The Transformation Problem and 'Bortkiewicz Corollary'

A Short Note on Maurice Dobb's 1948 Unpublished Paper

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In 1942 Sweezy published *The Theory of Capitalist Development* which was conceived with the "modest task of discovering what, if anything, can be learned from Marx" (Sweezy 1942, 8). This book has given rise to renewed debates related to Marxian political economy, and in the history of the transformation problem, Sweezy's book is essentially known for having resuscitated a debate that was almost forgotten in English-speaking countries. Sweezy (re-)established a link with what was until that time essentially a German (and Eastern European) story, by getting Bortkiewicz's 1907 contribution out of oblivion.[2] As a consequence, the 1940s and 1950s saw several contributions in English journals, from Winternitz (1948) to Seton (1957), through May (1948) and Meek (1956). The period from Sweezy (1942) to Seton (1957) has been called a 'cycle' in Desai's reconstruction of the debates (1988, 297).[3]

During this cycle, Sweezy brought new material with his book *Karl Marx and the Close of his System* (1949), containing reprints of Böhm-Bawerk's criticism of Marx and Hilferding's defense of Marx, together with a translation, made by Sweezy himself, of Bortkiewicz's 1907 solution to the transformation problem, which will be the starting point of this new round of controversies.

Maurice H. Dobb was to become fascinated with the transformation problem since the publication of Sweezy's book. During all the 1940s, Dobb smelt a rat in what Sweezy dubbed "A Corollary of the Bortkiewicz Method", and which was soon to be known as the "Bortkiewicz Corollary" in the literature. About his

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[2]Besides Bortkiewicz, Sweezy also introduced in a footnote the work of Moszkowska (1929). Other names, such as Charasoff or Rubin, were not mentioned.

[3]And about this period, he comments: "[w]hile the mathematical formulation was tighter and more elegant, nothing of substance was added" (Desai 1988, 313). For a comprehensive survey of the period between Bortkiewicz (1907) and Sweezy (1942), see one of the following histories of the transformation debate: Benetti & Cartelier (1975), Dostaler (1978), Howard & King (1989), Howard & King (1992), Faccarello (1983), Jorland (1995).

doubts, Dobb has left a few visible traces in the literature. In a first place, in his review of Sweezy's book, published in 1943, Dobb expressed two "minor points of criticism" to "*the* most important work" on Marxian economic theory "that has yet appeared on this subject from an English-speaking pen". The first of them is on the transformation problem: "Dr. Sweezy seems to adopt rather too uncritical an attitude towards the so-called Bortkievicz Corollary (p. 123 f.)." (Dobb 1943, 270) Later, when Dobb prefaced the UK edition of Sweezy's 1942 book, published in 1946, he referred to his own 1943 review of Sweezy, and added an hesitation:

> "Perhaps I should add that further reflection has caused me to doubt whether the comments made in [my] review about the book's treatment of 'the Bortkiewicz corollary' were entirely justified. It is a very special point; but it clearly deserves further discussion in the light of Dr. Sweezy's fresh and stimulating analysis of the problem."
>
> (Dobb, in Sweezy 1946, vi)

Eventually, Dobb wrote a "Note on the Transformation Problem" in late 1947 and in January 1948. Dobb hesitated to publish his note, as he was unsure about the "Bortkiewicz Corollary". In 1948, Dobb was engaged in several private conversations on this topic, and eventually renounced to publish it. In 1955, while collecting already published papers for his volume *On Economic Theory and Socialism*, Dobb added his Note at the end of the volume (Dobb 1955). He added a Postscript, indicating that "[t]he above Note was privately circulated, but not published". His doubts had apparently vanished, and the debates continued with Meek (1956) and Seton (1957).

In my short note, I want to illustrate how an unpublished paper may have had an impact on the development of economic thought. This will of course not be done in general, but on a particular case: the conditions of non-publication of Dobb's Note, in the context of the debates on the "Bortkiewicz Corollary" in 1948. I will try to explain these conditions by bringing new material to those already present in the above mentioned secondary literature (see footnote 3). [This note, at a very preliminary state, only brings the first results of this ongoing research.]

In the history of the transformation debate, I argue that Dobb's role in this debate was more important than only catching the attention to a problem in 1943. More generally, I would like to provide a case for historians of economic thought: papers that were not published or events that did not happen are sometimes as equally important in the understanding of what did happened, and what was published.

Bortkiewicz's Corollary

But let me first return to what exactly was meant by the "Bortkiewicz Corollary". In chapter VII of *The Theory of Capitalist Development* (1942) devoted to the transformation of values into prices, Sweezy first stated the problem and shows Marx's solution to it, before explicitly endorsing Bortkiewicz's (1907) method of 'correcting' Marx's way of transforming value into price of production: "Since this section is essentially nothing but an abbreviated version of Bortkiewicz's argument, specific references have been omitted." (Sweezy 1942, 115)

On pages 112–115, Sweezy presented Marx's solution. More exactly, Sweezy followed Bortkiewicz in adapting Marx's solution to a three-sector model (following Tugan-Baranovsky), with means of production (I), means of consumption for the workers (II) and luxury goods (III). Bortkiewicz's model also respects the conditions or simple reproduction. But unlike Tugan-Baranovsky, the model is in algebraic form. The following variables were used by Sweezy: c for constant capital[4], v for variable capital, s for surplus value, p for the average rate of profit, w for value and P for prices. Given Marx's conception of the average rate of profit, $p = \frac{S}{C+V}$, which ensures that total profit equals total surplus value, Marx would have arrived to the following tables:

System of value		System of price
$c_1 + v_1 + s_1 = w_1$	I	$c_1 + v_1 + p\cdot(c_1+v_2) = P_1$
$c_2 + v_2 + s_2 = w_2$	II	$c_2 + v_2 + p\cdot(c_2+v_2) = P_2$
$c_3 + v_3 + s_3 = w_3$	III	$c_3 + v_3 + p\cdot(c_3+v_3) = P_3$
$C + V + S = W$	Totals	$C + V + p(C+V) = P$

Marx's mistake, according to Bortkiewicz followed by Sweezy, is to have transformed surplus value *only*, and to have forgotten to transform constant and variable capital *as well*, and as a sequel of this to have derived from the system of value a wrong average rate of profit for the system of price.

By setting transformation rates (x, y, z) for every unit of value of each sector (I, II, III), Bortkiewicz derived, from the same system of value, a quite different system of price, of which the average rate of profit is to be determined:

Sweezy-Bortkiewicz System of Price				
I	$(1 + r)\cdot(c_1\cdot x + v_1\cdot y)$	$=$	$(c_1 + c_2 + c_3)\cdot x$	$= w_1\cdot x$
II	$(1 + r)\cdot(c_2\cdot x + v_2\cdot y)$	$=$	$(v_1 + v_2 + v_3)\cdot y$	$= w_2\cdot y$
III	$(1 + r)\cdot(c_3\cdot x + v_3\cdot y)$	$=$	$(s_1 + s_2 + s_3)\cdot z$	$= w_3\cdot z$

[4]Regarding constant capital, it should be noted that in the 1940s, all the discussions were dealing with circulating capital only, i.e. without fixed capital.

The source of all controversies started when Bortkiewicz had to solve this system of three equations, with four unknowns (x, y, z and the average rate of profit, r). Bortkiewicz had to choose a numéraire in order to solve the system. He evoked two choices (Bortkiewicz 1907, 202): either equating total value with total price ($Cx + Vy + Sz = C + V + S$), or setting $z = 1$ and consequently getting total surplus value equal to total profit. Bortkiewicz chose the last one, and, with the following notation

$$\begin{array}{lll} \frac{v_1}{c_1} = f_1 & \frac{v_1+c_1+s_1}{c_1} = g_1 & z = 1 \\ \frac{v_2}{c_2} = f_2 & \frac{v_2+c_2+s_2}{c_2} = g_2 & r = m - 1 \\ \frac{v_3}{c_3} = f_3 & \frac{v_3+c_3+s_3}{c_3} = g_3 & \end{array}$$

he was able to solve the system:

$$x = \frac{f_1 y m}{g_1 - m} \quad y = \frac{g_3}{g_2 + (f_3 - f_2)m} \quad m = \frac{f_2 g_1 + g_2 - \sqrt{(g_2 - f_2 g_1)^2 + 4 f_1 g_1 g_2}}{2(f_2 - f_1)}$$

And it is in the formulation of the rate of profit ($r = m - 1$) that the Bortkiewicz Corollary came to light: Bortkiewicz's solution implied that only variables from sectors I and II, i.e. only those that directly and indirectly produce the consumption goods for the workers, that are relevant for the determination of the average rate of profit. Or, as formulated by Sweezy:

> "A close inspection of the formula for the rate of profit, derived above, reveals a striking fact. [...] It will be observed that neither f_3 nor g_3 appears in the formula. In other words, the organic composition of capital in Department III (luxury goods) plays no direct role in determining the rate of profit.
>
> This is a result of considerable theoretical interest. It means essentially that the rate of profit depends only upon the conditions of production existing in those industries which contribute directly of indirectly to the make-up of real wages. Conditions existing in industries catering solely to capitalists' consumption are relevant only in so far as they influence conditions in the wage-goods industries".
>
> (Sweezy 1942, 123–124)

Winternitz's was the first, after Dobb's public invitation, to try to refute Bortkiewicz's Corollary (as exposed in Sweezy 1942, 123–125).

Dobb's Visible and Invisible Hesitations

In his 1943 review of Sweezy, Dobb expressed his intuition that something went wrong concerning the Bortkiewicz Corollary, with the following words:

> "I believe that it can be shown that the non-appearance of certain variables in the final equation from which the rate of profit is derived in the solution of the 'transformation problem' has no significance, since these missing variables are implied in the remainder given the assumption of equal rates of surplus value in the initial value position."
>
> (Dobb 1943, 274)

There is a short hand-written note by Dobb in the Sraffa Archives titled "Re. Sweezy on Bortkiewicz". In this note, Dobb asks (presumably to Sraffa) if he was missing something concerning the Bortkiewicz Corollary, how to explain the reasons for the variables of the third sector to be missing:

> "Since c_3 must $= (c_1 + v_1 + s_1) - (c_1 + c_2)$; and since similarly $v_3 = (c_2 + v_2 + s_2) - (v_1 + v_2)$; c_3 and v_3 are known as soon as $c_1 + c_2, v_1\ v_2$, $s_1\ s_2$ are known. Moreover, given v_3, s_3 can be deduced once the rate of surplus-value ($\frac{s_3}{v_3} = \frac{s_2}{v_2}$) is known."
>
> (Dobb, s.d., "Re. Sweezy on Bortkiewicz")

The note is undated. It can of course not have been written before Dobb had Sweezy's (1942) book in hand, and from the content of the note, it cannot have been written after 1948. But the wording of the note is so similar to the above quote from Dobb, that we are most inclined to believe that it was written *before* Dobb had his 1943 review published. Indeed, several expressions of this note are verbatim the same ("in the initial value position", "given an assumption of an equal rate of surplus value"). What Dobb did only implicitly mentioned in this note, and *not* in his review, is that the conditions of *simple reproduction* (besides "the assumption of equal rates of surplus value") are crucial in the "non-appearance of $f_3 + g_3$".[5]

We found no written evidence that Sraffa ever replied to Dobb, but they well might have spoken on this matter.[6] The only evidence that can be guessed from this note is that this question quite absorbed Dobb. In 1946 Dobb was not sure

[5] See Dobb (s.d., "Re. Sweezy on Bortkiewicz"). The whole note is reproduced in the Appendix.

[6] Sraffa and Dobb already knew each other in 1942–43, and at that period, Sraffa was no longer in an internment camp. But it is only starting from 1948 that they met on a very regular basis, for the edition of Ricardo. On the relation between Sraffa and Dobb, see Pollitt (1988).

whether to change his mind on the question (Dobb, in Sweezy 1946, vi; see quote above p. 2).

Winternitz's Long Draft

In the period 1947–1949, and as far as the transformation problem is concerned, Dobb is not at all isolated. He is *non-incidentally* in contact with most participants (Sweezy, Moszkowska, Winternitz, May), and meets regularly Cambridge residents or visitors (Sraffa, Robinson, Meek—at the time a PhD student working under the supervision of Sraffa and Dobb).[7] Unfortunately, according to Dobb's literary executor, it seems that many letters are missing from the Dobb papers. For instance, May's first letter to Dobb in the archives dates from 1950, while is is more than evident that they wrote each other earlier. So the following relies on incomplete information, and often unilateral letters to Dobb.

The assistance provided by Dobb to J. Winternitz in the writing of his paper "Values and Prices: A Solution of the So-called Transformation Problem" (1948) is striking. From October 1947 to June 1948, Winternitz sent to Dobb at least eleven letters, evidently followed by answers from Dobb.[8] It is not known whether Dobb and Winternitz ever met, although there are several attempts or propositions to meet in their correspondence, either in Cambridge, or in London. Winternitz's address in London (16 Aberd[are] Gard[ens] NW6) is found in Dobb's address book, but Dobb's diaries do not contain any meeting with Winternitz in the period under consideration.

Winternitz sent at least three versions of his draft to Dobb, and apparently, Dobb sent them back to Winternitz, this is why we can only guess their content from the letters sent by Winternitz. First, it must be said that Winternitz wrote a long paper (more than 20 pages), but the version published in *The Economic Journal* is only $4\frac{1}{2}$ pages long. Dobb was clearly instrumental in this move.[9] Commenting upon a first series of remarks by Dobb, Winternitz agreed that his own

[7] See Maurice Dobb Archives, Trinity College Library, Cambridge (DOBB CA, DOBB B7, DOBB H2).

[8] We have not [yet?] been able to locate J. Winternitz's papers.

[9] "Maurice Dobb, in a review in Science and Society (Summer, 1943), was the first to raise questions. It was Dobb also who stimulated J. Winternitz to a mathematical reconsideration of the transformation problem. A very brief summary of Winternitz's results appeared in the Economic Journal for June 1948 under the title: 'Values and Prices: a Solution of the so-called Transformation Problem.' I hope the entire paper on which this summary is based will eventually be published. A valuable commentary on Winternitz, which throws new light on several aspects of the controversy, appeared in the December 1948 issue of the Economic Journal: 'Value and Price of Production: a Note on Winternitz' Solution," by Kenneth May.' (Sweezy 1949, xxix, n2) See also Howard & King (1992, 243n28): "Sweezy refers ([1949, p. xxxii]n.2) to a longer version of Winternitz's paper; to the best of our knowledge this was never published."

solution to the three equations–four unknowns system (caracterised by the equality "sum of prices" = "sum of values") was equivalent with that of Bortkiewicz (with $z = 1$). And when he compared the two profit rates, he had to confess that Dobb was right: the Bortkiewicz Corollary was correct, and his criticisms were not: "the rate of profit is determined by the relation in I and II whatever happens in III".[10] Later, Winternitz would have entirely rewritten the introduction according to Dobb's further remarks. He even removed all numerical examples, even if he prefered them to algebra.[11] Dobb and Winternitz tried to compare these numerical solutions with Moszkowska (Dobb make them enter in contact in 1947, and sent a book by Moszkowska to Winternitz later in December 1947).

In January 1948 (when Dobb wrote his own "Note", Winternitz arrived little by little at his published solution. Winternitz thought that they were confused by Sweezy[12]: his numerical examples depicted simple reproduction while the algebra did not do so, and therefore, the conditions of simple reproduction have nothing to do with the transformation problem. Indeed, Winternitz distinguished two cases: the formal problem of transformation, in which the conditions of reproduction have nothing to do, and the actual transformation, for which things are less clear. He concluded that with his transformation (where total value equals total price), together with Sweezy-Bortkiewicz transformation (where $z = 1$), the conditions of simple reproduction are not affected, while this is the case with expanded reproduction as he wrote to Dobb:

> "While there is only one relationship which corresponds to simple reproduction, there are numerous possible relations compatible with expanded reproduction."
>
> (Letter from Winternitz to Dobb, 12 January 1948)

Eventually, it is learned that Roy Harrod, for the *Economic Journal*, asked for a short version, and that this resulted in a sharp selection, that was advised by Dobb: all numerical examples were rejected, together with the long analysis of the conditions of expanded reproduction, that were devoted to show that the

[10] Letter from Winternitz to Dobb, 15 November 1947 (Maurice Dobb Archives, DOBB CA 236, Trinity College Library, Cambridge).

[11] Letter from Winternitz to Dobb, 15 December 1947 (Maurice Dobb Archives, DOBB CA 236, Trinity College Library, Cambridge).

[12] We also learn that in January 1948, neither Dobb or Winternitz have read Bortkiewicz's 1907 paper, and Winternitz always wrote "B[ortwiewicz] or only S[weezy]". In a letter dated 10 February 1948, Winternitz wrote to Dobb: "Meanwhile, I read Bortkiewicz's original article. This does not lead any further, Sweezy actually reproduced all the essential items."

Bortkiewicz Corollary was not working in this case.[13]

Epilogue

Winternitz has been the first to *explicitly* state that "Bortkiewicz and Sweezy base[d] their analysis of the transformation problem on Marx's *scheme of simple reproduction*" (Winternitz 1948, 277, emphasis added). He found Bortkiewicz's assumptions "unjustified and unnecessary", going to qualify simple reproduction as "not relevant to this problem" (Winternitz 1948, 278). At the same times, he added a complication, into which Dobb did not want to enter: for Winternitz, expanded reproduction was the "normal case", and therefore the case to be investigated.

May paid tribute to Winternitz for having separated the conditions of reproduction from the procedure of transformation, clearing up "an artificial confusion initiated by Bortkiewicz" (May 1948, 596). May further noted that Winternitz's method is "independent not only of the conditions of simple reproduction (as he correctly points out) but also of the context of the division of the economy into three branches" (1948, 598), thus paving the way for Seton's (1957) *n*-sector model. The Bortkiewicz Corollary becomes more complex in a disaggregated model: it depends on the actual relations of dependence or independence between sectors (and the concept of fundamental commodities was soon to come).

And indeed, in his "Note on the Transformation Problem" (unpublished) that was circulating in 1948, Dobb is, after Winternitz and May, still convinced that the conditions of simple reproduction have something to do, as quantities are not independent between sectors.

Sweezy is, on his side, seems a little overwhelmed. He wrote to Dobb:

> "I am not at all sure that I will have [something to say to Winternitz and May] because they are both mathematically out of my range, and I certainly do not want to enter into a technical argument with either of them"
>
> (Letter from Sweezy to Dobb, 28 January 1949, DOBB CA 212)

In the same letter, Sweezy wrote that he was not certain that reproduction and transformation have something to do together, but believed that a good transformation procedure should not disrupt the equilibrium of reproduction.

Seton (1957) will follow on their tracks: the different numéraires used in the Sweezy-Bortkiewicz model, in the Winternitz model, or even in the Meek model

[13] Hodgson (1974, 366–368), without knowing Winternitz's long draft, rejected this kind of refutations of the Bortkiewicz Corollary. In general, criticisms of the Bortkiewicz Corollary are dismissed in the literature: see e.g. Faccarello (1983, 155), who qualifies this as a "curious debate".

will be handled as "postulates of invariance", and the model will be extended to n sectors. The link between reproduction and transformation vanished, and the doubts about the Bortkiewicz Corollary—included Dobb's—disappeared altogether. In his *Theories of Value and Distribution Since Adam Smith* (1973), Dobb evoked this episode as "a curiosity [...] which has occasioned some discussion" (1973, 160).

On the sole records of published materials, Dobb appeared as a background actor in the Bortkiewicz Corollary discussions. His 1948 unpublished paper was in fact only the tip of the iceberg of his actual role in the story: a scholar well inserted in national and international relevant networks, who asked a good questions, know all relevant publications before they get published, and who authored Winternitz's 1948 paper at least as much as Winternitz himself.

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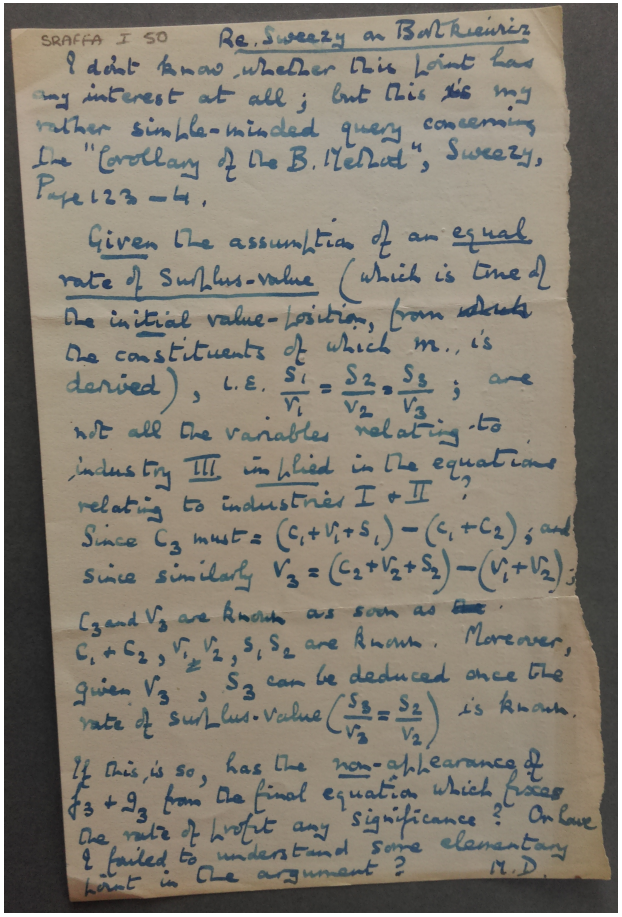
APPENDIX. Dobb's Hand-Written Question [Most Presumably to Sraffa]


SRAFFA I 50 Re. Sweezy on Bortkiewicz

I don't know whether this point has any interest at all; but this is my rather simple-minded query concerning the "Corollary of the B. Method", Sweezy, Page 123—4.

Given the assumption of an equal rate of surplus-value (which is true of the initial value-position, from which the constituents of which $m.$ is derived), $\iota.\epsilon.$ $\frac{s_1}{v_1} = \frac{s_2}{v_2} = \frac{s_3}{v_3}$; are not all the variables relating to industry III implied in the equations relating to industries I + II ?

Since c_3 must $= (c_1 + v_1 + s_1) - (c_1 + c_2)$; and since similarly $v_3 = (c_2 + v_2 + s_2) - (v_1 + v_2)$; c_3 and v_3 are known as soon as $c_1 + c_2, v_1\ v_2$, $s_1\ s_2$ are known. Moreover, given v_3, s_3 can be deduced once the rate of surplus-value $(\frac{s_3}{v_3} = \frac{s_2}{v_2})$ is known.

If this, is so, has the non-appearance of $f_3 + g_3$ from the final equation which fixes the rate of profit any significance? Or have I failed to understand some elementary point in the argument?

M.D.

Maurice Dobb's manuscript note [with my transcription], undated, Sraffa Archives, SRAFFA I 50, Wren Library, Trinity College, Cambridge. This note has been found [quite appropriately] in Sraffa's library, inside Paul M. Sweezy's *The Theory of Capitalist Development* (NY: OUP, 1942, inventory number SRAFFA 1764).