explanatory variables", or, in purely economic terms, to use the regression coefficients for straightforward ceteris-paribus projections. I do think that the text of Ch. IX would stand some more "ritualistic disclaimers" (to use Mr. Shourie's term), i.e. a careful interpretation of the meaning and limitations of the results.

4. The equations on p. 3 of Mr. Shourie's memo have other intriguing qualities. While making the step from the simple $\log N_i = f(\log W_i)$ and $\log N_i = g(\log Y_i)$ to the more elaborate $N_i = F(\log W_i, \log Y_i)$, the intercepts are disposed of for no apparent reason. The equations arrived at are completely different (as far as regression coefficients, R^2's and t-values are concerned) from our equations (9.5), (9.8) and (9.11) which, for the

matter of the argument, they ought to restitute. Even if we assume that natural logs were taken in one case, and decimal logs in the other, there is still an unexplained difference (possibly brought about by the use of undeflated sectoral GDP figures). While these points do not have a bearing on the arguments on pp. 2 and 3, they give the impression that the memo was prepared somewhat carelessly.

5. Next, on p. 3, comes an attempt to introduce an additional variable, the ratio of non-agricultural GDP to total GDP. As a result, the regression coefficients of the "old" variables change, their t-values (with the curious exception of that for the log Yi coefficient) drop, in one case from a significant to a non-significant level, the t-values for the regression coefficients of the "new" variable are far from even the 10% level of significance, and the R^2 rises from 0.87 (or 0.88, two contradictory values are given) to 0.92 in the first case (insignificant increase) and falls from 0.91 to 0.78 in the second case.[1/] (This truly embarrassing result apparently perplexed Mr. Shourie to the extent that he left out the "old" R^2 of 0.906 on the bottom of p.2).

6. What does all this suggest?

(a) the low t-values for the coefficients of the "new" variable Y* imply either that the dimension "structural change" does not significantly contribute to the explanation of the log Ni variance, or (more likely) that Y* is a poor proxy for that dimension[2/];

(b) Y* is probably closely correlated with Wi and/or Yi.

Big deal indeed. What else should one expect from a variable that relates, in the case of the first equation, to one of the "old" variables as directly as $Y^* = \frac{Y - Y_1}{Y}$ (Y being total GDP)? As for the "robustness" or otherwise of our coefficients, this is not only a "quick illustration" but also a completely irrelevant one.

7. Our remarks about the ratio of non-agricultural to total GDP and the Development Plan referred not just to the former's being mentioned in the latter, but to its explicit use for projection purposes. In our study, we have paid due respect to the work that went into the Development Plan.

1/ The expanded version of the log N_3 equation has not been given.

2/ Needless to say that there are no 1957-66 time series for indicators that would a priori seem more appropriate (such as average size of firm, energy consumption per worker, horsepower installed per worker, etc.).

However, unless we have completely misread that document, we do not think that it provides us with additional time-series for the period 1957-66 which would enable us to expand our model (and this is after all what the whole argument is about). This means that the "scope for exploring a number of alternative and more comprehensive hypothesis" Mr. Shourie believed to see and which the authors allegedly failed to realize shrinks, as it were, to Y* size.

8. I also have to put on record continued disagreement on the labor force coverage argument on p. 4. Kenya's labor force data are "appalling", and so are her GDP figures, and both are subject to periodic revisions. Granted. But that is not really the point. Neither is the observation that for a certain segment of sectoral employment turnover is rather high (but obviously overrated by Mr. Shourie - the African "target-worker" is more of a "pre-emergency" phenomenon that furthermore related to the least qualified workers only). The core of Mr. Shourie's argument is rather that the mere existence of a large group of self-employed and family workers would influence the level of wage employment in the monetary segment of the economy. However, the only way in which this could be brought about would be through some kind of Lassallean "squeeze" - which seems all but impossible under present-day Kenyan conditions that are characterized by a detailed and strict minimum-wage legislation.

9. Towards the end of para. 3, Mr. Shourie returns to what seems to be his favorite argument - namely, that a simple time variable could do the job of explaining sectoral employment in Kenya as well as our model does. May I point out again that we proceeded in accordance with the rules of econometrics, i.e. we started with a certain hypothesis that was in agreement with established economic theory and incorporated the available (viz. for the period 1957-66) evidence, and submitted it to statistical testing. The results of the testing did not suggest that we should reject our hypothesis. This statement applies of course rebus sic stantibus, i.e. once additional data become available, we may have to re-test our hypothesis, reformulate it, or even abandon it. Yet for the time being, we have come as far as one can get.

10. Some final remarks on Ch. 5 (which, together with Ch. VI is clearly the heart of our study but curiously enough has not received much attention). In our note of July 10, 1970 we contended that for cross-section data of the type used in our study (viz. those dealing with the development and use of human resources), one could not expect R^2's as high as those derived in other areas of econometric research, particularly in studies based on time series. To counter this argument Mr. Shourie refers to cross-section information which has yielded high R^2's, such as "data on fertilizer consumption in Indian districts". Fertilizer consumption and what? If it were, say, the relationship between fertilizer consumption and crop yields, I would

fail to be impressed, given the semi-technical character of that relationship. But even if it were a relationship where behavioral phenomena played a role (say, the impact of age, education, income of farmers on fertilizer consumption), I would still claim that the complexity of that relationship is moderate in comparison to the multi-dimensional topic of Ch. V.

11. Para. 5 contains another - somewhat furtive - remark which I think permits us to resolve much of our controversy about the acceptability of certain levels of R^2 under certain circumstances. This is Mr. Shourie's statement that this issue is "partly a matter of preference". This is a position which has my whole-hearted support (though I cannot help musing why it had to take us so long to get there).

HHT:csm

cc: Messrs. Henderson
Please
van der Tak

DRAFT

A Comment on Mr. Shourie's reply

July 28, 1970

I have received Mr. Shourie's reply to my memo, which dealt with the Kenya study, and I have also had the opportunity to discuss it with him.

In his memo, Mr. Shourie does not deal with any of the points which I discussed in the main part of my memo. His reaction, in duscussing this with him, is that my observations are irrelevant. Since the main part of my memo addresses itself to observations brought up in Mr. Shourie's original memo, why did he then use them as arguments against the Kenya study in the first place?

I find it extremely difficult, and time-consuming, to pinpoint Mr. Shourie's position on econometrics. He either judges my arguments irrelevant or moves his position.

Mr. Shourie states: "First, ... we are very seldom justified in looking upon a coefficient (say b_i) in a multiple regression as indicating "the change in Y as a consequence of a one unit change in X_1." There are many reasons for this. One of these is the fact that some explanatory variables may have been left out of our model and that one or more of these may be colliniar with the ones that have been included."

What does he mean by "seldom justified?" He suggests that explanatory variables may have been left out of a model so the coefficients are not "justified." This is hardly profound. What he is saying is that if a theory is wrong, so are any deductions we make from it! What he is not saying is that there exist statistical tests which tell us when the individual coefficients are significant. Does he or does he not accept significance testing? The authors of the Kenya study have shown when the individual coefficients are significant, Mr. Shourie has yet to show that they are mistaken. As I pointed out in my first memo, we can never prove a theory. Yes! Some explanatory variables may have been left out of a model, but if the ones which are included significantly explain the dependent variable then we have no way of knowing that the model may be mis-specified. There do not exist any sufficient tests which can tell us if a model is mis-specified. Only by working with the models which are significant according to our tests can we hope to obtain further insight which will enable us to develop better theories and models.1/

1/ Newton did not include the speed of light in his model - Einstein did. Should Newton have given up beforehand because "some explanatory variable may have been left out"? Of course not, Newton's theory explained many phenomena, and it is difficult to imagine that Einstein would have been able to come up with his theory had a Newton not published his. And some day someone will probably come up with yet another theory which can explain more yet.

In the appendix, attached to my memo, I dealt with the objective of econometrics. Mr. Shourie has not addressed himself to this point either. I would, however, like to come back to this question, since it seems to me that there is also a main disagreement.

Mr. Shourie contends that the main objective of econometrics is to obtain unbiased estimators. First, Mr. Shourie has yet to demonstrate that the coefficients in the Kenya study are not unbiased. Second, supposing for a moment they were biased, then he would conclude that the Kenya study should he published in order to show thefailure of the use of multiple regression analysis in cost-benefit studies. Let us pause for a moment and ask the question: show the failure of what? The only thing we may have shown is the failure to obtain unbiased estimates of the coefficients. Should this stop us? The answer is "yes" if the objective of our analysis was to obtain unbiased estimators. The answer would be "no" if our objective is to apply cost'benefit analysis to investments in education.

This leads us to consider what the authors and myself conceive to be the objective of econometrics. We believe that the objective of econometrics is to give empirical content to economic theory. Thus when multiple regression is used, then the estimators may be biased - which has not been demonstrated. But so what? It is not our objective to get unbiased estimators - although it is a nice property. Suppose the estimators are bound to be biased, then at least there is a possibility of finding out to what degree they are biased, and this information would be valuable for further studies. It may also be that if the estimators are biased, then they will have a smaller variance than estimators which are unbiased. It is thus seen, that this approach to econometrics is far more general and feasible than the one proposed by Mr. Shourie, since one can consider the effects of different properties of the estimators.

When I said that Mr. Shourie has "unrealistic idells" then I meant his approach to econometrics. Mr. Shourie says that: "In my comments to the study I was only making a plea for something that would be defensible." Defensible for whom? The study is apparently not defensible for Mr. Shourie, but again we have to be aware of the standards by which he measures what is defensible.

Let me finally address myself to some other points which Mr. Shourie brought up in his memo.

With regard to my statement that the number of excluded variables must be the same as the number of included variables, Mr. Shourie agreed, in our discussion, that his derivation implied exactly that.

With regard to the question concerning unbiasedness, I did not "demonstrate" that the coefficients are "not biased." As a matter of fact I said that in the general case the coefficients would be biased. I did,

however, point out cases where the coefficients would not be biased.

With regard to Mr. Shourie's contension that the exclusion of the error term "does not effect the validity of the proof in any way", I can only state again that one cannot derive the best liniar unbiased estimators (BLUE) without the error term. His "proof" is therefore not valid.1/

1/ That the results are the same is a coincidence. He is determining the slope of the hyperplane in which all the points are lying. The derivation of BLUE determines the slope of the hyperplane, under the condition that the sum of squares of the distances "the errors" from the points to the hyperplane is minimal, and this is quite different.

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee

DATE: July 24, 1970

FROM: T. H. Silcock

SUBJECT: Comments on the Thias/Carnoy Study: Erratum

I have been asked to circulate the attached note from Mr. Shourie. Please insert the required constants which he had inadvertently omitted. Mr. Shourie informs me that they should all bear a positive sign.

cc: Mr. Hoffman
Mr. Pryor
Messrs Kamarck, Stevenson, Lowther (for info)

INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Thomas H. Silcock DATE: July 24, 1970

FROM: Arun Shourie

SUBJECT: Comments on the Thias Carnoy Study: Erratum

1. The constant terms have been inadvertently omitted from some equations that I reported in my note of July 22 to you.

2. The three equations on the top of page 3 should include the constant terms 8.5955, 7.6010 and 3.6137 respectively. Similarly, the four equations in the second half of that page should include the terms 8.5955, 8.5444, 7.6010 and 7.0053 respectively.

3. These terms do not affect the substance of the argument given in the note.

AShourie:lcm

cc: Members of the Editorial Committee

To attend Editorial Committee Meeting on Tuesday, July 28, at 3:30 p.m. in Room D.560.

	D-400	Stanley Please	x 4081	on leave
	D-534	T.H. Silcock	x 2482	yes
	C-307	George B. Baldwin	x 2635	C.B. YES
	D-450	Bela Balassa	x 2781	YES
(Hon 300)	C-706	Barend A. de Vries	x 4526	YES
Chairman x	D-441	P.D. Henderson	x 3435	22-30th, Back 3 Tel
[illegible] Secy.	A-831	John A. Holsen	x 4908	Brazil - ret. July 25
	A-210	Benjamin B. King	x 4482	away until end Aug.
	C-305	Samuel Lipkowitz	x 4605	O.K.
	D-545	Herman G. van der Tak	x 2141	O.K.
	D-536	Rena Zafiriou	x 1481	O.K.
	D-1123	Michael L. Hoffman	x 2173	away until Aug 3.
	D-921	Donald J. Pryor	x 2530	O.K.

to discuss Per Eklund's paper "Taxation and Earmarking in Developing Countries"

and other matters outlined on agenda to follow.

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: July 23, 1970

FROM: T.H. Silcock

SUBJECT: Agenda for Editorial Committee Meeting
on Tuesday, July 28
at 3:30 p.m.
in Conference Room D-560

1. Correspondence on the Thias/Carnoy paper "Cost-Benefit Analysis in Education: A Case Study on Kenya".

2. Preliminary consideration of paper by Per Eklund "Taxation and Earmarking in Developing Countries".

3. Preliminary report by the Editor on discussions on the publication of country studies.

cc: Mr. Hoffman
Mr. Pryor
Mr. Kamarck, Mr. Stevenson, Mr. Lowther (for info)

3 (4 to be withdrawn)
~~4~~ Enclosures: copies of correspondence.

OFFICE MEMORANDUM

TO: Mr. Thomas H. Silcock DATE: July 22, 1970

FROM: Arun Shourie

SUBJECT: Comments on the Thias-Carnoy Study

1. I have read the two memoranda that you sent me about the cost-benefit study of education in Kenya. I will comment on the piece by Messrs. Thias and Carnoy in this note and shall outline my reactions to Mr. Holm's comments in a separate note.

2. In commenting on the regressions in Chapter 9 of the Thias-Carnoy study I had suggested that the coefficients of the regression equations should not be taken to indicate "the extent by which y will change with a given change in x_i" and that we should be extremely careful in using such aggregative equations computed from just 9 or 10 observations for forecasting. There were three reasons for these comments:

a. the sample period data indicated that the two explanatory variables (average earnings divided by Nairobi's cost of living index and sectoral GDP) were not really independent of each other;

b. the model that made employment in each of the sectors a function of these two variables alone was an inadequately specified one and it may be that the coefficients of the two explanatory variables would alter considerably once some additional variables were introduced; and

c. because it seemed a bit strange that the entire GDP in the monetized part of each of the three sectors should be attributed to only a small proportion of the labor force that is in fact contributing to the value added in each of the sectors.

3. Messrs. Thias and Carnoy respond to these possible objections in four ways.

a. First, they say that in suggesting that they should extend the list of explanatory variables beyond the two they have chosen I am advocating "freak economic theory." However, Kenya went through very significant institutional and structural changes in the sample period of their study. There is nothing in economic theory that tells us that we need not include explanatory variables to take account of or to represent such institutional and structural changes.

b. Second, they say that "multicollinearity does not result in biased estimates of coefficients. . . it causes an increase

in the estimated variance" and that in any case the coefficients are robust and their values do not alter when additional explanatory variables (e.g., time or the ratio of value added in nonagricultural sectors to total GDP) are introduced. In regard to collinearity among explanatory variables two cases should be distinguished: collinearity among explanatory variables that are included in an equation and collinearity among included and excluded variables. In the first case, the coefficients are "unbiased" in a technical sense but they can be very misleading since the attempt to partition the variance of the dependent variable between collinear explanatory variables becomes more and more artificial as the collinearity increases. In such a case the coefficients are very sensitive to model specification, to data errors and to the coverage of the sample.

This point is clearly brought out by considering the data given in the Thias-Carnoy study. Let us postulate that the numbers employed in each of the three sectors are a function of the 'real wage' in that sector (i.e., of average earnings divided by Nairobi's cost of living index). Using the data for Africans in Appendix Tables 9.1, 9.2 and 9.3 of the study the computed equations are as follows:

	R^2
$\log N_1 = 8.45041 - \underset{(6.688)}{0.82005} \log W_1$	0.848
$\log N_2 = 7.12615 - \underset{(6.268)}{0.47726} \log W_2$	0.83083
$\log N_3 = 3.68672 + \underset{(5.054)}{0.27714} \log W_3$	0.76153

where N stands for numbers employed, W for 'real wages' and the subscripts 1, 2 and 3 for the three sectors agriculture and forestry, private industry and commerce and government respectively. The figures in parentheses are t-ratios. Alternatively, let us postulate that employment in each of these sectors is a function of the value added in that sector. The computed equations then are as follows:

$\log N_1 = 7.68316 - \underset{(4.008)}{0.60242} \log Y_1$	0.667
$\log N_2 = 5.17530 - \underset{(0.489)}{0.05301} \log Y_2$	0.029
$\log N_3 = 4.00206 + \underset{(4.4497)}{0.30126} \log Y_3$	0.712

where Y represents the sectoral GDP. Now, let us postulate - as Thias and Carnoy do - that sectoral employment is a function of both W_i and Y_i. The computed equations are as follows:

$$\log N_1 = \underset{(3.300)}{-1.18505} \log W_1 + \underset{(1.0800)}{0.32114} \log Y_1 \qquad R^2 = 0.870$$

$$\log N_2 = \underset{(8.090)}{-0.49351} \log W_2 - \underset{(2.371)}{0.08593} \log Y_2 \qquad R^2 = 0.906$$

$$\log N_3 = \underset{(1.241)}{0.35529} \log W_3 - \underset{(0.279)}{0.08973} \log Y_3 \qquad R^2 = 0.764$$

Notice that the coefficients for log W_1, log W_3, log Y_1 and log Y_3 have changed very dramatically. It is in this sense that collinearity among explanatory variables that are included in an equation makes the coefficients highly suspect. (The correlation coefficients between Y_i and W_i for the three sectors are 0.941, -0.112 and 0.979 respectively.)

Collinearity among excluded and included variables has effects that are equally serious: the coefficients of the included variables are definitely biased and should not be treated as representing "the extent to which y changes with a given change in x_i." Books and articles on regression analysis give explicit proofs of this effect[1/] and Thias and Carnoy would be in error if they assumed that this was not the case. Furthermore, on the factual question as to whether their coefficients are robust or not Thias and Carnoy seem to be mistaken. For purposes of a quick illustration let us represent structural change in the economy by the ratio of non-agricultural GDP to total GDP and compute the equations once again. They turn out as follows:

$$\log N_1 = \underset{(3.300)}{-1.18505} \log W_1 + \underset{(1.080)}{0.32114} \log Y_1 \qquad R^2 = 0.877$$

$$= \underset{(3.126)}{-1.27641} \log W_1 + \underset{(1.181)}{0.43861} \log Y_1 + \underset{(0.585)}{0.18723} \log Y^* \qquad R^2 = 0.920$$

$$\log N_2 = \underset{(8.090)}{-0.49351} \log W_2 - \underset{(2.371)}{0.08593} \log Y_2$$

$$= \underset{(4.272)}{-0.62870} \log W_2 + \underset{(0.573)}{0.11733} \log Y_2 - \underset{(1.009)}{1.01756} Y^* \qquad R^2 = 0.780$$

where Y^* is the ratio of GDP originating in nonagricultural sectors to total GDP. The point estimate of the coefficient

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1/ I shall list only three readings that deal with this specific issue: H. Theil, "Specification Errors and the Estimation of Economic Relationships," Rev. Inst. Int. de Stat, Vol. 25, Nos. 1/3, especially pp. 41-44; Draper and Smith, Applied Regression Analysis, J. Wiley, especially Section 2.12 entitled 'Bias in Regression Estimates,' pp. 81-84. Snedecor and Cochran in their Statistical Methods, Iowa University Press, pp. 394-398, provide a more intuitive discussion of the same issue, which does not use matrix notation.

for log Y_1 changes by about one-third, that for log W_2 by about one-fifth and that for log Y_2 changes from -0.08593 to +0.11733.

c. The third reason that Thias and Carnoy give for not including a variable like the ratio of nonagricultural to total GDP is that "The Development Plan (which we use as the basis of our future GDP figures) does not discuss this ratio." This is incorrect. Kenya's Development Plan, 1970-1974 explicitly gives sectoral growth projections and targets on pages 141-145 and explicitly discusses growth prospects in each of the sectors - including the sectors other than agriculture. Thus, as an instance, pages 305-310 and 313-314 contain a description of likely developments in manufacturing industry which would be quite adequate for the discussion in Chapter 9 of the Thias-Carnoy study.

d. The final point on which there is some disagreement relates to the advisability of attributing GDP originating in Kenya's "monetized sector" to about 550,000 to 650,000 workers. Thias and Carnoy imply that I am making "the trivial point that even a 99% coverage is fractional vis-a-vis the 100% ideal." The point is far from trivial. Kenya's labor force data is appalling and the GDP series (including its partitioning between GDP originating in the 'monetized' and that originating in the subsistence sectors) have been very considerably revised over the last three to four years. Broadly speaking, about three-quarters of Kenya's GDP is attributed to the monetized sector. Of Kenya's total population of about 10.2 million[1/] about 53%, say 5 million, fall in the age groups 15 to 55. About 1.03 million are registered as 'self-employed', about 2.10 million as 'family workers' and about 1.03 million as being in 'wage employment'. Of the last segment about 65% - i.e. roughly 650,000 are said to be in the 'modern' sector. Large numbers of workers in the 'self-employed' and 'family worker' categories are known to drift in and out of the 'wage employment' group just as they are known to move in and out of the 'modern sector'. In their regressions Thias and Carnoy ignore the effects of these movements from one category to another and indeed disregard the extent to which the 'self-employed' and the 'family workers' - even without crossing over to the 'employed for wages' category - constitute competing groups for those who are registered as being 'employed for wages' in the 'modern sector'. The GDP that is said to originate in the 'monetized' sector results from the labor of all categories of workers and not just those in 'wage employment' in the 'modern' sector. Thias and Carnoy simply regress the latter

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1/ All figures relate to 1968.

against the 'real wage' and the value added in the 'monetized' portions of the sectors and as they get good fits they assume that they have a satisfactory model for explaining and projecting sectoral employment in Kenya. Good fits are very tenuous evidence regarding the adequacy or justifiability of a model. As an illustration, on taking their employment data for Africans and making it simply a function of time I find that a simple quadratic equation has R^2 of 0.817, 0.817 and 0.871 for their three sectors. These are not much worse than the 0.931, 0.950 and 0.766 they obtain using 'real wages' and sectoral GDP and yet I suppose many of us would find a model with time as the only explanatory variable as being very far from adequate.

4. The considerations I have outlined suggest that there was scope for exploring a number of alternative and more comprehensive hypotheses and that the study did not attempt to do so.

5. I turn now to the equations of Chapter 5. In their note Thias and Carnoy correctly argue that their use of dummies is in many ways similar to analysis of variance procedures. This is an important point and the chapter would gain a good deal if they were to elaborate on this. The rest of their argument seems to be that there is some inherent reason on account of which cross section data always has a great deal of random 'noise' and that for this reason even very low R^2 should be acceptable. I suppose this is partly a matter of preferences. I have a number of results available for inspection which report very high R^2 (above 0.9) for cross section data on fertilizer consumption in Indian districts. Cross section data is not inherently more unsystematic than time series data and there seems to be no reason why our standards should be more generous in one case rather than the other. Just as there are a number of factors (e.g., trends) that can affect estimates of coefficients in time series data and result in spurious correlations so are there many factors (e.g., effects of scale) that can have similar consequences in cross section data. I have collected a good number of examples to illustrate this point, which again I would be glad to make available.

AShourie:lcm

cc: Members of the Editorial Committee

OFFICE MEMORANDUM

TO: Messrs. P.D. Henderson and Herman G. van der Tak **DATE:** July 22, 1970

FROM: Arun Shourie

SUBJECT: Mr. Holm on Regression Analysis

1. Mr. Silcock sent me a copy of a memorandum by Mr. Holm that contains a number of assertions about what he thinks I think about regression analysis. I would like to comment on some aspects of his memorandum.

2. On examining a number of studies utilizing regression analysis and in working with a good deal of data myself I have come to two conclusions about regression equations. First, that we are very seldom justified in looking upon a coefficient (say b_i) in a multiple regression equation as indicating "the change in Y as a consequence of a one unit change in X_1." There are many reasons for this. One of these is the fact that some explanatory variables may have been left out of our model and that one or more of these may be collinear with the ones that have been included. Second, that in some circumstances the equation as a whole can be useful even though we cannot attach any causal interpretations to its coefficients. For example, if we are interested solely in forecasting then we may be satisfied with an incomplete model; we may, for example, have reason to believe that in the period for which we are making a forecast the excluded and included variables and the excluded and the dependent variables will continue to be related in the same way as they have been in the past. But I believe that in all such cases we should present the reasons for our belief that these relationships among variables in the future will remain the same as in the past. Unless we do this we are only asking the reader to join us in an act of faith.

3. Mr. Holm has three things to say about this position. First, that it is not novel; second, that it is "extreme" and constitutes an attack on "methods developed by Fisher, Tingergen, Haavelmo, Wald, Theil, Zellner, Koopmans, Samuelson, Frisch, Malinvaud, Sargan, Kendall, Durbin and others"; third, that it is wrong and reveals my "misunderstanding of the nature and properties of residual errors in econometric models."

4. I have not claimed any originality. In fact, the theme of my paper on the misuse of econometric techniques is that elementary theory is often disregarded in practice; and I argued that we should pay greater attention to textbooks that "so often warn us against doing so many of the things we do."

5. Invoking a long list of names gets us nowhere. So far as I know, there is nothing in the writings of any one of the authors listed above that contradicts the position I have outlined here and in my earlier paper. In connection with the specific point about the significance of regression coefficients, Mr. Holm might like to know that I had sent my paper to Professor Maurice Kendall - who figures in his list of luminaries - and he had this to say:

> "It seems extremely difficult to convince econometricians that no reliance can be placed on the value of the individual coefficients unless the regressors are really independent. If anything I would put it more strongly than you have."

6. From the way in which the matrix algebra of Mr. Holm's appendix is written up it seems that he thinks that he has demonstrated two things: first, that coefficients are not biased even when some of the excluded variables are collinear with the ones that have been included; second, that my argument is relevant only when the number of excluded variables is the same as the number of included variables. Both these deductions are demonstrably wrong.

7. As for the first, I have already given a simple proof in my paper. In the context of that paper an expression for the error term should have been included, but the fact that it was not does not affect the validity of the proof in any way. The argument is equally applicable to stochastic and non-stochastic models. A similar proof is furnished in texts dealing with specification errors in regression models. Theil in his paper in the Review of the International Institute of Statistics, Vol. 25, and Draper and Smith, Applied Regression Analysis, pages 81-84, furnish the proofs with the error term. Professor Box's paper "The Use and Abuse of Regression," Technometrics, Vol. 8, No. 4, states the argument without the error term. Snedecor and Cochran's Statistical Methods, pages 394-398, discusses the issue in a more intuitive way without using matrix notation.

8. Moreover, the argument is completely general and does not require that the number of excluded variables be the same as the number of included variables. Draper and Smith give a simple arithmetical example in which the postulated model has a constant term and the first power of one explanatory variable alone while the true model has two additional explanatory variables. They and also Snedecor and Cochran work out examples in which the postulated model has a constant term and one explanatory variable while the true model has one additional variable. The point can be readily clarified by employing the matrix notation. For simplicity I will omit the error term and shall indicate the dimensions of a vector or a matrix by subscripts. Let us assume that the true model is

$$E(Y_i) = X_{ij}\beta_j + Z_{ik}\theta_k \qquad (1)$$

where $i = 1. . . . . t$ (the number of observations)
$j = 1. . . . . n$
$k = 1. . . . . m$

and let us specifically postulate that $j \neq k$ - that is, the number of variables in the Z-matrix is not the same as the number of variables in the X-matrix. Mr. Holm says that in this case my argument about a specification - bias in the estimated coefficients (b_j) is inadmissable. Let the postulated model be

$$E(Y_j) = X_{ij}\beta_j \qquad (2)$$

Then the least squares estimates of β_j are given by

$$\begin{aligned} E(b_j) &= (X'_{ij}X_{ij})^{-1} X'_{ij}E(Y_j) \\ &= (X_{ji}X_{ij})^{-1} X_{ji}(X_{ij}\beta_j + Z_{ik}\theta_k) \\ &= (X_{ji}X_{ij})^{-1} X_{ji}X_{ij}\beta_j + (X_{ji}X_{ij})^{-1} X_{ji}Z_{ik}\theta_k \\ &= \beta_j + (X_{ji}X_{ij})^{-1} X_{ji}Z_{ik}\theta_k \qquad (3) \end{aligned}$$

As will be clear from the subscripts the matrix resulting from

$$(X_{ji}X_{ij})^{-1} X_{ji}$$

is (j x i). As Z is (i x k) the two are conformal and there is no reason to assume that the number of excluded variables (1. . . . . m) is the same as the number of included variables (1. . . . . n).

9. Mr. Holm says "I must confess I do not know what an optimal estimator is." Perhaps Theil's allusion to a least squares estimator not having "optimal Markov properties" unless the variance of the errors is σ^2 (Theil, op. cit., p. 42) will prove to be suggestive.

10. On the issue of the Thias-Carnoy study Mr. Holm insists that my position on regression analysis reflects some "unrealistic ideals" of mine in that it "requires the impossible, namely unlimited evidence." It is very difficult to decide what is 'realistic' and what is not. In my comments on the study I was only making a plea for something that would be defensible. And the fact is that much of the text of the Thias-Carnoy study and some of the things that are deduced from the results in it are not defensible.

AShourie:lcm

cc: Members of the Editorial Committee

INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Herman G. van der Tak DATE: July 9, 1970

FROM: Soren Holm

SUBJECT: Comments on Mr. Shourie's Criticisms of the Kenya Study

In the meeting of the editorial committee where the Kenya study was discussed, Mr. Shourie argued that the study should be published to show the failures of using the multiple regression approach for cost-benefit analysis. Mr. Shourie concluded that the text should be drastically revised in order to emphasize these failures. He elaborated on this and other points in a memo to Mr. Stanley Please and it is this memo which I will address myself to in the following.

Let me begin by making it clear that I do not at all share Mr. Shourie's opinion as to the form in which the study should be published. Although he points to well-known limitations of multiple regression, and these should surely be spelled clearly out in the study, I can not discard this approach solely on the ground that it has limitations. All sciences work with methods which have limitations which are not conceptually different from the ones which the authors have been faced with in the Kenya study, and work with the same statistical methods as econometrics. After all, we only label it "econometric" since it deals with economic entities. Indeed many aspects of econometrics, such as multiple regression, variance analysis, statistical testing, and the concept of the correlation coefficient were developed and applied in other fields decades before they were used in economics. A theory can never be proven, all one can do is to present evidence for or against a theory and this evidence is never final. No work could be done if everybody had the unrealistic ideals of Mr. Shourie, who seems to require the impossible, namely unlimited evidence.

In the memo Mr. Shourie talks about the different approaches quantifying costs and benefits of outlays on education. He states: "One may, for example, follow the early work of Becker, Schultz, et al., and derive point estimates of income streams and other magnitudes from samples of persons with different educational background or attainments. One may go a step further and seek to adjust these estimates by some notional 'shadow prices' to take account, for example, of unemployment. If one is fortunate enough to have the data and the skill one may derive these shadow prices from a linear programming model." First, suppose for a moment that Mr. Shourie sets up the linear programming problem, then I would like to know by which method Mr. Shourie would estimate the coefficients which enter in the program. Would he use regression analysis? If so, then he is biting his own tail. If not, then I would very much appreciate it if he would inform me how he would do it. The crux of the matter is that the linear programming

problem would also be "limited by the same evidence". Second, while it is true that a linear programming problem may have a dual, and the dual prices can be interpreted as "shadow prices", it does not follow that a cost-benefit study can be cast in the framework of a linear programming problem. Would Mr. Shourie give me some references to studies where this has been done?

Mr. Shourie gives the frequency distribution, with respect to R^2's of the equations in the study. He points out that the R^2's are between 0.01 and 0.78, with the greater part on the low side. He states: "This is not an impressive array by even the most generous standard." It should be pointed out, which he does not do, that the number of degrees of freedom are such that the R^2's are significant. There are two explanations to the low R^2's. The first one, and the one he solely concentrates upon, is that these are specification errors in the equations. That is, some relevant variables have been left out, or wrong variables have been included, or variables have been included in the wrong form (e.g. as linear instead of non-linear). The authors are of course aware that specification errors are likely, and that more data, and better theories could increase the explained variance. The theories used were developments of those by Blaug, Denison - can Mr. Shourie inform us from where or whom we might obtain a better theory? Data limitations are common to every science; in my opinion one outstanding contribution of this paper is the enormous effort put into reducing this limitation. Moreover, the authors interpret the data as efficiently as possible, and their hypotheses are the best they could make given the present theoretical knowledge of educational processes. They do not pretend that the study is the "ultimate solution". Better theories will surely be conceived, but it is difficult to imagine that this could come about without the knowledge and insight obtained through studies like the one we are discussing here.

The second explanation for low R^2's is that the unexplainable error terms are random and irreducible. That Mr. Shourie does not consider the second possibility is in my opinion not likely to be an oversight: it is the second occasion when he has revealed his misunderstanding of the nature and properties of residual errors in econometric models.1/

It should also be made clear that Mr. Shourie is not merely attacking the Kenya study, but the whole methodology of econometrics. This attack on the Kenya study is an "application", as it were, of the philosophy in his

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1/ Mr. Shourie's negative paper on the value of the econometric method in economic research, upon which he bases his comments here, will be discussed in the appendix. It will be seen that in the theoretical part, which at best is a sloppy presentation of common knowledge and at worst a complete lack of understanding, Mr. Shourie completely misinterprets (and sometimes ignores) the role of the error term.

recent paper. This position is extreme. His stand is not only against Thias and Carnoy, but against methods developed by Fisher, Tinbergen, Haavelmoe, Wald, Theil, Zellner, Koopmans, Samuelson, Frisch, Malinvaud, Sargan, Kendall, Durbin and others.

There is of course considerable skepticism about the success of econometrics, not least on the part of the researcher who works with the data, but this is more due to the poor state of economic theory rather than econometrics, and this in turn is due to the fact that the economist has very little opportunity to do experimentation as compared to other sciences where "econometrics" has proven itself. However, the situation is improving all the time, thank to research like the Kenya study.

The study uses the following equation to estimate employment

$$\log N = a + b \log W/P + c \log Y$$

where N is employment, W is wage, P is the Nairobi cost of living index and Y is GDP.

Mr. Carnoy pointed out that this is "the well-known neoclassical hypothesis" and Mr. Shourie comments that this is "a bit rhetorical" and points to the fact that the assumptions for perfect competition will probably not be fulfilled. His remark also seems to be a bit rhetorical. The question is not whether or not it is a neoclassisal hypothesis, the question is how well the equation explains the demand for employment.

Mr. Shourie does not seem to appreciate the fact that econometrics is a tool - and nothing more - which is used in economics. Economic theory must provide us with the relationships among the economic entities under consideration, and it is then by applying econometric techniques that we accept or reject these relationships. Mr. Shourie, therefore, misconceives the proper role of econometrics when he argues that certain economic, or non-economic variables should be included in the equations, since they may increase R^2's, when in fact these variables were not included in the theory which is to be tested. In reality, of course, there is a give and take in both directions; that is, the economist may often adapt his theory to the data, but it would be wrong to include variables which he could not explain by economic theory.

Conclusions

I agree that the present Kenya study could, in its concluding paragraphs, make more clear the limitations of the methods employed; but I see no reason for changing it in any drastic fashion as Mr. Shourie would like to see. The authors, as well as myself, have never been dogmatic

about the completeness of the study. However, given the data and the state-of-the-art, there is nothing in his argumentation on a methodological level, which would exclude the use of multiple regression analysis in cost-benefit studies. On the contrary, it is an important stepping stone for further understanding of the cost-benefit approach and the educational process.

SH:csm

A P P E N D I X

Mr. Shourie's argumentation is based on the paper he has written: "The Relevance of Econometric Models for Medium- and Longer-Term Forecasts and Policy Prescription". At the time of the meeting I did not know the content of this paper, and I therefore felt that I could not participate in the discussion, although some of his arguments were rather unusual. I have now had a chance to read the paper, and I would in the following, like to make some comments on it. I will limit myself to the cases where it has a direct bearing on the Kenya study, as brought up in the meeting.

On page 4, in his paper, it is stated that: "The objective of econometric technique is to obtain unbiased, consistent and optimal estimates of individual coefficients and equations". As far as the literature on econometrics is concerned, unbiasedness, consistency, and optimality are properties of estimates and not the objective of econometric theory. J. Johnston[1/] states: "Economic theory consists of the study of various groups or sets of relations which are supposed to describe the functioning of a part or the whole of an economic system. The task of econometric work is to estimate these relationships statistically, and this empirical testing and measurement of economic relationships is an essential step in the acquisition of economic knowledge."

A.S. Goldberger[2/] defines econometrics in the following way: "Econometrics may be defined as the social science in which tools of economic theory, mathematics, and statistical inference are applied to the analysis of economic phenomena. Its main objective is to give empirical content to economic theory..."

The reason why I stress the point of the objective of econometrics is, that Mr. Shourie equates the desirability of having certain properties of the estimates with the main objective of econometric theory. It should also be noted, that the properties which Mr. Shourie has singled out are by no means the only properties of an estimator which one could choose, and his choice seems quite arbitrary. I must confess that I do not know what an "optimal estimator" is. According to the literature no "optimal estimator" exists. As pointed out above, this is a very narrow objective and moreover, it excludes a great proportion of econometric work which studies the situations where the aforementioned properties are not fulfilled. To give an example, Mr. Shourie would never consider maximum likelihood estimates, since such estimators are only consistent, asymptotically unbiased and asymptotically efficient. A maximum likelihood estimator is in general not unbiased.

1/ J. Johnston: "Econometric Methods", McGraw-Hill, 1963, p. 3.

2/ A.S. Goldberger: "Econometric Theory", John Wiley & Sons 1964, p. 1.

By limiting himself to his stated objective, he limits his study, and therefore his conclusions, and it is not correct of Mr. Shourie to argue the Kenya study from such a narrow and unorthodox basis, when the authors of the study have taken the more general and orthodox position as to what constitute the objective of econometrics.

As mentioned above, I will only discuss his paper insofar as it has a direct bearing on the discussion of the Kenya study, so I will only concentrate on the section "(b) Specification Errors: The choice of Explanatory Variables" which starts on page 20.

Mr. Shourie states: "The standard argument for least squares proceeds as follows...." Taking into consideration his stress on rigor, I do not consider myself pedantic if I suggest the following rephrasing: "The best linear unbiased estimates (BLUE) is derived as follows..."

He continues: "Let Y, X and Z be matrices and b, β and θ be vectors; assume that the model is

$$Y = X\beta \qquad \text{(i)}$$

Least squares procedures estimate β by b where

$$b = (X'X)^{-1} X'Y \qquad \text{(ii)}$$

Substituting for Y from (i)

$$b = (X'X)^{-1} X'X\beta \qquad \text{(iii)}$$

But $(X'X)^{-1} (X'X) = I$ and so

$$b = \beta$$

that is, b is an unbiased estimator of β ."

This derivation puzzles me, since it is completely different from anything found in any standard textbook.

The significant error of this "proof" is that the assumed model should not be as given in (i) - at least not according to econometric theory - but should be

$$Y = X\beta + \varepsilon \qquad \text{(i')}$$

where ε is a vector of errors, which must fulfill some conditions, the important one, for the future argument, being that $E(\varepsilon) = 0$ where $E(')$

is the expected value of the argument. The substitution in (iii) therefore becomes

$$b = (X'X)^{-1} X'(X\beta + \varepsilon) \qquad \text{(iii')}$$

and the right argumentation in (iv) is

$$E(b) = (X'X)^{-1} X'X\, E(\beta) + (X'X)^{-1} X'E(\varepsilon) = \beta$$

Since $(X'X)^{-1} X'X = I$, $E(\beta) = \beta$ and by assumption $E(\varepsilon) = 0$. Note that it is $E(b) = \beta$ and not $b = \beta$.[1/]

It should also be mentioned that Mr. Shourie has only concentrated upon the limited case where X is a matrix which is fixed in repeated samples, and not taken into consideration the general, and more realistic case, where X is a sample from a stationary mutlvariate stochastic process.

I will later come back to the error term ε. For the time being I will just point to his statement that: "The standard argument for least squares..." Least squares of what? It is the least squares of the error terms, and it is therefore strange that he does not include the error terms in his derivation.

Mr. Shourie continues: "If however, the correct model is

$$Y = X\beta + Z\theta \qquad \text{(v)}$$

while the model used for estimation is

$$Y = X\beta$$

that is, if some of the explanatory variables (Z) have been left out because the analyst misconstrued the causal relationships or because he could not obtain satisfactory data for them or for some other reason, then

$$b = (X'X)^{-1} X'Y$$

Substituting once again for Y - but this time from (v),

$$b = (X'X)^{-1} X'X\beta + (X'X)^{-1} X'Z\theta \qquad \text{(1)}$$

$$= \beta + (X'X)^{-1} X'Z\theta \text{ or}$$

$$b = \beta + \delta\theta$$

where δ gives X as a function of Z($X = \delta Z$). Thus, 'b' can be an under- or over- estimator of β."

1/ For this derivation, see Goldberger, p. 162.

Again we need the error term ε. That is, (v) should be

$$Y = X\beta + Z\theta + \varepsilon \qquad (v')$$

The derivation of (1) should be as follows:

$$E(b) = (X'X)^{-1} X'XE(\beta) + (X'X)^{-1} X'ZE(\theta) + (X'X)^{-1} X'XE(\varepsilon)$$

$$= \beta + (X'X)^{-1} X'Z\theta \qquad (1')$$

Since $(X'X)^{-1} X'X = I$, $E(\beta) = \beta$, $E(\theta) = \theta$ and by assumption $E(\varepsilon) = 0$.

If, as stated in the paper, $X = \delta Z$, which is an extremely limiting assumption as seen in the following, then

$$E(b) = \beta + (Z'\delta'\delta Z)^{-1} Z'\delta'Z\theta$$

Case A. δ is a scalar.

$$E(b) = \beta + (1/\delta)\theta$$

and <u>not</u> $b = \beta + \delta\theta$ as given in Mr. Shourie's paper.

Case B. δ is a matrix.

$$E(b) = \beta + (Z'\delta'\delta Z)^{-1} Z\delta'Z\theta$$

Mr. Shourie does not distinguish between cases A and B.

For the sake of argument I have gone along with his derivation, but I must confess that I do not understand his argumentation.

Case A is only defined if Z is of the same dimensionality as X; that is if Z contains as many variables as X. Granted that Z and X are the of same dimensionality then it follows from the fact that δ is a scalar that Y is completely determined by X and the argument is therefore meaningless, which can also be seen from the following:

$$Y = X\beta + Z\theta + \varepsilon$$

$$= X\beta + (1/\delta) X\theta + \varepsilon \qquad (X = \delta Z)$$

$$= X(\beta + 1/\delta\ \theta) + \varepsilon$$

$$= X\beta_1 + \varepsilon$$

But this is exactly the model we assumed!

We now turn to case B. Again it is true that Z must have the same dimensionality as X in order for the problem to be defined. It is true that b may be biased in this case. Mr. Shourie has restricted himself to the very special case where Z is of the same dimension as X. Taken into consideration his aim with this analysis, namely that excluded variables may result in a biased estimator, he has showed almost nothing. His analyis is only valid if the number of excluded variables are the same as the included variables; and why should this be the case? Nevertheless, when he criticized the Kenya study he never paid any attention to this limitation.1/

We now come to the crux of the matter and a point which Mr. Shourie does not take into consideration, namely, the interpretation of the error term.

Let us assume that the true model is

$$Y = X\beta + Z\theta + \varepsilon_1$$

but we use the model

$$Y = X\beta + \varepsilon_2$$

now these two models imply that

$$\varepsilon_2 = Z\theta + \varepsilon_1$$

that is, in the second model the error term includes the excluded variables. Therefore, what is the error term? The error term takes into consideration all the excluded variables and we are interested in measuring the degree to which the included variables explain the dependent variable. It is of course true that ε_2 must fulfill the same kind of conditions as ε_1 but insofar that this is the case then it is not methodologically wrong to use the model

$$Y = X\beta + \varepsilon_2$$

instead of

$$Y = X\beta + Z\theta + \varepsilon_1$$

the only difference being that one could probably explain more of the variance in the second case than in the first case. Moreover, if we use

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1/ For the general case see Goldberger, p. 196.

$$Y = X\beta + \varepsilon_2$$

instead of

$$Y = X\beta + Z\theta + \varepsilon_1$$

then b will still be an unbiased estimator.[1/]

Conclusion

I have in this appendix concentrated on Mr. Shourie's basic philosophy as to what constitutes the objective of econometrics and on his ideas with regard to the specification of equations. Both are relevant to the Kenya study, the first because it shows that Mr. Shourie has a completely different conception of the objective of econometrics than the authors of the Kenya study who are in agreement with orthodox econometric theory. I concluded that it is not appropriate for Mr. Shourie to criticize the authors on a methodological level from such an unorthodox basis.

The second point is relevant since a great deal of Mr. Shourie's argumentation deals with the problem of excluded variables. I have demonstrated that Mr. Shourie's theoretical speculations do not justify his more than critical remarks with respect to this problem. No doubt the exclusion of variables is a problem in econometrics; however, Mr. Shourie has contributed nothing which is not known to the authors and the econometric community at large.

1/ For the conditions under which b will be an unbiased estimator see Goldberger, p. 197.

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: July 15, 1970

FROM: T. H. Silcock

SUBJECT: Meeting of the Editorial Committee

A meeting of the Editorial Committee will be held on July 28, at 3:30 p.m., in Conference Room D-560.

A paper by Per Eklund on Taxation and Earmarking in Developing Countries is circulated, and the views of members of the Committee would be welcome, as one of the items of business will be the preliminary consideration of this paper.

The editor wishes to raise a number of other items of business and the agenda will be circulated shortly.

cc: Mr. Pryor
Mr. Hoffman

INTERNATIONAL DEVELO' IT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT . INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. H.G. van der Tak DATE: July 10, 1970

FROM: M. Carnoy/H. Thias

SUBJECT: Kenya Case Study - Some Further Comments and Explanations

The comments on the technical aspects of the Kenya cost-benefit study seem to result from a new awareness on the part of many in the Economics Department of the Bank that empirical work should be scrutinized carefully. In general, this new awareness can only serve to make Bank studies much better and Bank researchers more careful. However, the Kenya study has been already rather careful on this score. The authors recognize data difficulties where they arise, discuss alternative methods of making projections, make alternative projections, admit the possibilities of misspecification where they might occur, and try to predict the direction of biases in the results. For the most part, the results of the study are not subject to the common errors made in regression analysis. Our critics, in responding to a general problem of empirical studies done at the Bank, have overreacted to the results in the Kenya study. As we discuss below, their comments on the technical work are a mixture of quite reasonable objections, statements which have no foundation in fact, and criticisms which arise from a careless misinterpretation of the data and statements in the text.

(i) Defense of regression estimates in Chapter 5: The regression estimates of age-income profiles are made within levels of schooling. They are based on cross-section data on individual wage-earners. In the econometric literature, an R^2 of 0.4 is considered "very good", or even "excellent" for cross-section analyses of this type. There is just so much random "noise" in such cross-section data, that higher R^2's are difficult to achieve. In nine out of eleven schooling levels, we achieve R^2's of 0.2 or above with just four independent variables (Annex Table 5.3, 5.7, 5.8), and 0.3 or above in five schooling levels.

Furthermore, by making schooling a parameter, we remove one of the most important explainers of income variance among wage-earners in Kenya (10 percent of the variance to be exact) when schooling alone is run. In an earlier version of the manuscript we included a regression of earnings on age, socio-economic background, etc., including schooling as a variable instead of parameter). "Parametrizing" schooling enables us to take account of the interaction effect of schooling with age and particularly socio-economic class. Thus, we are looking at a greatly reduced set of income variances when we look at income variation within schooling level. In Table 5.3, we relate income (earnings) to one variable, age. As we would expect from all the age-income profiles reported to date, the slope of income with age increases with additional schooling. People with more schooling evidently have more opportunity to invest in themselves after they finish school (Becker and Mincer articles in JPE, Oct., 1962), supplement). Age is therefore a much better explainer of earnings variance at higher levels of schooling than at lower. It is important to note that the F values of the 1 year and 4 years of schooling regression estimates are significant at the 1 percent

level of significance, and the regression coefficients for the early age group (most important for our analysis -- see below) at the 5 percent level.

This means that the regressions as such are good and the estimated parameters unbiased. The fact that the R^2 for low levels of schooling is small means that the population variance due to random noise (some of which we explain in Tables 5.3 and 5.5) is high, but not that the expected value should not be used to estimate age-earnings differentials. It appears from the criticism that this point has not been fully understood. Also, the low R^2 does not imply that the age-earnings profile we have estimated for those with 1 year of schooling (R^2 of earnings regression = .01) is highly variable in absolute terms. It is crucially important to see that in the calculations of the rate of return we have compared the absolute differences and these do not have such a high variance as suggested by the R^2, especially at early ages in the earnings profile. The mean earnings of those with 1 year of schooling in our sample is 321 Ksh per month, the standard deviation of the mean 173, and the number of observations 1079. At a 95 percent confidence level the mean earnings of the group with 1 year fall between 310 Ksh and 332 Ksh per month. For those with 4 years of schooling, mean earnings equal 355 Ksh per month, and at a 95 percent confidence level, they fall between 343 and 367 Ksh per month. Eventually, by taking account of variables which depend on decisions made by the wage-earner <u>after</u> he finishes school and involve choices of where to live, what sector of the economy to work in, the size of firm he works in, etc., we explain about 30 percent of the income variance at the lower levels of schooling. These choices may be linked to an ability or motivation factor; they may involve sacrifice of non-pecuniary returns for pecuniary returns; or include connections to others; or they may simply be reduced to luck -- being at the right place at the right time.

If, in fact, we rely on the variance about the mean to show that our estimates are reasonably stable, why do we use regression analysis at all? Conceivably we could have even divided male Africans not only into schooling groups, but age groups as well, and calculated a mean within each schooling/age group. For one thing, the regression estimates do exactly that and, in addition, tell us whether the earnings figures estimated for each age group within a schooling group are significantly different from each other. Secondly, and more important, regression analysis allows us to systematically account for socio-economic background and other effects on earnings in a relatively small sample (as contrasted with a census which can be divided into large numbers of cells). The regression estimates tell us whether a particular variable has a significant effect on earnings at that schooling level. "Ideally" we might want to divide the sample down into socio-economic, age, and schooling categories, and to estimate mean earnings in each of the sub-categories. We would have to test the significance of each of those means against others.

From a policy point of view, the R^2 of a regression estimate has to be viewed in the context of its use. An educational planner or even a "typical" individual has to make decisions on the basis of data available to him, and variables that will behave systematically in the

future. We would expect that for those with high levels of schooling in Kenya, a group which currently faces a well-organized, full-employment labor market, we can find systematic variables which will explain a high fraction of earnings variance even within level of schooling. At lower levels of schooling we should be prepared to expect a large fraction of income variance to be simply due to chance. Chance is defined as unpredictable and uncorrelated with any of the included independent variables. Perhaps what we view as chance is really motivation, but we may have no measure of motivation which will correlate well with the unexplained variation in earnings. In either case, planners may be left with a low R^2 of regression estimate, but, as we have shown, a highly predictable mean earnings by age within schooling level (the coefficients of the low age groups--most important in the rate of return estimates because of their higher weight in discounting -- are significant in all the regressions shown in annex in Tables 5.2 and 5.3, except for one case). Furthermore, without doing very detailed studies of students in school, or the components of the relationship between family and child, or discrimination in the labor market, it may not be possible for us to reduce the 3 percent error in the mean at the 1 and 4 years of schooling levels down to, say, 1 percent. That does not mean that we shouldn't try to get more accurate estimates or even more careful measurements of the variables we already have, but in defense of our present estimates, we must say that for planning purposes they are very good in comparison to other available guidance. At higher levels of schooling, of course, we do even better. The variability in the mean income increases at these levels, but a higher percentage of that variation is explainable by variables we include in the regression.

To conclude, we note that a great deal of experience has been gained by econometricians in cross-section analysis (see, for example, the vast literature on estimating price and income elasticities from income-expenditure surveys). Although it may appear at first glance that small R^2's indicate "weak" economic results, experience and the argument discussed above show that this causal interpretation is far from accurate. Our critics do not seem to know that in cross section analysis, R^2's greater than 0.2 are regarded as fair; greater than 0.3 as good, and greater than 0.4 as excellent; an examination of the literature in this field will bear this out.

Perhaps all these points should be clarified in the text of the study, and we are more than willing to include such a clarification in Chapter 5 of the present manuscript, where the age-earnings profiles are estimated. However, we would like to stress once more that the techniques and its implications have been discussed elsewhere. We have referred to much of the relevant literature in our paper. The technique is a good one and has enabled us to make corrections on the rates of return which had not been made before. The results are defensible in statistical, economic, and educational terms. An attack on the technique of regression analysis regardless of the context in which it is employed, shows a basic misunderstanding of the whole concept of regression analysis. Regression estimates have often been used incorrectly to "create" a theory, and have not been well used in statistical terms to

estimate the effect of observable phenomena. We contend that neither of these criticisms does justice to the results of Chapter 5. We believe that the literature in the economics of education, including the work of outstanding econometricians such as Griliches, Hanoch and Bowles, bears us out.

(ii) We agree with Mr. Shourie's point on Chapter 7. As he himself mentions, the text goes out of its way to point out that the regression estimates are not good enough to make policy conclusions. However, it apparently was not clear enough that we were trying to demonstrate a method, not to arrive at policy-oriented results. Again, these are cross-section data, and the results are not nearly as poor as they are made out to be by our critics. The results of equations 7.13-7.19 and equation 7.21 are very good as cross-section estimates go. We make the point both in the text and here that they should be interpreted and used with care. We can modify the text to make the limitations of the estimates even more apparent to the reader.

(iii) There is little on which we can agree with our critics regarding Chapter 9. We do concede that the statement to which Mr. Shourie refers (pp. 185-186) should be eliminated. It is no way crucial to the study, nor is it, as he points out, adequately substantiated by the regression estimates. The statement is not made to support the use of the equations as an improvement on ordinary manpower analysis. That connection is a figment of Mr. Shourie's imagination. In assuming higher elasticities for real GDP and lower for real wages in our equations for employment of higher level skills, we can refer to other literature besides our estimated equations.

The rest of the arguments about these equations are simply wrong. The high R^2 cannot be summarily dismissed. To suggest that employment is functionally dependent upon exports and imports, rather than upon GDP, is freak economic theory. The correlation coefficient (r) of our independent variables in equations (9.5) and (9.8) is high (about 0.8), but it should be remembered that with 9 degrees of freedom r must be greater than or equal to 0.65 to be significantly different from 0 at a 5 percent confidence level. In any case, multicollinearity does not result in biased estimates of coefficients as Mr. Shourie seems to think rather, it causes an increase in the estimated variance. This does not appear to cause any problem in our case since the t-values are high inspite of the slight multicollinearity that may be present.

Including a simple linear time variable does not improve the R^2's of the regression estimates (9.5) and (9.8) as Mr. Shourie claims it would. Neither are the coefficients of real wages and real sectoral GDP affected by the inclusion of the time variable. They are "robust", in Mr. Shourie's words. Given the logic of the specified equations and the high resulting R^2, there is no a priori evidence of misspecification. The ratio of value added in non-agricultural sectors to total GDP might be a useful variable

in improving the equation; however, as Mr. Holsen would argue -- and we believe somewhat correctly -- it is not easy to predict its future behavior in a country like Kenya. The Development Plan (which we use as the basis of our future GDP figures) does not discuss this ratio. Besides, the fact that there are other variables available which might move our R^2 from 0.95 to 0.97 is not really Mr. Shourie's point. He is saying that other variables exist that are highly correlated with real GDP and with real wages. This alone, however, does not mean that the model is misspecified, nor does it imply that the coefficients of the variables used are biased. The price of beef in Nairobi (or, for that matter, the number of elephants in the game parks) may be highly correlated with real wages of Africans in industry and commerce. Does it follow that not including either of them in our equation biases the coefficient of real wages with respect to employment? Besides producing a significant coefficient and contributing appreciably to the R^2, the variable(s) introduced should also have the trifle(!) attribute of making economic sense.

Mr. Shourie's last paragraph is certainly enigmatic. We cannot understand why and how "some index of the marginal revenue product" should be used as a deflator of nominal wages. We make further very clear in the text and the tables that the employment, earnings (wages), and gross domestic product figures correspond to the monetary sector only. Thus, all the figures in the agricultural sector estimates refer to the total of those in the monetary sector of agriculture only as indicated repeatedly by the use of the term "commercial agriculture" (p. 180). Projections later on in Chapter IX again emphasize this point. Mr. Shourie's assertion that "...there is little in neoclassical economics that could justify our taking data about number of persons employed in a sector that covered only a fraction of the labor force in that sector" manages to combine a non-sensical use of the term "labor force" with a gross misreading of our data (unless he wants to make the trivial point that even a 99% coverage is fractional vis-a-vis the 100% ideal), which is surprising for a person as familiar with Kenyan macroeconomic data as he is. The wage figures refer to earnings per annum, not wages per hour. If Mr. Shourie means that one should use man hours as the dependent variable and the real wages per hour as the independent variable, he is right. Why doesn't he find such data over time for any sector in Kenya? Our earnings figures are not corrected for hours worked per week and number of weeks worked annually, but the data in Kenya simply do not permit these adjustments. However, that is not enough to make our estimates invalid or even unreasonable.

Comments on Mr. Holsen's memo and remarks at the editorial seminar

Mr. Holsen's written comments center on the idea that the model developed in Chapter IX is misspecified. Our analysis certainly does not assume that the demand curve was stable during the 1957-1966 period. The

real sectoral GDP is our shift term. As can be seen from the figures in Annex Table 9.3, the sectoral GDP in agriculture or industry and commerce during 1960-1963 did reflect pre-independence fears and anxieties. By the same token, real GDP in industry and commerce grew rapidly after independence. Couldn't this variable be important in explaining the "turning point" referred to in paragraph 4 of Mr. Holsen's memo? In fact, our equations (9.5) and (9.8) indicate that for Africans in these two sectors, real sectoral GDP is a significant variable in explaining employment before and after independence.

Mr. Shourie did not "rightly point out the identification difficulties" since the model is well specified (from a single equation standpoint), at least in equations (9.5) and (9.8). We doubt that higher wages in agriculture are due to the shift in roles occupied by Africans, since there was a decrease of only 0.3 thousand Europeans and Asians in the period 1957-1966. There are over 200,000 Africans employed in the monetary sector of agriculture. Similarly, in industry and commerce, there was a net increase in the number of Europeans and Asians employed between 1957 and 1966. Surely, there has been Africanization, but its impact is small against the total number of Africans employed in these two sectors. In fact, in the equations for the government sector, we show that independence is a very important variable in determining the number of employed of all three groups, and that real GDP and real earnings are not adequate explainers of employment. Since independence tends to be correlated with real earnings, not real GDP, in the government sector (see equations 9.11-9.13), Mr. Holsen is right about the effect on wages of the Africanization program in the government sector. But what are we left with when we do use the independence dummy? African employment in the government sector increases exponentially at a constant rate. Does Mr. Holsen want us to use that estimate as a basis for projections? Isn't it much more likely that as the economy moves away from the immediate post-independence period, it will continue to exhibit employment trends in the non-government sectors which are based on economic rationale?

The paradox mentioned on page 190 of the text referred to the positive elasticity of substitution estimated in a completely different model. That result could arise from a number of effects other than Africanization. In fact it probably derives from quite the opposite of Africanization -- big firms with more Europeans and Asians employed per worker than small firms.

Mr. Holsen brought up two points in the seminar which should be dealt with here, since we are not satisfied with the answers given at the seminar.

1. Mr. Holsen's statement: In the Chapter 9 estimates, the sectoral wage bill of Africans decreases as sectoral GDP increases. Our

answer: The wage bill of Africans decreases in agriculture and remains about the same in industry and commerce. The elasticity of real GDP is 0.67 in both sectors. If the new path of real earnings would be the same as during the period 1957-1966, employment would decrease 0.5 percent for every 10 percent rise in real GDP in monetary agriculture, and increase 1.1 percent for industry and commerce. Real earnings of Africans during 1957-1966 increased on the average by about 3 percent annually in agriculture and by 4 percent in industry. A ten percent rise in GDP over two years thus means about a 6 percent rise in real wages in agriculture and a 8 percent rise in industry and commerce. The wage bill of Africans in agriculture decreases by (-1.2(6) + .67(10), or -0.5 percent for a 10 percent rise in GDP in agriculture. Similarly, employment in industry and commerce rises by (-0.7(8) + 0.67(10) = 1.1 percent. The wage bill thus increases 1.06 (.995) = 1.055, or 5.5 percent for each 10 percent increase in agriculture GDP. Similarly, the wage bill in industry and commerce rises by 1.08(1.011) = 1.092, or 9.2 percent for each 10 percent increase in industry GDP. These results are consistent with economic development theory.

2. Mr. Holsen's concern with using a historical demand curve as the basis for projections: We realize the pitfalls, but we still think our estimates are reasonable proxies of what will happen between 1968 and 1974. At least, none of the criticisms of Chapter 9 has convinced us that our estimates are "a disastrous example of how technicians .... can provide poor guidance for policy makers." In our work we are proposing a method of dealing with manpower planning problems. The alternative to this method -- manpower projections -- is currently being used in Kenya and many other developing countries with generally poor results. We show alternative employment results assuming various alternative real earnings. We vary our elasticities. We suggest in the text that the results not be taken in the strict sense, but merely in the order of magnitude sense. While we agree with Mr. Holsen and others that the policy implications of the study should not be pushed too far, we do not believe that our empirical work or our "methods used at some crucial points" do not "withstand careful examination." The results presented in this study are much, much more accurate and reliable than the educational planning data available to and relied upon by planners in almost any developing country in the world. Depending on the political preference function, they could be interpreted and used in a number of ways (Mr. Silcock's point), but at least they are available to the decision maker. Many more studies would have to be done before our approach could provide a "safe" method of generating policy guidelines. We have provided a comprehensive and useful start, nevertheless. To judge it exclusively by "absolute" theoretical standards of sorts rather than against the background of present "best practice" in this field is not going to help anybody, least of all education and manpower planners in search of an improved approach to their problems.

MC/HHT:csm

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OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: June 25, 1970

FROM: Stanley Please

SUBJECT: Cost-Benefit Analysis in Education: A Case Study of Kenya

I am attaching copies of memoranda I have received from John Holsen and Arun Shourie on the above study.

SPlease/lo

cc: Messrs. Thias and Carnoy

FORM No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Stanley Please DATE: June 24, 1970

FROM: John A. Holsen

SUBJECT: EDITORIAL COMMITTEE -- More on the Kenya Case Study of Cost-Benefit Analysis of Education

1. There appears to be wide agreement within the Editorial Committee on a number of points with regard to the subject study -- the desirability of moderating the claims with regard to policy implications, of considering some of the problems of statistical methods, of tightening up the presentation. I would like to stress my own very serious reservations about Chapters IX and X (which are the core of the study as far as policy implications are concerned). I find this part of the study a disastrous example of how technicians, applying what seem to be fairly sophisticated analytical tools, can provide poor guidance for policy makers.

2. My present concern centers on the demand curves for labor since the conclusions depend so heavily upon this analysis. In the meeting yesterday I emphasized the inappropriateness of using a historical demand curve for educated workers as a basis for future projections of demand when the society is making a major effort to "change the parameters." I have since taken another look at how the demand curves used were derived and I am more concerned than ever. Mr. Shourie rightly pointed out the identification difficulties when one does not have a fully specified model. A lot of work has been done on the conditions under which one can estimate demand curves; it is ignored in the study at hand. The response given at yesterday's meeting was simply that there is a lot of economic theory that says the demand for labor should be related to real wages and GDP.

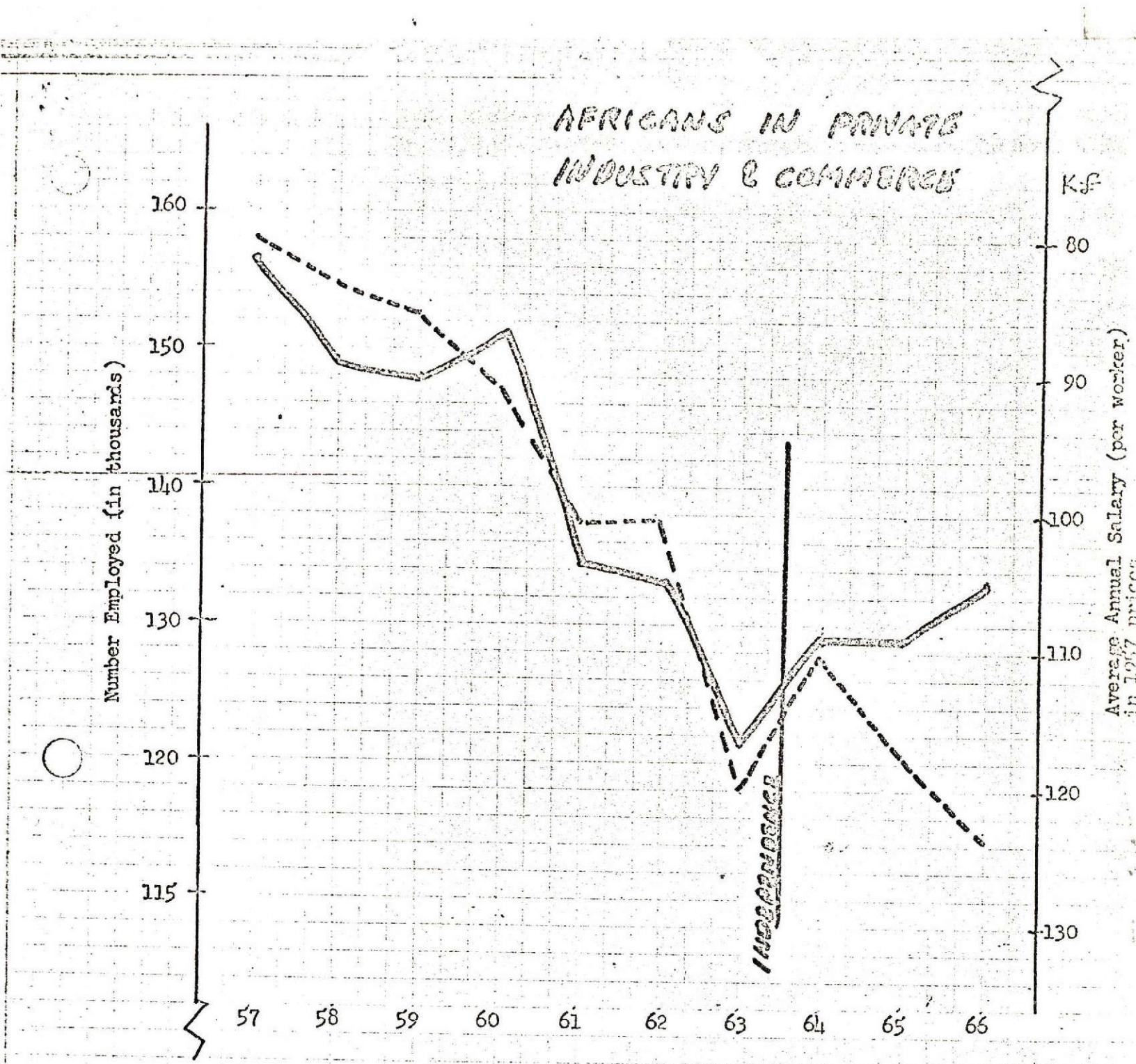
3. I have strong doubts about whether these were the only important influences during the period -- 1957 to 1966 -- used to estimate the demand curve for labor. The analysis necessarily assumes that the demand curve for labor was stable during this period, i.e., that the data give you points on the same demand curve. In reality it must have been shifting rather drastically. The future was highly uncertain; Europeans were leaving the country in large numbers. The sharpest drop in employment of Africans in commerce and industry took place between 1960 and 1963 -- when it fell from 151.1 to 121.8 thousand (19.4 percent). During these same years the number of Europeans occupied in the sector fell from 12.3 to 10.2 thousand (17.1 percent). I am confident that these two events are related! The situation in the agricultural sector is generally similar. (When the statistical analysis suggested this possibility, the authors dismissed it as a "paradox" to be explained away by the special nature of the statistics used; see page 190. William of Occam would have said, I'm sure, look first at the nature of a colonial society and its transition to independence.)

4. I attach a simple graph of employment and "real wages" in industry and commerce. It shows how strong the correlation is between "real wages" and employment prior to independence. But doesn't the trend in 1964-66 suggest that independence was a turning point? I think both common sense and the statistical evidence suggest a major change in the relationships after independence.

5. I have put "real wages" in quotes because the data simply are not wage data. They are, according to the title of Annex Table 9.1 "average annual earnings." Employers were under heavy pressure to "Africanize" their staffs during this period. There must have been a much larger number of Africans in relatively well paying jobs in 1966 than there were in 1957. One can't assume that wage rates were proportional to the average salaries of all Africans employed in a particular sector. I suspect that what is presented as a rapid increase in real wages is in large part the result of a shift in the roles occupied by Africans.

6. One could go on at considerable length on these and other points but time does not permit it. I suspect that the authors felt themselves under pressure to come up with some conclusions about the future demand for educated labor and consequently found themselves making whatever assumptions were necessary to do the job. To the person who does not understand and carefully go through the analysis, the results may look very "scientific" and therefore be convincing. From my sample review of the study -- and this is all I have really done -- I don't think the methods used at some crucial points withstand very careful examination. I am very much worried about the poor use of quantitative techniques in Bank economic work. I have no doubt that there is a great demand both in and outside the Bank for more sophisticated analysis directed toward practical problems. I agree fully that we should try to produce it. But our bosses will often not be able to judge its usefulness for reasons of time, temperament or training. It is therefore necessary that we technicians be very careful about what we do. I really think the last part of the paper -- pp. 177 on -- should be rewritten as a case study of the difficulties and limitations of this kind of approach; its audience should be other technicians more than educators and policy makers.

c.c.: Messrs. Thias
Shourie



Source: data from Tables 9.1, 9.2 and 9.3 of Annex of Report

CAUTION: NOTE SCALE FOR AVERAGE ANNUAL SALARY HAS BEEN REVERSED

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Stanley Please DATE: June 24, 1970

FROM: Arun Shourie

SUBJECT: Comments on "Cost-Benefit Analysis in Education: A Case Study of Kenya"

I would like to elaborate on some of the points I made at yesterday's seminar on the Thias-Carnoy study.

I. Introduction

In quantifying costs and benefits of outlays on education one can adopt one of or some combination of many approaches. One may, for example, follow the early work of Becker, Schultz, et al, and derive point estimates of income streams and other magnitudes from samples of persons with different educational backgrounds or attainments. One may go a step further and seek to adjust these estimates by some notional 'shadow prices' to take account, for example, of unemployment. If one is fortunate enough to have the data and the skill one may derive these shadow prices from a linear programming model. Yet another approach to isolating and quantifying these costs and benefits is through multiple regression analysis.

Thias and Carnoy rely on the last mentioned approach. Their quest is in principle a very legitimate one for under certain circumstances multiple regression analysis can indeed help us isolate and quantify the relationships between a dependent variable and a number of explanatory variables.

The principal outcome of the study - though the authors do not make this clear - is that in this particular case multiple regression analysis was of little help because of an inadequate data base, incompletely specified equations and some - perhaps unavoidable - collinearities among variables. Although this is a negative result, it is an interesting and useful one. Sometimes we should publish our failures so that they may forewarn others who may be using techniques similar to the ones we used for problems that are similar to the one we tackled. The Thias-Carnoy study is such a case: it should be published as an instance of the limitations of a particular approach.

A corollary of this is that the text should be drastically revised before it is published. In its present form the text conveys the impression that multiple regression has in fact helped a great deal and that it is the appropriate technique for quantifying costs and benefits of education. In his oral presentation Mr. Carnoy said that various authorities in the economics of education regarded the study as an important advance in methodology. It would be unfortunate if the Bank associates itself with this view. For in fact the study demonstrates quite the opposite.

II. Some Substantive Comments

Apart from tabulating a good deal of information and a general discussion of cost-benefit analysis and education the study attempts three things. First, it attempts to adjust the income profiles so as to remove the effects of factors other than education; second, it attempts to quantify the returns to outlays on education; and third, it attempts some projections of employment so as to compare them with the likely output of the education system. For each of these three objectives the study relies almost entirely on a host of regression equations.

(i) In adjusting the income profiles the criterion by which one should assess the regressions is the extent to which the total variance in the dependent variable has been accounted for by the equations. Annex Tables 5.3, 5.4 and 5.5 and the equations in the text have coefficients of determination from 0.01 to 0.78. The frequency distribution of the equations in Annex Tables 5.3, 5.4 and 5.5 is as follows:

1. Coefficient of determination	>0.0 <0.1	>0.1 <0.2	>0.2 <0.3	>0.3 <0.4	>0.4 <0.5	>0.5 <0.6	>0.6 <0.7	>0.7 <0.8	>0.8 <0.9	>0.9 <1.0
2. Number of equations	3	6	7	3	2	3	0	1	0	0

This is not an impressive array by even the most generous standards.

(ii) For quantifying the returns to education outlays one is interested not only in the overall explanatory power of the equations but also in the significance of individual coefficients. I have pointed out in another context that the study uses the coefficients to make statements that imply that if one, for example, raised teachers' salaries by £1 the average examination scores would increase by 0.09 points (para. 31, page 149). Now it turns out that a very large proportion of the coefficients are not significantly different from zero and that, as the text acknowledges, the equations are incompletely specified. Moreover, the coefficients are not robust: they vary a great deal with the specification of the equation. Consider, for example, the equations on pages 154-155 explaining average examination performance for each school: the coefficient linking average examination performance to number of students in Forms I-IV varies from 0.1012 to 0.0584, that linking it to expenditure per CSC student varies from +0.0114 to -0.0114, that linking it to percent boarders in Forms I to IV varies between 0.1117 to 0.0785. All this would indicate that the study should not take the coefficients as seriously as it does.

(iii) In Chapter IX - the one dealing with the future demand for educated labor - Thias and Carnoy wanted to project future demand in a way that would be different from the more conventional manpower approach. This latter approach - as Mr. Carnoy explained yesterday - does not take account of likely effects of changes in real wages. Thias and Carnoy assume that

they have now furnished us with a more comprehensive approach to manpower projections by introducing a real wage term in their regression equations. Their equations in Chapter IX take the general form

$$\log N = a + b \log W/P + c \log Y$$

where N is employment, W is wage, P is the Nairobi cost of living index and Y is GDP. Mr. Carnoy commended these equations on three grounds: first, that they provide a more flexible approach to manpower projections as they incorporate changes in real wages; second, that they are based on the "well-known neoclassical hypothesis that links employment to real wages and output" and, third, because the R^2 are very impressive. The text mentions an additional reason: "The regression results are consistent with two hypotheses, concerning the relative wage and price elasticities of skilled and unskilled labor, which we have occasion to refer to when estimating elasticities for the projections in Sections 4 and 5 of this chapter. The first hypothesis is that unskilled labor is more easily substitutable with other inputs than is skilled labor, i.e., that unskilled labor is more price-elastic than skilled labor (equations (9.5) and (9.8)). The second is that employment of more skilled labor is less sensitive to wage changes and more sensitive to GDP changes than less skilled labor (equation (9.6))." (pages 185-186)

The point about the high R^2 can be summarily dismissed: the R^2 would have been equally high if we had related employment to, say, exports, imports, a simple time variable and any of a host of other variables. Mr. Carnoy said that it would have been quite reasonable to include some of these other variables (e.g., the ratio of value added in non-agricultural sectors to total GDP as a variable explaining employment in the agricultural sector) in the equations and that they were not included because they were collinear with the variables that already figure in the equations. This precisely is the least defensible reason for leaving out an explanatory variable, if one is interested in individual coefficients as the authors in their quest for a more flexible model are. Some elementary algebra will show that the coefficients are biased when we leave out some explanatory variables because they are collinear with the ones that have been included.

Similarly, the point about "the well-known neoclassical hypothesis" is a bit rhetorical. As sectors like agriculture, Government, etc. do not operate in a perfectly competitive environment one can reasonably argue that variables other than 'real wages' and value added should be introduced into an equation explaining employment in individual sectors. For example, one could argue that what happens in sectors other than agriculture in regard to real wages and output affects the numbers employed in agriculture and that some variables should be introduced to take account of such intersectoral influences. Moreover, even if one took "the neoclassical hypothesis" about equating real wages and the marginal revenue product (a la Meade) at face value one would want to use a variable that compared money wages to some index of the marginal revenue product in each sector rather than just Nairobi's cost of living index. Finally, there is little in neoclassical economics that would justify our taking data about number

of persons employed in a sector that covered only a fraction of the labor force in that sector and which had not been adjusted in any way to take account of number of days worked, and relating this data to the value added in that sector as a whole. This procedure would require some fairly extreme assumptions about the three-fourths of the labor force that are not covered by the data and about the extent to which the data on numbers employed reflected the intensity of employment.

The points about providing a more flexible model for manpower projections and of the equations substantiating some hypotheses about skilled and unskilled labor would have deserved more attention but for two facts. First, seven of the twelve coefficients linking employment to real wages are not significantly different from zero; for three of the remaining five the coefficients are significantly different from zero but the standard errors of the coefficients are one-third to one-fourth of the coefficients so that the latter are not precise enough to be useful for policy purposes. Second, the deduction about the equations supporting the hypotheses is unwarranted. Presumably the authors think that the equations support their hypothesis that the demand for skilled labor is more elastic with respect to changes in GDP than for unskilled labor because the elasticity for African labor with respect to GDP in equation 9.5 is 0.6724 and for Asian labor it is 1.6712. But a comparison of equations 9.6 and 9.7 and of 9.8, 9.9 and 9.10 will show that the results are not at all clear cut. In fact, from equations 9.8 and 9.9 one could argue that the demand for skilled labor is less elastic with respect to GDP than for unskilled labor and from equations 9.8 and 9.10 that there is no significant difference in the elasticities for the two types of labor.

III

One can go on in this refrain to deal with other parts of the study. That would perhaps not serve much purpose. The examples in section II will illustrate the general point that the authors set out to see if regression analysis would help them in the three objectives I outlined above and that in fact it did not. The study should, therefore, reflect this outcome. And it cannot reflect this important outcome by just a few ritualistic disclaimers. The study has to be rewritten from a point of view altogether different from the one of the present draft.

AS:lcm

cc: Mr. P.D. Henderson

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Files DATE: June 29, 1970

FROM: Judy Maguire

SUBJECT: Editorial Committee Meeting

A meeting of the Editorial Committee was held on June 23, at 3:30 p.m. Present were Mr. Please (Chairman), Mr. Silcock (Editor), Mr. Henderson, Mr. Holsen, Mr. van der Tak, Miss Zafiriou, and Miss Maguire (Secretary).

The meeting was held in order to discuss the publication of the Thias/Carnoy paper "Cost-Benefit Analysis in Education: A Case Study on Kenya", as a World Bank Staff Occasional Paper. The committee meeting was preceded by a seminar conducted by the authors -- Hans Thias and Martin Carnoy -- which was attended by a number of other interested staff members.

The discussion of the paper made it quite clear that a number of staff members had very serious reservations as to the suitability of the paper for publication. The most serious objection, perhaps, centered on the validity of the statistical methods employed. It was felt by some that the analysis is used to support conclusions which are not justified by the data available. Special concern was voiced with respect to that section of the paper dealing with projections for the future. Another source of concern surrounded the policy implications inherent in the text. It is felt by some that a wider circulation of these might hamper a fair consideration by the Bank of cost-benefit methods applied to education.

In spite of the substantial objections, however, it was felt that several factors warranted publication of the paper. It was agreed that the subject is one which would be of interest to a wide range of readers. Also it was noted that this paper represents one of the best attempts to apply cost-benefit analysis comprehensively to education, and it is the most detailed and sophisticated study which has been attempted thus far. As such it is worthy of attention and would be an addition to current literature. Finally, the paper represents a considerable amount of research and study on the part of the authors, and much of the analysis and the data seemed to the committee deserving of wider circulation. Thus, in sum, it was felt that the paper's defects should not be allowed to obscure its inherent worth.

It was suggested that a number of changes would have to be made in the paper before it could be recommended to the Publications Committee. Specifically, it was felt that:

1) The paper was much too long and difficult to read. Certain chapters and sections were specifically mentioned as being

extraneous to the central presentation. Much of the technical material could likewise be cut and the findings summarized or included in annexes.

2) The assumptions and limitations inherent in the method would have to be more clearly spelled out. Also it seemed that the claims made on the basis of the calculations were excessive and should be modified.

3) The policy implications of the analysis would have to be considerably toned down.

The decision of the committee was that the paper should be recommended for publication, provided that the authors were willing to make substantial changes in content and style along the lines specified above and to the satisfaction of the Committee. It was agreed that the Editor and Mr. van der Tak would convey the decision to Mr. Thias and Mr. Carnoy.

cc: Members of the Editorial Committee
Mr. Pryor, Mr. Hoffman
Mr. Kamarck, Mr. Stevenson, Mr. Lowther (for info)
Mr. Friedman
Division Chiefs

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: June 23, 1970

FROM: Judy Maguire

SUBJECT: Cost-Benefit Analysis in Education: A Case Study on Kenya

Mr. Baldwin was unable to be present for the Editorial Committee meeting on the Thias paper and asked Professor Silcock to convey his sentiments to the committee.

He feels that the paper is an interesting and impressive piece of work, and that it definitely ought to be published. He feels, however, that we would have to include in the preface more than the usual pro forma disclaimer. We would have to be extremely careful in the presentation of the material. The fact should be stressed that the paper's utility is as an experiment in the use of this method for providing a base for educational programming. It should not be represented as a suggested Bank tool for operational programming.

Mr. Baldwin added that he felt that the statistics ought to be looked at by someone familiar with the approach. If the limitations of the analysis were simply due to the limitations inherent in regression analysis, this point could also be handled by including a warning in the preface.

In effect, Mr. Baldwin feels that the paper represents the first substantial major effort to apply cost-benefit analysis to the problems of education at the country level. It would, in his view, be an excellent point of reference for those who might be interested in the field, and could also serve to stimulate new interest in a vitally important subject. He feels we are unlikely to see as good a study in the near future, and as such it most definitely deserves to be published.

cc: Mr. Pryor
Mr. Hoffman

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: June 23, 1970

FROM: T. H. Silcock

SUBJECT: Meeting

An Editorial Committee meeting has been called for today, at 3:30 p.m., in Conference Room D-556, to discuss "Cost-Benefit Analysis in Education -- A Case Study on Kenya" by Hans Thias/Martin Carnoy.

My comments on the possible inclusion of this paper in the Occasional Papers series are enclosed.

cc: Mr. Pryor
Mr. Hoffman

Mr. Thias

DRAFT
June 22, 1970

Paper on Cost-Benefit Analysis in Education: A Case Study on Kenya by H.H. Thias and Martin Carnoy - Publication as an Occasional Paper

This is a study using sophisticated techniques and involving a considerable amount of research. It is presented primarily as a contribution to the general problem of analyzing education projects; Kenya is simply used as a case study. We must, therefore, assess it by strict standards as a contribution by the Bank to the literature of this subject and not judge it simply as a contribution to the study of Kenya's educational system. It is highly desirable that the relative merits of cost-benefit analysis and manpower projections in the assessment of education should be studied and their respective strengths and weaknesses discussed. If we are to publish the study, we must be sure that it really contributes to this discussion.

One of the important problems in this discussion is that there are large areas of educational policy in which mathematical--or even objective--techniques cannot be used. Education deeply affects both the transmission and the modification of a society's whole culture. It influences the selection of leaders and the relation between them and the led, the system of values and the structure of communication. It can generate both equality and mobility. We can hardly imagine any group of men who would not feel deeply about such matters, and judge them in relation to their own most profound convictions about the meaning of life and society.

It is not possible to consider education as a whole without being involved in these problems. A part of the whole argument--among those who are interested in using objective techniques--is whether we should spend

effort on trying to reach fairly broad consensus, treating a fairly large part of education in terms of mainly quantifiable goals such as cooperating with the more intelligent members of any government in power, subject to the condition of persuading them, where we can, to promote more development, or maximizing the rate of growth of GNP per head within the limits of what is politically possible from time to time -- or whether we should try to secure more rigor, within a very limited field, by trying to restrict the analysis, and seek areas in which relatively value-free criteria can be useful.

It is perhaps too much to hope that supporters of manpower projections should not, in some degree, base their arguments on respect for national sovereignty, and supporters of cost-benefit analysis on respect for the market. However, the fact must be faced that neither side is going to be able to achieve completely value-free consensus over the whole of educational policy.

I should perhaps express a personal preference for confining any mathematical analysis to areas where terms can be rather rigidly defined and a good deal of rigor achieved. Although long chains of verbal reasoning are often clumsy, they seem to me to give better defenses against the hidden assumption and the false analogy. This may be because to me, mathematical methods are difficult. However, I would not wish to push this preference to the point of condemning any attempt at rather wide coverage by mathematical techniques. I would, however, insist that any such attempt should define any assumptions and explain the significance of any analogies as clearly as possible.

I oppose publication of this study partly because it would put the Bank in an unhelpful position on the manpower-projection cost-benefit controversy, and partly because it fails to specify its assumptions about educational values and institutional implications, and thereby contributes to an excessive reliance on mathematical techniques as such.

This paper is admittedly highly sophisticated, and it covers a great deal of ground. How good is it? What is the quality of its presentation, its economics and its statistics?

Presentation

I found the paper extremely dull to read. This does not seem to be merely an editorial problem. It arises from the wide variety of issues discussed and the lack of any effective coordination of them. The claims of success in the introduction are also irritating and tend to make the reader even more critical than he would otherwise be about the assumptions that are so freely made and often rather inadequately justified. It is also surprising that writers who must have taken a considerable number of examinations should write as if one could wholly ignore the effects of examinations on the behavior of both teachers and pupils, and regard them as if they were not part of the educational process but merely combined tests of student ability and teaching efficiency. It is also almost incredible that a study of a whole education system should never once mention the educational pyramid and the effects of selection on quality at higher stages: what advantages and disadvantages accrue from having a larger field from which to select. Many other questions nag at the mind of any reader who is interested in education.

Economics

The study appears to miss one of the main points of Dr. Blaug's analysis in his paper for the Bank entitled "Cost-Benefit Approach to Educational Planning in Developing Countries". In Dr. Blaug's paper, manpower projections were treated as more suitable when factor proportions in the economy were relatively rigid and cost-benefit analysis when they were flexible. Dr. Blaug makes a lot of use of shadow prices and treats the pattern of education as something very flexible. He is also concerned to emphasize the rationality of the educational decisions of parents in less developed countries. The present study is clearly reluctant to use shadow prices, treats the projections of changes in salary levels largely for psychological effect, calling them "unrealistic", and stresses the irrationality of Kenyan parents in desiring various types of education for their children. The emphasis of the whole analysis is different and it fails to bring out the special advantages of a cost-benefit approach.

In the present study, differences in income levels derived from monopoly positions are treated as accepted indications of differences in welfare merely on the ground that trade union structure or civil service structure can be treated as a permanent part of the society. These distortions, however, result in a lower welfare optimum than could be achieved if they were removed, and it is at least desirable to make allowance for this by shadow prices. In distinguishing between private cost and social cost, the present study does not attempt to distinguish between income foregone in the former and marginal product foregone in the latter. One of the chief purposes of cost-benefit analysis is to enable us to take account of the effect of education in changing relative earnings. and a study which uses it, in effect, as a substitute for a manpower study is not really contributing very much to the debate.

Statistics

I have considerable reservations here about the use of the sample. Again, I think the problem arises from trying to cover too wide a field at once. The usefulness of a stratified sample is that one can make deliberate mathematical corrections for using larger proportional representation in the classes which are smaller in the original universe and thereby derive more significant results from a given number of observations. If one were dealing with a relatively small number of issues, it would have been possible with some 4,000 observations to derive more adequate information about the non-African pupils, about female education and about various other groups on which the information is a little too inadequate. I doubt, however, that it would have been practicable to deal with all the questions which are covered in this study except by using a proportional sample.

I believe that there are also instances here in which a random distribution is assumed where this is rather questionable, and I have great doubts about the possibilities of correcting for socio/economic factors by the kind of dummy variables that are used here. In view of the human importance of the recommendations the theoretical implications should at least be spelled out more than they are here.

Form of the Controversy

My main hesitation is that the whole form of the work gives the impression that a cost-benefit approach and a manpower-needs approach are all or nothing alternatives in the appraisal of education. This is very different from the original Blaug approach which showed the two types of analysis as complementary. Though I personally would be reluctant to apply cost-benefit analysis to a whole educational system -- because I think it would imply making welfare postulates that would not command consensus if they were stated

explicitly, and are liable to mislead if wrapped up in definitions for a quantitative argument -- I would, nevertheless be quite prepared to publish a mathematical study in this area if it gave a lot of attention to the implications of variations of prices and thereby gave fair treatment to the cost-benefit approach.

In this study, it is shown that expansion of secondary education at the present level will lead to unemployment and a lower level of earnings for people with such an education. In the original Blaug study, it is pointed out that from the earliest colonial times recommendations for vocational secondary education adapted to the so-called economic needs have always failed because the actual salary structure made the choice of academic secondary education a rational one. In Kenya, unemployment at the secondary level could be expected to lower the earnings of those with nothing more than a secondary education and thereby make possible more vocational education. This may not be the most rational way of proceeding but it plainly shows up the fact that expansion of academic secondary education can have indirect effects through the price level. The present study, however, uses cost-benefit analysis merely to show that the expansion of secondary education is irrational. It is an example of misplaced sophistication which leads to a general assumption that cost-benefit analysis is mainly a tool for economizing on the total amount of education instead of for distributing educational funds more effectively.

Audience

We must also consider the prospective audience for such a study. I am inclined to think it is too heavy and difficult to have much impact on educationists. It may appeal to economists in general, but with them it will tend to put the Bank in a position of treating income simply as a price in the manner of the more extreme members of the Chicago school, and this

will tend to emphasize an irrelevant issue in the consideration of the manpower and cost-benefit methods of analysis. The people who would be likely to be most interested would be specialists on cost-benefit analysis. The study undeniably uses elaborate techniques and attempts to deal with a number of the criticisms raised against this method. My hesitation about producing it as an occasional paper, primarily for this group, is that if cost-benefit analysis is to be extended to large sectors of an economy, it is particularly important that the macro-economic problems and the welfare problems should be tackled effectively and not simply assumed away. It is in this sphere that the theoretical analysis seems weak, and hence I have considerable doubt about publishing the paper even for this limited audience.

On balance, therefore, I must recommend rather strongly against publication of this paper. I have great respect for the authors' virtuosity and for the immense industry displayed in preparing the paper, but I do not think it is suitable to be published as one of our occasional papers.

To attend Editorial Committee Meeting on Tuesday, June 23 at 3:30 p.m.
in Room D-560 now D-556

D-400	Stanley Please, Chairman	x 4081	YES
D-534	T.H. Silcock, Editor	x 2482	YES
C-307	George B. Baldwin	x 2635	No - ~~traveling~~ outside Bank
D-450	Bela Balassa	x 2781	OUT OF COUNTRY FOR MONTH
G-1038	Barend A. de Vries	x 3625	No YES - will call back to confirm.
D-441	P.D. Henderson	x 3435	YES
A-~~538~~ 831	John A. Holsen	x ~~4908~~	YES
A-210	Benjamin B. King	x 4482	(OUT SICK)
C-305	Samuel Lipkowitz 4605	x ~~2678~~	No
D-545	Herman G. van der Tak	x 2141	YES
D-536	Rena Zafiriou	x 1481	YES
D-1123	Michael L. Hoffman	x 2173	in Europe
D-921	Donald J. Pryor	x 2530	YES

Mr. Thias x 4555 D-504 YES

to discuss: Cost-Benefit Analysis in Education: A Case Study on Kenya" (distributed to committee members Jan 15, 1970)

OFFICE MEMORANDUM

TO: Mr. Andrew M. Kamarck DATE: April 3, 1970

FROM: Stanley Please

SUBJECT: The Economic Benefit of Road Transport Projects -- van der Tak/Ray

1. I am attaching a memorandum from Messrs. van der Tak and Ray which gives their reactions to Professor Vickrey's comments on the above paper and also indicates the amendments which they intend to make to the paper to meet his comments. It seems to me this should give the necessary assurance to Publications Committee that the authors are acting upon the report of the external reader.

2. I have received no comments from other members of the Editorial Committee since my memorandum of 23 March 1970.

SPlease/lo

cc with attachment: Members of the Editorial Committee
Mr. Ray

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Stanley Please

DATE: April 1, 1970

FROM: H. G. van der Tak and A. Ray

SUBJECT: Professor Vickrey's comments on The Economics of Road Transport Projects

1. We have read Professor Vickrey's comments on our paper with considerable interest. We feel that we can satisfactorily meet the points he has made without making substantial revisions.

2. The main point of Professor Vickrey is that the simple framework in terms of which the principles of our analysis have been discussed will mislead some readers into thinking that the simple framework is descriptive of reality. We feel that we can easily warn the readers as to the realism of our assumptions in the appropriate cases.

3. The second point which Mr. Vickrey is making is that we should give much more emphasis and space to the analysis of imperfectly competitive markets. We agree that chapter 6 of our paper is too brief. We propose to expand it to include the major cases of market imperfections. We feel that such an expansion will fully meet the suggestions of Professor Vickrey as well as dispel the notion that we regard reality as very simple.

4. The third point of Professor Vickrey relates to the treatment of tax revenue. We shall try briefly to deal with this point, but we feel that a full discussion of how to treat changes in tax revenues in the context of project evaluation would take us too far afield. The subject will perhaps be worthy of a separate paper.

HGvanderTak/ARay:jln

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: March 23, 1970

FROM: Stanley Please

SUBJECT: The Economics of Road Transport Projects -- van der Tak/Ray

1. In the absence of any suggestions to the contrary from Committee members, I propose to recommend to Mr. Kamarck that he propose the above study to Publications Committee for publication as an Occasional Paper. The Committee has concluded on two occasions in the past that "the subject matter was important, the approach justified and that the substance of the paper warranted publication." It has been felt, however, firstly that significant changes in the exposition were required before a recommendation to publish could be made and secondly that the opinion of an outside reader was required.

2. On the first of these reservations I stated in my memorandum to Committee members of August 14, 1969 (copy attached) that I considered the authors had responded very fully to the points of both detail and of general presentation which had been raised. Since then a further and more polished version of the paper has been produced (copy attached).

3. Professor Vickrey was asked to comment upon the paper for the Committee and you have already received copies of his letter under Hugh Latimer's memorandum of January 15, 1970. His comments are very unfavorable and conclude with the statement: "I hope it is not too late to make substantial revisions in this paper before it is published." This is a judgment to be respected. However, it does not seem to me that the content of his criticisms justifies either rejecting the paper for publication or for delaying even further a recommendation to Publications Committee. His major reservations are with the realism of the assumptions and, as a consequence, with the dangers of misinterpretation and mis-use of the conclusions. I stated before the paper was sent to Professor Vickrey that the weight of opinion of the Committee and of the Bank's Transportation Projects Department was so strongly in favour of publication that I would only consider refusing a recommendation to publish if Professor Vickrey stated categorically that the paper should not be published and gave overwhelming reasons in support of such a statement. It seems to me that neither of these conditions has been met. What I propose, therefore, is that we should recommend the paper for publication and at the same time recommend to the authors that they study Professor Vickrey's criticisms and determine to what extent his points can be met at the editing stage.

SPlease/lo

cc: Messrs. Hoffman and Pryor
Kamarck and Stevenson

OFFICE MEMORANDUM

TO: Mr. Hugh Latimer

DATE: August 14, 1969

FROM: Stanley Please

SUBJECT: The Economic Benefits of Road Transport Projects -- van der Tak/Ray

1. I have read a revised version of the above paper. The revisions have been undertaken in response to the Editorial Committee's decision that the paper should not be recommended to the Publications Committee for permission to publish. This decision, as you know, was based on the Committee's view that the presentation of the analysis required to be much clearer and much more comprehensible to the reader, that the narrative should keep the reader more in touch with the economics of the problems examined, that the notation be clarified and that a check be made to correct all errors which had crept into the analysis.

2. I consider that the authors have responded very fully to the points raised by the Committee. The paper is certainly much more readable in general; it gets into the substance much more quickly than earlier versions which had had a long and not always entirely relevant introduction; it explains as it goes along the way in which the netting out of consumer and producer surpluses is undertaken, and it provides summaries of the argument as it goes along. The notation also appears to have been made consistent throughout. In general it seems to me that the paper is now in a form that will make it more acceptable to the Editorial Committee. At the same time I think there are still certain problems with it which should be attended to by the authors before it is circulated to the Committee. For instance, the paper's major message has been lost. This message was to the effect that the economic benefits of road projects are accurately measured by the area under the demand curve for transport between the regions and thus double-counting will arise if, for instance, the benefits to the ultimate consumers are added in. In addition I still find some of the diagrammatic representations of the analysis less easy to follow than should be possible. This is aggravated by at least one error I found in the delineation of a geometric area, which then makes one worry lest other parts of the analysis, where one was less alert or less detailed in one's reading, might also contain errors. I think the Committee's point regarding the need to check the work thoroughly for errors still stands.

3. I have spoken to Herman about these matters and am also sending him my annotated copy of the revised draft. I understand from him that he intends to produce a more polished version of the paper. I would suggest we then distribute this to the Editorial Committee. At the same time I would also wish to have the paper read by an outside reader so that we can ensure that our standards of publication can be defended from the charge of being those of an "inward looking" rather than an "outward looking" organization.

SPLEASE/lo

cc: Editorial Committee Members
Mr. Kamarck

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT

OFFICE MEMORANDUM

March 24, 1970

TO: Members of the Editorial Committee and Members of the Publications Committee

FROM: Andrew M. Kamarck

SUBJECT: Bela Balassa's Book on "Structure of Protection in Developing Countries"

Mr. Balassa's draft of the book has been reviewed by Professor Tibor Scitovsky of Yale University, as an outside reader, and by a review group of Bank, IFC and IDB staff members.

A copy of the Note on Points made at the Review Group Meeting and a copy of Professor Scitovsky's Report are attached for your information.

Mr. Balassa is now preparing the final version of the book, to be completed by June, 1970. We would appreciate receiving suggestions for improvement by April 30.

I plan to raise the matter of joint publication with the Inter-American Development Bank at the next meeting of the Publications Committee.

Attachments

cc: Those present at the Review Group Meeting;
Mr. Stevenson.

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT

OFFICE MEMORANDUM

TO: Mr. Andrew M. Kamarck March 12, 1970

FROM: Jack L. Lowther

SUBJECT: Note on points made at the meeting held to review the draft of the "Structure of Protection in Developing Countries", by Bela Balassa

General:

1. On February 27, 1970, the following people met to discuss Mr. Balassa's draft book "Structure of Protection in Developing Countries":

From the Bank:	Mr. Kamarck (Chairman), Messrs. Balassa, Lerdau, van der Tak, Kalmanoff, King, Lowther (Recorder).
From IFC:	Mr. Qureshi.
From IDB:	Messrs. de Beers, Gonzales.
From Yale University:	Professor Tibor Scitovsky, who had been asked to review the study for the Bank.

2. Discussions of the meeting concentrated on problems of improving the presentation so the published version would serve the needs of non-specialists, i.e., policy-makers and practitioners in international institutions and LDC's. Professor Scitovsky has submitted a written report containing the main recommendation discussed by the group. A copy is attached. The following detailed summary should be read in connection with Professor Scitovsky's report:

Chapters 1 and 2

3. Since the book was to be addressed to the non-specialist, chapters one and two should be simpler and more explanatory. The technical and methodological background should be placed in an appendix. In this connection, the following points were made:

(a) The order of presentation should be reversed -- the arithmetic relationships should be followed by an explanation of their meaning in economic terms.

(b) The reasons for using two sets of input-output coefficients in calculations should be better explained.

(c) There is a need for further explanation of the Corden and Balassa methods, showing why both were used.

(d) There is no need to argue the relative merits of the Bruno and Balassa methods.

Chapter 3

4. There was some question about the need for Chapter Three. After discussion, it was decided Chapter Three was necessary to provide basic numerical information on the countries as background for later discussions.

Chapter 4

5. Chapter Four was considered OK as is.

Chapter 5

6. Two points were raised about Chapter Five. One concerned "organization" and the other "content". Regarding organization, the group felt that the information in Chapter Five could be better presented in two sections -- evaluation and recommendations. Some felt that the evaluation section could be included in Chapter Four. Regarding content, discussion centered on the fact that the costs of protection appeared to be highlighted at the expense of the benefits. Mr. Balassa pointed out that, though some protection is needed in starting new industries, there is a question about how much and for how long. The research had shown that there was usually too much for too long. This may have given the chapter its slant against protection. It was agreed that this bias could be changed with some clear indications of the value of prudently applied protection.

Proposals for Change

7. At the conclusion of discussions on the first five chapters, Mr. Balassa indicated he intended to make the following changes:

(a) Chapters One and Two would be combined and re-written to more fully explain in simple terms the economic principles involved. The technical and methodological explanation would be placed in an appendix.

(b) Chapters Three and Four would be retained as they are.

(c) Chapter Five would be divided into two chapters -- one on evaluation and the other on policy recommendations. Any unfounded bias against protection would be eliminated.

Chapters 6 through 12

8. There was agreement that, apart from minor changes, Chapters Six through Twelve, the country chapters, should be retained as they are.

Yale University *New Haven, Connecticut 06520*

DEPARTMENT OF ECONOMICS
Box 1905A Yale Station
412 Strathcona Hall

TIBOR SCITOVSKY, *Professor of Economics*

March 4, 1970

Mr. Andrew M. Kamarck
Director, Economics Department
International Bank for Reconstruction
and Development
1818 H Street, N.W.
Washington, D. C. 20433

Dear Andy,

I am writing partly to say that I found last Friday's meeting very interesting and instructive and partly also to reiterate my main points lest they may have gotten lost in the midst of all the detailed discussions.

To begin with the obvious, I am impressed by the excellence and usefulness of the study and very much in favor of its being published soon and in full. The profession will be greatly indebted to the Bank for initiating and financing so ambitious a work on so important a subject; and I have no doubt about its high level of technical competence. This is why most of my criticism was concerned with presentation: I am anxious for it to reach and be accessible to the largest possible public.

My main objection was that technical discussions of statistical problems were given pride of place and crowded out or pushed into the background explanations of the economic meaning of the data, of differences between different sets of data, and of their different derivations and ways of presentation. To make the volume readable and its argument easy to follow for the non-specialist reader, this order of priorities needs to be reversed and the economic arguments and explanations greatly expanded. This can be easily done, most of it by a thorough and careful rewriting of Chapters 1 and 2. I am also strongly in favor of retaining in the text of these two chapters the basic formulae, accompanied or not accompanied by numerical examples, but with a much simpler notation and with the longer and more forbidding looking derivations and equations relegated to an appendix. I am sure that doing this will greatly enhance the usefulness of the whole study.

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: See Distribution

DATE: February 25, 1970

FROM: Stanley Please

SUBJECT: Structure of Protection in Developing Countries

Attached please find Appendix to Chapter 7 of Mr. Bela Balassa's study on The Structure of Protection in Developing Countries. It should be appended to the draft forwarded under the signature of Hugh Latimer dated February 17, 1970.

Distribution: Mr. Baldwin
Mr. de Vries
Mr. Henderson
Mr. Holsen
Mr. Pryor
Miss Zafiriou

SP:be

Ideas reading committee

(Study Grp:)

X Kamarck
Adler
Avramovic
Goreux
Kalmanoff
X B. King
Lerdau
(away) X Lipkowitz
X Van der Tak

X Hoffman Library Publications

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: February 17, 1970

FROM: Hugh Latimer HL

SUBJECT: Structure of Protection in Developing Countries

Attached is the preliminary version of the book prepared by Mr. Bela Balassa and associates.

Mr. Please feels that since a Study Group has been set up for the editorial guidance and evaluation of the book, it would be otiose for the editorial committee as such also to be involved. However, comments from members in their individual capacity would be much welcomed by Mr. Balassa. The Study Group is meeting on February 27.

cc: Mr. Pryor, Mr. Hoffman

Mr. Kamarck, Mr. Stevenson, Mr. Lowther (for info)

1 enclosure

Members of the Editorial Committee

Mailing for Balassa's paper:

✓ Please

✓ Baldwin
✓ de Vries
✓ Henderson
✓ Holsen

Latimer nas a copy

B.B. King has copy thru Balassa's Study Group
Lipkowitz " " " " " "
van der Tak " " " " " "

✓ Zafiriou
✓ Pryor

Mr. Kamarck " " " " "

7

Hoffman got a set through the Publications Committee

✓ (to send the memo to each member, crossing out reference to enclosure, where there is not an enclosure)

FORM NO. 75
(2-60)

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPI

INTERNATIONAL FINANCE CORPORATION

INTERNATIONAL DEVELOPMENT ASSOCIATION

ROUTING SLIP	Date Feb. 17, 1970

NAME	ROOM NO.
Miss Ann Jeffery	D-450

	To Handle		Note and File
	Appropriate Disposition		Note and Return
	Approval		Prepare Reply
	Comment		Per Our Conversation
	Full Report		Recommendation
	Information		Signature
	Initial		Send On

REMARKS

Mr. Latimer suggests I send this to you, to let you know to whom the copies of Structure of Protection are being sent.

From Betty E.

Form No. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: February 5, 1970

FROM: Hugh Latimer HC

SUBJECT: Editorial Committee Meeting

The editorial committee meeting arranged for February 10, is now called for Thursday, February 12, at 4:00 p.m., in Room D-560 to discuss "Benefits of Road Projects" van der Tak/Ray; "Cost-Benefit Analysis in Education: A Case Study on Kenya" Thias; and Professor Vickrey's comments on "Benefits of Road Projects".

cc: Mr. Pryor
Mr. Hoffman

No minutes prepared on this meeting

FORM NO. 57 | INTERNATIONAL DEVELOPMENT ASSOCIATION | INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT | INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Members of the Editorial Committee DATE: January 15, 1970

FROM: Hugh Latimer

SUBJECT: Editorial Committee meeting

A meeting of the committee is called for February 10 at 4 p.m. in Room D-560, to discuss the candidacy of the following papers for publication as Occasional Papers:

a) "Benefits of Road Projects" van der Tak/Ray (sent to Editorial Committee on Nov. 18, 1969)

b) "Cost-Benefit Analysis in Education: A Case Study on Kenya" Thias

Paper a), together with my own comments, was circulated to you on November 18, 1969. Please let us know if you lack copies.

I attach herewith Professor Vickrey's comments.

2 Enclosures: Cost-Benefit Analysis in Education (EC-173)
Professor Vickrey's comments (on van der Tak/Ray)

cc: Mr. Pryor
Mr. Hoffman

Mr. Kamarck, Mr. Stevenson, Mr. Lowther (for info)

Another copy filed under Van der Tak / Ray

504 Fayerweather Hall
Columbia U., N.Y. 10027
Jan 11, 1970

H G Van der Tak
A Ray
Hugh Latimer
IBRD, 1818H St, DC

Dear Confreres:

Permit me to offer some comments on the Mss.: "The Economic Benefits of Road Transport Projects" which you were kind enough to send me some time ago and at the same time to offer apologies for being so dilatory in responding.

It seems to me that this paper is chiefly devoted to what is to me a rather tedious verification, in a number of special cases, of the general proposition that the overall social benefit derived from the operation of a plant, project, or industry is measured by the area between the demand and supply curves, subject to the assumptions that prices are everywhere else equal to marginal social cost, no externalities are involved, changes in the marginal utility of money are negligible, and redistributive effects are not of concern. The Ray-Tak measures are then simply instances of this for the case of changes in the supply curve due to capital outlays or innovations. It would seem to me to be more useful to demonstrate, possibly in a simplified and not completely rigorous manner, the general theorem and then apply it to the special cases.

My more serious concern is with the fact that only the cases involving constant marginal costs, given on pages 1-13, are reasonably close to real situations, and that the other cases discussed are so unrealistic or at least so atypical as to carry a danger that they will be misapplied and lead to erroneous policy recommendations. It may be that the discussion of the constant cost special cases would clarify matters for some, as compared with the general theorem, but to go beyond this is, I think, dangerous. While market imperfections are discussed in section 6, this discussion should, I think, be brought much closer to practical situations.

In particular, the notion of an increasing marginal ~~cost~~ private cost, introduced on page 13 along with an assumption of equality between private and public marginal cost is to me almost necessarily inconsistent. Rising marginal cost on a highway is almost always the result of congestion with its externalities; the only way marginal private cost and marginal social cost could be brought into agreement would be through the imposition of road user charges corresponding to the difference. If this is the situation that is meant, it should be so stated, but then I think the impact of an improvement on the appropriate level of user charges and on the public revenues should be discussed explicitly. And since a dollar in the public treasury is not the same thing as a dollar in the hands of private individuals, some discussion of the marginal cost of public revenues could be appropriately introduced, to allow for the marginal excess burden of taxation.

The notion of rising cost, marginal or average, private or social, would be more realistically portrayed by a cost curve beginning with a segment that is horizontal or nearly so, with a concave upward segment at higher volumes. One could then distinguish capacity-adding projects (rightward shifting of the cost curve) from quality-improving projects (downward shifting). It is hard for me to visualize a circumstance where the kind of linear cost curve depicted is a reasonable approximation to a real situation.

To discuss competition between road and rail in terms of complete identity of the service, even in its complete, door-to-door form, plus rising marginal (?) unit cost of the rail service is to me totally irrlevant to any real situation. Marginal costs of rail transportation will almost always be found to be ~~xxxxxxxxxx~~ declining or at most rising extremely slowly, at least for any reasonably long run analysis, and in lieu of the congestion externalities found for the road mode, such externalities as exist are internalized through operation of the system as a coordinated unit, while rates almost invariably deviate from marginal cost by a considerable margin necessary to cover the intra-marginal residue of costs (i.e., "fixed" or "overhead" costs, roughly speaking). To present an analysis in which not only are the marginal costs of the rail mode rising, but the rail rates adjust themselves automatically to changes in marginal cost as traffic changes is, I think, at best not very helpful in the resolution of real problems, and at worst presents a serious danger of being misunderstood and the results misapplied.

To be sure, the modifications of the analysis required in cases where there is divergence between marginal social cost and price or marginal private cost, whether from monopoly, externality, or taxes, are presented briefly on pages 27-32. But this discussion is excessively brief: it takes no account of the cost of public revenues in the case where congestion charges are involved, makes no attempt to indicate any sort of second-best solution, and the general impression is that modifications required on this account, while possibly complex, are quantitatively minor and suitable to be treated as some kind of second-order approximation. To my mind the allocation of space should be reversed: 5 pages to deal with the general perfect-competition case where social and private marginal costs coincide, and 20 pages to deal with externalities on the roads, financing of railroad intra-marginal residues, the impact of congestion or other road user charges, the marginal cost of public revenues, and the solution of second-best problems.

I happen to have a fairly immediate practical interest in this matter: I am currently engaged in deflating some cost-benefit calculations of the US Corps of Engineers relating to the benefits from the Florida Barge Canal now under construction: benefits were calculated on the basis of cost savings resulting from shippers' current charges for shipment by rail, as compared with free transit through the canal. Neither the margin of rail rates above rail ICC-out-of-pocket cost (let alone strict marginal cost), nor the contribution out of these rates to governmental revenues, nor the costs incurred in the raising of the revenues to finance the canal, were allowed for in the estimates, which furthermore were discounted on the basis of a 2-5/8 % rate of interest! So you can see I am quite leary of anything that would give aid and comfort to such outrageously incorrect calculations!

I hope is is not too late to make substantial revisions in this paper before it is published. If I can help in any way in further specifying what I think ought to be done, please let me know.

Cordially yours,

William Vickrey