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ACCOUNTING AND THE THEORY OF THE FIRM

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This paper describes the background and objectives of a series of papers written fifty years ago at the London School of Economics (LSE). One objective was to encourage the use of accounting numbers in economic research. A second objective was to improve the theory and practice of accounting. Understanding cost accounting and opportunity costs within a firm was tied to understanding the organization of firms. The theory of the accounting system is part of the theory of the firm. Like a similar request made fifty years ago, the paper concludes with a call for interdisciplinary studies between economics and accounting.

This paper will be largely autobiographical and will mainly be centered around what happened at the London School of Economics fifty or more years ago. In 1937 my article 'The Nature of the Firm' was published, based on ideas I had developed some years earlier. It has since been reprinted on several occasions and has been much discussed, particularly in recent years. One year later, in 1938, I wrote a series of articles for *The Accountant* entitled 'Business Organization and the Accountant', articles which have also been reprinted and referred to in the literature, although not nearly to the same extent as 'The Nature of the Firm'. At any rate, in the 1930's I was working on both accounting and the theory of the firm. Today I want to discuss the relation between these subjects.

I explained how I came to be interested in and to write on the theory of the firm in some lectures on 'The Nature of the Firm' which I gave at Yale last year and which some of you may know. I will now explain how I came to be interested in and to write on accounting. In my Yale lectures I disclosed, which may be somewhat surprising given that my career has been that of an economist, that I did not take any course in economics while I was a student at the London School of Economics, a circumstance which I believe gave me a freedom in thinking about economic problems which I might not otherwise have had. The position with regard to accounting is quite different. I took a degree in commerce, with the thought that I would go into business. As I chose the 'Industry' group in my final year, an option supposedly designed for those who were going into works management, I took courses in works and factory accounting and cost accounting as well as financial accounting. I learned, among other things, that there were many methods of calculating depreciation, valuing inventories, allocating on-costs, and so on, all of which gave different results but all of which were perfectly acceptable accounting practices. In addition, there were, of course, practices which were not acceptable and which should not be countenanced by any accounting firm which aimed at respectability.

As we were to discover, the views that I and my young colleagues at the London School of Economics (LSE) came to hold were considered by a number of accountants (including the head of the accounting department at LSE) not to be respectable and, indeed, it would not be going too far to say that they were thought to be sacrilegious. The books that had been entrusted to the accountants' keeping were apparently sacred books. Those most involved with this critique of accounting practices were Ronald Edwards and Ronald Fowler. Edwards, after the war, became Chairman of the Electricity Council, which was responsible for the operations of the British electricity supply industry, and later became head of Beechams, the pharmaceutical company. But in the middle 1930's he was a humble assistant lecturer like myself. He had worked in an accounting firm and taken the external B. Com., studying with the Commerce Degree Bureau (which organized correspondence courses under University auspices). His work as a student with the Commerce Degree Bureau brought him to the attention of Arnold Plant and, as a result, he was appointed in 1935 to a position in the newly formed Department of Business Administration at LSE.

Ronald Fowler, who had been a fellow student of mine, wrote a book in 1934 on *The Depreciation of Capital* and worked with Plant on a project which surveyed annually the costs of British department stores (an investigation modelled on a similar one carried out by the Harvard Business School). Fowler joined the Department of Business Administration after Plant became its head. As a result of his work on depreciation, Fowler developed a keen interest in accounting theory.

Edwards played the leading role in our study of accounting and the work of accountants, and following his initiative the Accounting Research Association was formed in 1936, with the active cooperation of the Librarian of the Institute of Chartered Accountants, Cosmo Gordon. The work with which I was associated had two aims. The first was to show economists that the accounts provided valuable source material for economic research, and the other was to persuade accountants to change their practices so as to make the accounts more valuable for this purpose by, for example, not lumping together disparate items and by adopting more uniform methods which would facilitate the aggregation of the accounts of different companies. We were not content merely to stop there but went on to show economists what could be done and accountants what should be done. We did this by making an economic study of the British iron and steel industry, using the accounts of public companies.

The paper 'Published Balance Sheets as an Aid to Economic Investigation – Some Difficulties' came out in 1938 and recounted the difficulties we had encountered in carrying out our investigation. We started by asserting that 'balance sheets, in so far as they are reliable and the conventions on which they are based are thoroughly understood, will provide the economist with much material for applied study' and we pointed out that what we said was based on an examination of approximately 2,000 balance sheets. In summing up, we said that in drawing attention to the difficulties in interpreting the various items in the balance sheet, we were not claiming that 'the economist could do better than skilled accountants. The aim of this paper is merely to emphasize the problems as they are seen to users of balance sheets for economic research.' We concluded that 'some of the major difficulties are due to the lack of uniformity in the practice of accountants, and also, which is to some extent the reason for this, the obscurity of much of their reasoning and the distinctions which they employ. It follows that an improvement in accounting theory would materially assist the work of economic research.'

Later in 1938 we presented our findings on the iron and steel industry to the annual meeting of the Association of University Teachers of Economics. It aroused little interest. This was no doubt in part due to the Chairman's introduction. Usually the Chairman's introductory remarks are so flattering that one's heart sinks as one hears them, realizing that now whatever one says must inevitably be a disappointment. In our case, the Chairman's introductory remarks had a different character. They were uniformly hostile. I cannot now remember what they were, but they expressed the view that the figures provided by accountants were so unreliable that any attempt to use them for economic research was vain.

Nevertheless, we continued with our work and in late 1939 or early 1940 *The Iron and Steel Industry 1926–1935: An Investigation Based on the Accounts of Public Companies* (Special Memorandum No. 49 of the London and Cambridge Economic Service) was published. Of course our methods of analysis were extremely crude compared with those employed today. And the fact that there were no computers severely limited the calculations that could be made. All our computations were made by pencil and paper, with the aid of a Monroe handcalculator. Nonetheless, whatever the value of our results, I think we showed the potential usefulness of these accounting figures for economic research. I will give as an example of what could be done the discussion of the factors determining the demand for bank advances (one of the parts of the study for which I was responsible).

It was then widely believed that bank credit played a crucial role in generating and intensifying the business cycle and that this was particularly the case in the capital goods industries, industries which, in the view of some economists, played a dominant part in the whole process. The iron and steel industry can be regarded as the archetype of the capital goods industries. Our analysis of these balance sheets showed that, at least so far as our sample was concerned, the beliefs of some prominent economic theorists about the behaviour of bank credit were incorrect. Our figures confirmed the common view that most borrowing from British banks was for relatively short periods, but in other respects the results were quite unexpected. Analysis of the balance sheets showed that increasing bank credit was associated with losses and declining profits. The result was that, although with the onset of the depression in 1929 bank credit fell immediately, as the depression deepened bank advances to the iron and steel industry rose; and when, in 1935 and 1936, profits began to increase significantly, bank credit fell. Bank credit in the iron and steel industry, far from exaggerating the effects of a business fluctuation, acted as a cushion and as a moderating force.

In drawing attention to this part of our work, I do not wish to imply that this was our most significant finding. I have mentioned it because it was a part of the investigation for which I was largely responsible and with which therefore I have greater familiarity. Other parts of the investigation dealt with the movements of profits, undistributed profits, liquid balances, gross working capital, stocks, work in progress, and so on, and some of these sections are no doubt more important than that on bank credit.

We felt able to conclude that 'published accounts are a source of economic statistics that ought not to be neglected'. This view resulted in large part from the fact that, by using the accounts to derive information about firm behaviour, it was possible to relate the figures about one kind of firm behaviour to all other aspects of the firm's position and behaviour. This still seems to me a decisive advantage of using the accounting records.

The war intervened in 1939, and when it ended we did not return to this work. Whether similar work has since been carried out by others I do not know, but if it has, so far it seems to me that it has little effect on economic theory. Certainly the vastly improved quality of published accounts (many of the difficulties mentioned in our Accounting Research Association paper would not now exist), as well as the improved methods of analysis, should make it possible today to use the published accounts more fruitfully than we were able to do.

I would not wish to leave you with the impression that the Accounting Research Association was mainly concerned with the use of accounting records for economic research. It was also, and probably more, concerned with the improvement of the theory and practice of accounting. As part of this effort, it conducted, by means of questionnaires, studies of the methods of valuing raw materials, work in progress, and finished stock and of the methods of calculating depreciation actually used in business – which enabled an attempt at discovering how the method employed varied with the type of business.

In the examination of the financial accounts, I was very much the junior partner. Edwards knew about actual accounting practices, of which I was completely ignorant. And both Edwards and Fowler knew more about accounting theory than I did. My name appears first simply because in the alphabet C comes before E and F.

I now turn from our work on financial accounting to that on cost accounting, and in our discussions of this subject I played a more equal role. My published contribution consisted of twelve articles which appeared weekly in *The Accountant* in 1938, entitled 'Business Organization and the Accountant'. These articles were written each Wednesday night and were published on Saturday. They could be written in this way because I thought of them not as presenting ideas of my own but as an exposition of views which were the common property of the economists at LSE or, at any rate, of those with whom I was associated.

That the ideas in these articles were in no sense original is apparent if one also studies Edwards' 'The Rationale of Cost Accounting', which appeared in 1937 in *Some Modern Business Problems* and which has been reprinted elsewhere, or the paper of C.L. Paine (another student of Plant's), 'Some Suggestions for Measuring the Relative Profitability of Competing Uses of Floor Space and Fixed Equipment', or the writings of David Solomons, such as his 'Cost Accounting and the Use of Space and Equipment' (published in 1948 and reprinted in *Studies in Costing*). Solomons, of course, studied at LSE under Arnold Plant, and in a letter which Fowler wrote to me in 1932 while I was in the United States and which I have deposited in the Regenstein Library of the University of Chicago, he refers to a very good paper on costs which had just been given in Plant's seminar by a student, David Solomons.

Originally, I had not intended to write a series of articles in *The Accountant* dealing with costs and cost accounting. Edwards had suggested that I write a series dealing with problems of business administration. But when, in the summer of 1938, I sent him a draft of an article on vertical integration (whether to make or buy), Edwards complained bitterly that the accountants for whose benefit the articles were supposedly written would not understand what I was talking about, as the concepts and terminology I used would be completely foreign to them. I therefore decided to write an introductory section in which I explained the character of my approach. However, the introductory section came to occupy the whole of the twelve articles, and business problems were not discussed except as illustrations of the value of my approach.

When I wrote these articles I had no idea that they would ever be reprinted, as they were by Solomons in *Studies in Costing* and in *Studies in Cost Analysis* and by Thirlby and Buchanan in *LSE Essays on Cost*, nor that they would ever be referred to in the literature. Had I known this, I would undoubtedly have tried to be more careful in the writing – with the probable result that these articles would never have been completed.

What these extremely simple articles did was to argue that in business administration cost should be interpreted as opportunity cost and that the approach should be marginal (concerned, that is, with variations in costs and receipts). This led naturally to a critique of the practices and doctrines of cost accountants. The opportunity cost concept developed at LSE was of course derived from Knight and Wicksteed as expounded by Lionel Robbins and was also no doubt influenced by Hayek who would have added an Austrian flavour.

As I have indicated, accounting as taught at LSE at that time had the aspect of a religion. It is not, therefore, altogether surprising that these articles resulted in my being given a public rebuke by S.W. Rowland, the head of the accounting department at LSE, in an address given to the Accounting Research Association in 1939, entitled 'Experience, Research and Speculation in Accounting'. He opened his reference to my articles with the following sentences: 'If I particularize I do so with pure objectivity. I notice cases where the impact of economic studies on accounting lays greater emphasis on the speculative element than on experience.' He then proceeded to ridicule the opportunity cost concept after completely misunderstanding what I had said. On other occasions, in *The Accountant* he attacked in a vitriolic way the views of Edwards on the nature and measurement of income. In an article he compared Edwards' ideas to Dr. Johnson's leg of mutton, 'ill bred, ill fed, ill killed, and ill drest', and in a letter in the correspondence columns he stated that he regarded these ideas 'as dangerous nonsense made the more dangerous by the fog of words in which assumptions are disguised as truths'.

Later, in 1947, W.T. Baxter, who had written two papers on 'The Form of Final Accounts' for the Accounting Research Association, was appointed head of the accounting department at LSE and relations with the economists became very cordial. I should add that my articles also brought some criticism from accountants in the correspondence columns of *The Accountant*. However, we also found that we had some allies among cost accountants, and I remember particularly those at the Gramophone Company (which was or became part of EMI). Of course, we would tend to hear from those cost accountants who supported our efforts.

Buchanan, in his book *Cost and Choice*, has argued that the concept of cost developed at LSE in the 1930's was special to that institution, and he claims that my articles embody this particular point of view. He instances with approval my linking of costs with the decision to be taken. I 'tie[d] cost to choice', as Buchanan puts it. He also notes my rejection of any classification of costs into categories such as fixed and variable 'independent of the identification of the decision under consideration', and he makes other points of a similar character. Whether Buchanan is right in his thesis that these views on cost were not those normally held by economists elsewhere I do not know. But there is no question that I thought of these articles as presenting views which I shared with my economist colleagues at LSE. As I said in an introductory note when these articles were reprinted in *LSE Essays on Cost:* 'If Professor Buchanan's thesis about the special character of the LSE approach to costs is correct, it is the fact that these articles do not represent a personal point of view which gives them their historical significance.'

The opportunity cost concept of the London tradition of the 1930's which Buchanan had praised was to come under attack from a somewhat unexpected quarter. In 1974, a collection of papers entitled *Debits, Credits, Finance and Profits* was presented to W.T. Baxter on his retirement from the chair of accounting at LSE. J.R. Gould, a lecturer in economics at LSE, contributed a paper in which he discussed Buchanan's thesis and the London tradition. Although his arguments were essentially the same as those he had put forward in 1962 in *Studies in Accounting Theory*, in 1974, no doubt stimulated by Buchanan's book, he made detailed references to the writings of Edwards, Baxter, Solomons, and myself. He says this of my use of the opportunity cost concept: 'Coase's specification of opportunity cost, if interpreted as a definition for use in computation, is at best superfluous, and at worst downright misleading.' It is not a statement that one would be inclined to choose for one's epitaph.

Gould gives, as an example of how my definition could mislead, the case of a 'somewhat obtuse developer' who interprets my definition of opportunity cost in a business decision as the highest alternative receipts to mean the highest *gross* receipts and who therefore does not deduct the costs that would have to be incurred to secure those receipts. This does not seem to me a substantial objection to the opportunity cost concept nor to its use in computation. Gould continues: 'A less obtuse developer would interpret opportunity cost to mean highest alternative *net* receipts.... The correct decision would be made – but the concept of opportunity cost has played no useful part in the computations necessary to arrive at the decision. The net receipts of each plan must be worked out *before* the opportunity cost of any one plan can be computed; and once the net receipts are known the correct decision can be made without further computation.'

This is an interesting point, but consideration of it leads, I believe, to a conclusion different from that drawn by Gould. For one thing, if net receipts, that is, receipts minus costs, are to be calculated correctly, costs must be calculated correctly, that is, they should represent opportunity costs. If, for example, one of the plans requires the use of a material already in stock in that business and that material cannot be sold and is of no use elsewhere in the business, it would obviously be wrong to treat the cost of using that material as being equal to the price that was paid for it in the past. It is, in fact, zero.

All this I explained in 'Business Organization and the Accountant'. But there is a more serious objection to Gould's way of looking at the problem, and it is one which takes us to the heart of the theory of the firm.

When I was starting to study economics some 60 years ago, it was quite usual to illustrate the economic problem by considering how Robinson Crusoe would make his decisions. He had all the resources of his island at his disposal, but they were limited and he would not be able to achieve all he would like. He had to choose. He would choose to utilize these resources in those combinations which would yield that collection of products out of all possible collections that, in total, he preferred. Some modern economists would say that he maximized his utility. In reaching this position, Robinson Crusoe *need* make no explicit calculation of cost, although he might find it useful to place on his bulletin board 'one beaver equals two deer' or perhaps in his case 'fifty coconuts equals one turtle'.

Today, one would not be likely to start with this isolated man but with a society containing many men. The economic problem faced is then not simply to decide how one individual would use resources but also which individual should use them. It is normally assumed that this society uses money and that the actions of its members in the economic sphere are coordinated by a pricing system. To secure the use of a resource it is necessary to pay at least as much as someone else would. Consequently, prices are bid up until the amount demanded equals the supply. A consumer faced with these prices would make his purchases of the various goods and services so as to maximize utility. An individual who used resources to make a product for sale would aim to maximize profits, the difference between what he would receive for his product and what he had to give up to obtain the resources required to make it. Costs become explicit as a result of the existence of the demand of others for resources. However, the prices of the products he sells and of the resources he uses are not the result of any calculations he makes but are given to him as a result of the operations of the market. Since, leaving aside the effects of monopoly, the prices paid for resources must be equal or (slightly) greater than they would yield in another use or to another user, cost (the price of the resources) *is* opportunity cost, and resources will be employed in such a way as to maximize the value of production.

This is probably the way that most modern economists look at the working of the economic system, and it undoubtedly brings out some of its most important feature. But, as I've often said, it assumes that the operation of the market is costless, which it is not. Resources are not employed in those uses where they make the most valuable contribution, not because of the effect of such factors as the cost of transport, which is commonly taken into account, but because of the existence of what has come to be called transaction costs. I have argued, and I need not repeat the argument here, that firms emerge because they are able to achieve some of those more productive arrangements of factors of production which, because of their cost, are made impossible if one relies solely on market transactions.

I said in 'The Nature of the Firm' (and I have not changed my mind) that the expansion of a firm will halt at the point at which the costs which it has to incur to organize an additional transaction within the firm become equal to the costs of carrying out that same transaction on the market or to the costs of organizing it within some other firm. But what determines where this point will be? It cannot be said that we see very clearly the answer to this question, although as a result of the work now going forward the mists are beginning to clear. My present feeling is that, while transaction cost considerations undoubtedly explain why firms come into existence, once most production is carried out within firms and most transactions are firm–firm transactions and not factor–factor transactions, the level of transaction costs will be greatly reduced and the dominant factor determining the institutional structure of production will in general no longer be transaction costs but the relative costs of different firms in organizing particular activities. This does not mean that transaction costs will not be important in particular cases nor that they will not be important in determining the form of the contractual arrangements made by firms. What it does mean, if I am right, is that, as I put it in my Yale lectures, 'to explain the institutional structure of production in the system as a whole it is necessary to uncover the reasons why the cost of organizing particular activities differs among firms'.

This leads us to a point of particular interest to the participants at this conference. If economists are to study the determinants of the costs of organizing various activities within firms, they will have to call in the assistance of accountants since the costs of organizing clearly depend on the efficiency of the accounting system. It is in this connection that Gould's view that the opportunity cost concept is not useful for business computations, since all that is required is to maximize profits, seems to me wrong. In a firm, men also make decisions independently of those that others are making (there is delegation of responsibility and all decisions are not the product of a single mind) and when people in a firm use its resources, they often need to be given some figure representing their costs, so as to be able to compare it with receipts. Since using a resource denies its use to others, the figure for cost should represent what it would yield elsewhere in the business. In this planned society, the firm, costs do not, in the main, arise directly out of the operations of the market but are computed and provided by the accounting system. While outside the firm prices and therefore costs are explicit (because of the demands of others for resources) and are determined by the operations of the market, within the firm there are explicit costs for exactly the same reason but they are provided by the accounting system. This internal system takes the place of the pricing system of the market.

In 'Business Organization and the Accountant' I was concerned that these accounting calculations be properly made. But doing this is, no doubt, more difficult in some circumstances than in others – and we need to know what these circumstances are. When it is difficult, because of the particular activities or combination of activities in which the firm is engaged, the costs of organizing will be greater – either more mistakes will be made or additional costs will be incurred to avoid making them. As a result, the activities which we find firms undertaking must be influenced to some degree by their effect on the efficiency with which the accounting system operates.

Most production in a Western economy is carried out in this planned society, the firm, and when a firm decides not to take on another activity, what normally happens is that it will be done by another firm, if it is done at all. In a footnote in 'The Nature of the Firm', which so far as I know has never been commented on, I argue that in a competitive system there is an optimum amount of planning, and, I could also have added, an optimum quantity of market operations. In understanding how in a competitive society the choice is made between these alternative but interrelated means of organization, we must take into account the role of the accounting system. The theory of the accounting system is part of the theory of the firm. It is not my belief that the secret to the determination of the institutional structure of production will alone be found in the accounting system, but it certainly contains part of the secret.

I suggested that the accounts could be a valuable source of data on firm behaviour, and, if I am right, it follows that their use could greatly assist in the development of a theory of the firm. Then I argued that a theory of the accounting system is part of the theory of the firm. If this view comes to be generally recognized, we may expect to see a growth in interdisciplinary studies between economics and accounting. I hope this happens, and it would certainly give me great pleasure to take up once again the investigation of those interrelations between economics and accounting about which I and my colleagues at LSE held such high hopes some fifty or more years ago concerning what could be achieved. What I have foremost in my own mind is the gain to economics that would result. But it seems reasonable to suppose that an increased understanding of the part which accounting plays within the firm will not be without some advantage to accounting.

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