

**THE ACCOUNTS OF THE BRITISH EMPIRE:
CAPITAL FLOWS FROM 1799 TO 1914**

BY MARIO TIBERI

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This book presents a synthesis of the research on the quantitative estimates of the foreign investments of Great Britain during the nineteenth century. Given the limited amount of data available on stocks and flows of capital during the nineteenth century, the various scholars who have focused their attention on this topic had reconstructed either the balance of payments or the annual income earned overseas. This book exposes the principal results of the different scholars who have worked on the subject; it is a precious and important compilation of the work done in this field.

Mario Tiberi has done a remarkable job of collecting the different researches, and this book presents the different methods used by the authors. As emphasized in this book, there are three main methods in order to estimate the capital flows. The first one, the indirect method, measures the different elements of the balance of payments, and the capital flows are obtained as a residual. The second method makes use of the capitalization of annual income earned from British financial and real investment abroad. The third one does not properly estimate the capital flows, but concentrate on finding the information on assets owned by British citizen abroad.

The book is divided into four chapters. The first chapter presents the research of the authors who have used the indirect method; the second chapter presents the two other methods, the third one brings some of the critics on the different method, and chapter four is some sort of summary. ✕

In the first chapter, Tiberi presents the work of the scholars who have used the indirect method like Seyd, Shaw-Lefevre, Hobson, Schooling, Jenks, Caincross, Feinstein, Brezis and of course Imlah. They all use the indirect method, i.e. they measure the different elements of the balance of payments and as emphasized in the book, this method can be fraught with errors on each of the different elements.

The second chapter is devoted to the scholars who have used the two other methods. The authors who use the capitalization method, and in particular Giffen, Hirst, Crammond and Paish, start by identifying the flows of various types of income (profits, dividends, interests and rent) that can accrue from the capital invested overseas. With some

hypothesis on the year of purchases of the different components, it is possible to capitalize these flows and to estimate the stock of capital invested overseas.

The third method rests on the collection of information on all foreign assets, both financial and real, owned by British residents overseas. This method consists of identifying in all available sources the flows of financial assets purchased by residents in Great Britain as investments abroad. It is called the direct method and was mainly used by Nash, Harris, Ayres and Simon but also by Crammond and Paish,

The third chapter focuses on the scholars who have raised some critics on the research presented in the two previous chapters. These criticism, especially from Arnd and Platt focus on the general characteristics of the direct method. They claim that information from the financial market about the so-called portfolio investments cannot represent accurately the wealth accumulated abroad by the UK residents. They also propose downward revisions of the values attributed to the flows and stock of Britain's foreign investment suggested especially by Imlah and Paish.

The last chapter is a summary of the works presented above. Since the estimates are more problematic before 1870, Tiberi divides his critics in two periods – until 1870 and from 1870 until 1913-14. This chapter is the most interesting one.

On one hand, it is an objective summary of all the research. Tiberi summarized all the estimates in one table (table 4.1) which permits to compare the different results; this is a very useful table. On the other hand, he finally permits himself to be “subjective” and displays which research and which estimates he prefers. It seems that his preference goes to the capitalization method, and despite the fact that in chapter three he defends Imlah, in this chapter he raises some reservations on the indirect method. ✕

However it is clear that his preference is related to the objective of the research. For Tiberi, foreign investments are related to power and prestige, and therefore he does not give much importance to the flows, but mostly to the stocks of capital overseas. My own preference is, of course, for the indirect method, since by checking the balance of payments I could have proved that foreign capital inflows from overseas was almost as important as savings for paving the way to the Industrial Revolution. So preferences are related to goals. All in a whole, this book presents the research in total objectivity.

Therefore, my main critic on this book is that Tiberi is not critical enough, and he is somehow critical on the critics. Defending Imlah, he writes: “Unless an author can be proved unreliable, any attempt on his part to verify the quality of his own work through comparisons of hypotheses, methods and results of other scholars should be considered positively.” (p. 128). Still he agrees that “Every method of evaluation of the flows and stock of foreign investments needs both reliable data and plausible assumptions in order to obtain empirical results that are sufficiently representative of the real facts” (p.99).

Since this book is all about estimation of capital flows, I would have liked to see more interests /remarks on the whole problem of evaluation. *When one consider his summary in*

table 4.1 (p. 158-159), what is striking is that the estimates, using very different methods, give very similar results. If one compares for the year 1880 the results of the indirect method (1190 millions of pounds for Imlah) and the direct method (1250 for Nash, it is incredibly similar. It is even more for the year 1913 where the difference between Paish and Imlah is almost inexistent. Except for the first guesses at the beginning of the nineteenth century (where Imlah has a preference for 10 millions and Beeke and Seyd for 100, during the rest of the period, the differences are small).

Does this similarity mean that the probability that the “true” data is not far from these estimates is almost one? It would be so if all the different researches would have been independent, but the similarity of the estimates is also due to the fact that the works are interrelated. For instance, Imlah quotes most of the works written before him especially Jenks and Seyd. So in formulating his own data, he incorporates the previous estimates. This is the reason why the data are not so different. This comes back to a remark I have formulated on Maddison (2000), implying that creating data for the first time has a strong impact:

“The theoretical question that should be asked [about estimation] is: Does the first move, i.e., the first estimate, influence the entire ensuing quest for the “true” data? In other words, does the first guess influence the data estimated even after many corrections? If the first guess can affect the ensuing research, then despite the fact that Maddison is the best candidate “to throw the dice and create” the data, then—like every monopoly—this one is also undesirable.” (Brezis, 2001, p.3)

Probably, Tiberi should have raised more questions on the estimations, and emphasize which estimates are the outcome of new creation of data and which are some improvement of previous work. Yet, for scholars working on the balance and payments of the UK, this book is a must. It is a formidable synthesis of all what was done in this field.

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