1. The Nature of the Firm: Origin

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"It is difficult for a man to speak long of himself without vanity." So said David Hume at the beginning of his autobiography. If David Hume felt such a warning to be necessary, a man who, according to Adam Smith, approached "as nearly to the idea of a perfectly wise and virtuous man, as perhaps the nature of human frailty will permit," it is hardly to be expected that my lectures will be free from vanity. However, a natural tendency to be over-indulgent in dealing with my own work will be somewhat curbed by a real desire on my part to understand why "The Nature of the Firm" has been treated by the economics profession in the way it has. It is an article which is usually referred to with respect, has been reprinted in a number of books of readings, including *Readings in Price Theory,* sponsored by the American Economic Association, but which, until comparatively recently, has been largely without influence. It has had what the French call "un succès d'estime." To understand why this has been the case would be more difficult, if not indeed impossible, if I did not deal honestly with the circumstances surrounding its writing, with the ideas that it was intended to convey and with how they were expressed.

There is no problem in discovering when I formulated the ideas to be found in "The Nature of the Firm." They must have crystallized in my mind sometime in the summer of 1932. In October 1932 (or thereabouts) I was appointed an assistant lecturer at the Dundee School of Economics and Commerce. Duncan Black, the other assistant lecturer, has recalled that I

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arrived in Dundee with my head full of my ideas on the firm. His recollection is confirmed by a letter I wrote on October 10, 1932, to my friend Ronald Fowler, who was an assistant lecturer at the London School of Economics. It went as follows:

October 10, 1932

Dear Fowler,

Thanks very much for your letter. I am very sorry that I have not written to you before but I am terribly busy preparing lectures at the moment. Think of three courses all starting now! I am snowed under with work.

I am giving a course on organization of the business unit. I have given my first lecture on it. As it was a new approach (I think) to this subject, I was quite pleased with myself. One thing I can say is that I made it all up myself.

I first of all inquired the reason [for] an organization of the business unit which, the business unit, I said existed when anyone produced to sell in a market. This implied exchange and exchange specialization. But this specialization is a specialization of the business unit—it need imply no specialization within the business unit. I then differentiated between the regulation of the efforts of business units and that of individuals within the firm. The effort of business units in a market not organized—supply and demand, etc. I quoted that Salter piece in Plant's article. I pointed out [that] if there were atomistic competition, where every transaction involving the use of another's labour, materials or money was the subject of a market transaction, there would be no need for an organization. In fact, this is not so. Why? I found the reason in the costs of conducting these marketing transactions. Think of the inconvenience (increased cost) if every time someone worked with someone else, there had to be a market transaction. But if the transactions are not to be governed by the price mechanism directly there *has* to be an organization. Since under market conditions, the greatest use is made of the factors of production, [the] object of the organization was to reproduce market conditions, but production would be greater because of lowered costs. I then asked—if by eliminating market transactions, costs were lowered, why were there market transactions at all? That is, why are there separate firms? two reasons—

1. Increasing cost for each additional market transaction until cost of organizing marginal market transaction was equal to marketing cost of that transaction.
2. That as transactions increased, might not carry out its object of reproducing market conditions.

Then said, could not eliminate *ever* cost of selling to final consumer unless eliminated consumer's choice. Talked about organization in Russia, etc., etc. Concluded that object of organization was

(a) to reproduce distribution of factors under atomistic competition within the business unit

(b) to do so at a cost which is less than the marketing transactions it supersedes.

Of course, I was much fuller than the above. But I believe it was quite a good lecture. It certainly succeeded in linking up organization with cost. I intend to work up this argument a bit more. What do you think of the idea?

It is clear that this lecture, given when I was twenty-one, contained the main points which were later to appear in "The Nature of the Firm": the choice of the transaction as the unit of analysis, the concept of transaction costs, the distinction between the allocation of resources within the firm and

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through the market, the comparison of the costs of organizing a transaction within the firm and by means of a market transaction, and so on. The letter from which this account of the lecture is taken forms part of a correspondence with Ronald Fowler. In reconstructing the evolution of my ideas on the firm, I am relying to a considerable extent on this correspondence. It will be deposited in the Regenstein Library of the University of Chicago along with my other papers, notwithstanding that the boyish enthusiasm and juvenile expressions displayed therein now make me wince.

For some, their academic achievements have the character of inevitability. Had they not been attained by one route, they would have been by another. There was nothing inevitable about my writing "The Nature of the Firm." It came about as the result of a series of accidents. At my school (the Kilburn Grammar School, which would have been called a high school in the United States), after matriculating, which I did at sixteen, the normal age, it was possible to spend two additional years preparing for the external intermediate degree examinations of the University of London (the equivalent of the first year of university work). My inclination was to take a degree in history but having come into the school at twelve rather than at the usual age of eleven, there was no question of my taking Latin at school. This lack of Latin barred me from taking an arts degree (or at any rate the one I wanted to take). I therefore started to work for a science degree, with the intention of specializing in chemistry. However, I soon found that mathematics, a requirement for a science degree, was not to my taste, and I switched to the only alternative open to me at school, working for a commerce degree. This move was undoubtedly made easier by the fact that I was at that time a socialist and the interest in social problems that this implies made the prospect of studying economics (a requirement for a commerce degree) attractive. During the next two years I studied economics, geography, French, English economic history and accounting (this through a correspondence course since accounting was not taught at my school). Although I knew next to nothing about these subjects, I managed to pass the examinations, the standard required obviously being rather low. In 1929, I went, aged eighteen, to the London School of Economics (LSE) to continue work for a commerce degree.

In 1929–30, I worked for Part 1 of the final examination, taking courses in British foreign trade, the principles of currency, the theory of production, industrial relations, the economic development of the overseas dominions, India and the tropical dependencies, commercial law, statistical method, the organization of transport and psychology. In 1930–31, I decided to take group D (industry) in Part 2 of the Bachelor of Commerce, a group which was recommended for those wishing to enter the engineering and metal trades, distributing trades (in certain instances), and generally for those engaged in works and factory management. It was a choice of occupation for which I was singularly ill-suited, but what else was there for someone to do who did not know Latin and did not like mathematics? In studying for Part 2 in 1930–

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31, I took courses in French, accounting, business administration, works and factory accounting and cost accounts, the raw materials of industry and trade, problems of modern industry, the financing of industry, industrial law, and industrial psychology.

It will have been noticed that during my two years at LSE I studied a great variety of subjects, devoting therefore very little time to each and inevitably doing no systematic reading. I took no course in economics, and although some of the courses had an economics content, most did not. The courses to which I devoted the most time were those on law, particularly industrial law. I was fascinated by the cases and by legal reasoning. As a result of taking the B. Com., I knew a little about economics as well as a little about law, accounting, and statistics, a mixture of subjects which was not without effect on my subsequent work. But it was the teaching of Arnold Plant that had the greatest influence on me at LSE. I was the beneficiary of this by an extraordinary piece of luck. Plant, who had held a professorship in commerce at the University of Cape Town, South Africa, was appointed a professor of commerce (with special reference to business administration) at LSE in 1930. I attended Plant's lectures on business administration, but it was what he said in his seminar that really influenced me. I can still remember the impact of the first meeting of the seminar which Plant held. At a conference in Los Angeles in 1981 I described the dramatic effect this had on me: "We had been having a seminar previously by a man called Sargent. . . . Plant said, 'What have you been discussing?' And I said, 'Oh, we've been discussing policy in the oil industry. We've been contrasting the fine conservation policies which are carried out by the Anglo-Persian oil company with the wasteful policies they pursue in the United States.' And Plant said, 'Well, that is very interesting,' and then he started asking a few questions. After a little bit we discovered that the oil companies weren't maximizing their profits. And then we discovered that the government knew more about the oil industry than the oil companies did. That seemed odd. Even worse, that we knew more than the oil companies did. That was hard to take. Then we learned . . . that . . . a large part of the cost of producing oil is the present discounted value of the future receipts, and so on."[1] This seminar took place some five months before I took Part 2 of the final examination. Until I met Plant my economic views were extremely woolly. From him I learned that producers maximize profits, that producers compete, and therefore that prices tend to equal costs and the composition of output to be that which consumers value most highly. Plant also explained that governments often served special interests, promoted monopoly rather than competition, and commonly imposed regulations which made matters

1. Edmund W. Kitch, ed., "The Fire of Truth: A Remembrance of Law and Economics at Chicago, 1932–1970," 26 *Journal of Law and Economics* (1983) 214.

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worse. He made me aware of the benefits which flow from an economy directed by the pricing system. Clearly, I did not need Chicago.

If Plant gave me my basic approach, I learned such economics as I knew mainly from discussions with fellow students. Chief among them was Ronald Fowler, who had been away in France on a Cassel travelling scholarship during my first year at LSE but had returned to follow the same courses that I did during my second year. Fowler had a fine analytical mind and I greatly benefited from our discussions. I also benefited from discussion with other students, particularly those specializing in economics, among whom were Vera Smith (later Vera Lutz), Abba Lerner, and Victor Edelberg. Ronald Fowler and I were particularly interested in costs and the construction of cost curves. However, the subject which dominated the discussion of economics at LSE in the last few months before the final examinations was far from my main interest. It was the structure of production—not the organizational structure of production that was going to absorb me but the Austrian capital structure of production, both teachers and students at LSE having been captivated by Hayek's public lectures given at LSE in February 1931, which were later published in *Prices and Production*. My performance in the final examinations was undistinguished but was at least sufficient for me to be awarded a Cassel travelling scholarship for the academic year 1931–32. As I had spent only two years at LSE and three years' residence was required for a degree I was not awarded the B. Com. until 1932, my year on the Cassel scholarship being counted as a year's residence at LSE.

I proposed to spend this year in the United States and to study what I termed vertical and lateral integration in industry. My choice of subject (and it was my choice) undoubtedly resulted from the fact that Plant had referred to the different ways in which industries were organized. What stimulated my interest was that we seemed to lack any theory which would explain why those industries were organized in the way they were. My year in the United States was essentially devoted to a search for a theory of integration. This is quite explicit in my letters to Fowler. But there was another question which, while not directly concerned with the ways in which different industries were organized, was in my own mind related to it. Plant was critical of the rationalization schemes that were at that time advocated for a number of industries in Britain, and he was particularly hostile to proposals for the coordination of the various means of transport, a subject on which he presented a paper to the Institute of Transport in 1931. Plant argued that competition would provide all the coordination needed. Yet we had in economics a factor of production, management, whose function was to coordinate. Plant in his inaugural lecture at LSE in October 1931 spoke of the consumer as the "ultimate employer" in the economic system and he quoted Arthur Salter: "The normal economic system works itself."[2] (This passage was referred to

2. Arnold Plant, "Trends in Business Administration," 12 *Economica* (1932) 387.

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in my Dundee lecture and later cited in "The Nature of the Firm.") Plant's point of view seemed to me to leave obscure the role of business management and of the employer-employee relationship. I did not dispute what Plant was saying. Indeed, it made a lot of sense. But it seemed somehow incomplete.

What was essentially the same puzzle presented itself to me in another form which can be summed up in one word, Russia. It would be very easy for someone today to have a mistaken idea about the situation as we saw it in 1931. The storming of the Winter Palace in St. Petersburg had taken place in October 1917, some fourteen years previously. After a period of war and civil strife and an initial period of centralized control, Lenin had instituted the New Economic Policy. He died in 1924 and Stalin did not become the undisputed leader of Russia until 1928 when Trotsky was exiled. The first five year plan was not adopted until 1928. So, at the time I was a student at LSE, from 1929 to 1931, it was not easy to form a view of how planning in Russia would actually work. We had heard of the construction of the vast Dnieper Dam on the Volga and I went to see its giant generators being made, at the General Electric works in Schenectady. But detailed knowledge was hard to get. Lenin had said that the economic system in Russia would be run as one big factory. Although it was undoubtedly a good deal easier to find out what was happening in Russia then than now, there was very little experience of economic organization in Russia to go on and economists in the West were engaged in a grand debate on the subject of planning, some maintaining that to run the economy as one big factory was an impossibility. And yet there were factories in England and America. How did one reconcile the impossibility of running Russia as one big factory with the existence of factories in the western world? At the time I considered myself to be a socialist and one of the people I went to see in the United States was Norman Thomas, the socialist candidate for president. One may ask how I reconciled my socialist sympathies with acceptance of Plant's approach. The short answer is that I never felt the need to reconcile them. I would only recall that a fellow student, Abba Lerner, who, in the preface to his *Economics of Control*, acknowledges Plant's influence in the development of his views, went to Mexico to see Trotsky to persuade him that all would be well in a communist state if only it reproduced the results of a competitive system and prices were set equal to marginal cost. In my case my socialist views fell away fairly rapidly without any obvious stage of rejection.

During my time in the United States I attended very few classes and although I visited a number of universities, most of my time was spent in visiting businesses and industrial plants. Before I went to America, Ernest Bevin, the secretary of the biggest trade union in Britain, the Transport and General Workers Union, had approached Bruce Gardner of the Bank of England on my behalf. Gardner gave me letters of introduction to various businessmen in the United States, and as a result I had little difficulty in meeting them. I still remember one most instructive day I spent in the office of a

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purchasing agent, I think at Union Carbide, listening to his telephone conversations, a visit which gave me a lively sense of the possibilities of substitution. While in the United States I read the reports of the Federal Trade Commission and books describing the organization of different industries, such as Copeland's study of the cotton textile industry. I also read trade periodicals and used more unusual sources (for an economist), such as the yellow pages of the telephone directory, where I was fascinated to find so many specialist firms operating within what we thought of as a single industry as well as such interesting combinations of activities as those represented by coal and ice companies. What I was in fact doing in America was looking for clues which would enable me to solve the puzzles that I took there with me. The extraordinary thing is that I think I succeeded.

In recounting how this came about I am fortunate in being able to use my correspondence with Fowler. Not all the letters in the correspondence have been preserved, but enough have survived to indicate what I was thinking about as I investigated the problem of integration. The correspondence is not, of course, wholly concerned with this subject. Fowler told me about Viner's article in the *Zeitschrift für Nationalökonomie* for September 1931, an article that, after I had read it, I described as a "magnificent achievement." He told me of Yntema's article in the *Journal of Political Economy* for 1928, which Hicks had been discussing at LSE and which disputed Viner's conclusion that dumping would not affect the price in the home market. As a letter to Fowler shows, I completely failed to understand Yntema's argument. I was also baffled by Fowler's reference to a "marginal receipts curve." In addition, Fowler discussed his own work on the economics of depreciation that was later to result in his *Depreciation of Capital.* This led to a discussion of more general questions, such as the utility of a concept of "profit" or a "dynamic surplus." Nonetheless, the main question considered in my own letters was the development of a theory of integration. It is somewhat embarrassing to me to quote from these letters in which my innermost thoughts are expressed with great frankness and without of course any expectation that they would ever be published. As I quote from these letters I can only hope that readers will treat the aspirations of this young man as sympathetically as I do.

I must first mention some thoughts in my letters which, while they did not lead directly to the breakthrough that enabled me to solve my puzzles, yet played their part in the evolution of my ideas and certainly came into their own once that breakthrough had been made. In a letter dated February 28, 1932, I said: "I am developing a theory that economic integration is the result of the limitations of small-scale production—in essence it is the joining up of small-scale producers in different industries in order to get the advantages of large-scale production. Therefore I would regard the linking up of units already producing on a large scale as most probably unsound." Within less than a month, however, I was expressing uneasiness

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about the concept of integration and its analysis. On March 24, 1932, I said that I had been thinking of integration as "the bringing together under one control of different functions." However, it wasn't clear to me how this fitted in with our view of the role of specialization. In any case I thought the way to proceed was to examine the effect on costs of bringing various combinations of functions under one control, and I illustrated my position with diagrams somewhat in the manner of Stigler in his 1951 article, "The Division of Labor Is Limited by the Extent of the Market," although of course without any of his sophistication. This led me to argue that the distinction between vertical and horizontal integration was without value. "What is important is that different functions are in fact brought together under one control, what stage they are in being of little account."

Fowler, in replying to this letter, gave a particularly clear account of the relation, as he saw it, between integration and specialization. He said: "The whole problem of integration seems to be this. All economies, lowering of costs by more efficient methods are as a rule due to greater specialization. Horizontal expansion depends for its greater productivity on the greater productivity of specialization and in fact you can say that specialization and horizontal expansion are two aspects of the same phenomenon. The difficult question is why do we get economies from vertical integration since it is quite clearly a reversal of the process of specialization. Of course the easy cases such as vertical integration in iron and steel between smelting and rolling are due to special technical considerations which cannot be generalized into any concept such as specialization. Lavington, I believe it was, wrote an article denying any general tendency towards vertical integration (I mean apart from attempts to obtain monopoly). If one is of the opinion that there is a *general* tendency to vertical integration, one has got to admit that there is a general tendency to eliminate the middleman, in other words that *in competition* two middlemen are more efficient than 3 middlemen. It really amounts to denying the greater economy of specialization."

To this I replied as follows: "I am not so sure how far all economies are as a rule due to greater specialization. . . . I started with this attitude but found it was not very helpful since it is not clear really whether modern business is more or less specialized than it used to be. It is most probably true that manual operations are more specialized but what about management? . . . The question is rather—what is the economic degree of integration? And this you can only analyze . . . by saying in what ways can the joining together of two tasks hitherto separate affect the shape of the resultant cost curves. I think that this form of analysis will get you somewhere. The other concepts are too hazy to be of more than general use."

Fowler then commented: "I think it would be worthwhile going into the reasons for decreasing returns to managerial ability under conditions of equilibrium. Do decreasing returns arise from increasing complexity (the reverse of specialization) as you suggest or from some other cause? If, for example,

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you are a firm specializing in the production of a particular commodity you go on expanding to a point where decreasing returns from managerial ability arise. Is it possible that these decreasing returns are due to the fact that the firm now has become not more specialized but less? For a likely cause of decreasing returns lies in the wider range of markets which are now supplied by the firm. A wide range of markets means less specialization, greater complexity. Where different markets require different treatment because of different conditions, there must be a tendency inducing specialization for each market among producers." I responded to this point with enthusiasm: "I think your remark about decreasing returns to managerial ability arising not because the firm is more specialized but because it is less hits the nail on the head. There may be technical advantages in increasing complexity but it is decreasing returns to managerial ability which seems to set the limit." I go on to give an example: "When I was looking into the rubber tire industry, I considered the position of chemicals for the industry. Now, when you read American trade journals, it was obvious that these chemicals were marketed by the firms themselves. You could gather this from the advertisements. But if you looked at English trade publications, the advertisement for the particular chemical appeared under the heading—X and Co.—Agents for XYZ Chemicals. That is to say, the selling in a foreign market was left to people who specialized in that market. The chemicals considered were, of course, American chemicals."

Fowler also said that my approach "to the question—what is the economic degree of integration?—by means of finding out in what ways the joining together of two tasks hitherto separate can affect the shape of the resultant cost curve is a very good one. I hope however that you will be able to generalize the results in some form or other." To this I did not reply and for what I believe was a good reason—the approach, so promising, was going nowhere. What the correspondence with Fowler had done was to indicate some of the problems. Why was it that what seemed to be a movement away from specialization was more efficient? Why should we make a distinction between vertical and horizontal integration? And why was it that costs could be lowered by grouping together certain activities under one control? I was very conscious that I had not got a handle on these problems. In my letter of March 24, 1932, after characterizing my own approach via the effect on costs of putting different functions under one control as "very weak," I go on to lament the lack of help I received from economists either in discussion or through a study of their writings. "The ignorance of the academic world on this subject is amazing. I think it is true to say that I have yet to discuss this question with any professor who has clearer ideas on it than I have." I add in the margin, "I am very hazy." I continue: "This becomes obvious when we go on to discuss the major part of the problem—the classification of the reasons for the change in the shape of the cost curve. Some of the reasons they give are absolutely untenable. If you want to know the type of thing

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just read the literature of the subject—it is absolute bilge." What I undoubtedly meant by characterizing the literature as "bilge" was that it made assumptions which contravened some of Plant's basic positions. I have forgotten the specifics, but examples would be arguments which assumed implicitly that producers were not maximizing profits or that producers in one industry knew more about the problems of some other industry than the producers in that industry. I continued after the reference to "bilge": "The above may seem very boastful to you but it does not come from a particularly joyful heart. My criticism of the literature has been destructive, not constructive. But I still have hope of constructing a theory of integration (if it exists!)."

The route which was ultimately to take me to a theory of integration is a curious one, and the more so because it came about in part as a result of examining the role in promoting vertical integration of what is now called asset-specificity. Let me start by quoting once again from my letter of March 24, 1932, which was written in Cleveland, where I was visiting the Union Carbide and Carbon Company: "Before leaving New York, I went and saw Wassily Leontief, the mathematical economist whom I think I have mentioned to you before [this earlier letter has not been preserved]. He used an argument for integration that I had already rejected as not at all satisfactory. He said, suppose a company has a steady demand of a certain amount and fluctuating demand above this. Then it might pay the company to produce the steady demand and buy the fluctuating demand outside. My arguments against this were as follows—Following on Plant's usual mode of attack, if it would pay to produce this amount, why does not the raw material producer set up a plant or whatever the equipment is, and produce this regular amount?" I then noted that it might be cheaper to operate two plants, each with a moderately fluctuating output, rather than one with a constant and the other with a greatly fluctuating output but went on to introduce a qualification which I said "might be of some importance." This qualification was that Plant's argument that I had deployed "assumed that the cost to the original producer and the original consumer who later produces, is the same." However, "if, for instance, one sets up a plant to cope with the steady demand of one consumer, there is obviously the risk that that consumer will cease to demand your product." I then noted that "it is obvious that many of the risks are borne ultimately by whoever produces the product, but that the risks inherent in this condition of bilateral monopoly may make the capital costs so high that it is cheaper for the original consuming firm to produce itself even though the actual operating efficiency is less. I have in mind certain actual examples of this same thing that I have come across." A remark follows that indicates the direction in which my mind was moving and that would start up a line of inquiry which would ultimately lead to the thesis of "The Nature of the Firm": "The above has suggested to me that I should investigate long-term contracting between firms. This may in many instances be considered as an alternative to actual operating integration, and [it] seems

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well worth the trouble of trying to get any information." I added that, although obtaining information is difficult, "occasionally I am well rewarded as when the other day in the case of a power plant which had integrated with some coal-mines, I was shown actual cost curves that they had constructed." Although I don't say this in my letter to Fowler, the power company concerned was the Duquesne Power and Light Company of Pittsburgh, and some years later I adapted their figures for use in an article on the rationale of cost accounting.

Fowler did not understand my argument about the "risks inherent in this condition of bilateral monopoly" making the capital costs greater for the producing firm. I replied: "I don't think the term bilateral monopoly is a good one and it may have confused you. The case I am considering is this. Suppose the production of a particular product requires a large capital equipment which is, however, specialized insofar that it can only be used for the particular product concerned or can only be readapted at great cost. Then the firm producing such a product for one consumer finds itself faced with one great risk—that the consumer may transfer his demand elsewhere or that he may exercise his monopoly power to force down the price—the machinery has no supply price. Now this risk must mean that the rate of interest paid on this capital is that much higher. Now, if the consuming firm decides to make this product this risk is absent and it may well be that this difference in capital costs may well offset the relative inefficiency in actual operating. An example of this reason for integration is to be found in the Ford Motor Company. I am well aware of the many other factors that have to be considered and this is rather a specialized case (at one time for certain products Ford was such a large consumer that he might be considered the only consumer)."

This was written in Detroit but at a time when I had not as yet visited the Ford Motor Company. No doubt what I said about Ford was based on what I had read or heard. I was later to visit the Ford Motor Company, but before I did so I visited General Motors, where no doubt I discussed their relations with suppliers, and I certainly saw a Fisher Body plant, Fisher Body having been acquired by General Motors about six years before. I wish I could now repeat what I was told by General Motors officials about their reasons for acquiring Fisher Body, but nothing appears in the correspondence except that I had visited General Motors. My recollection is that I was told that the main reason for the acquisition was to make sure that the body plants were located near General Motors assembly plants. That I say nothing about my visit to Fisher Body is not surprising since the vast majority of my visits to businesses and industrial plants are not mentioned and my letters deal mainly with economic analysis.

My Detroit letter, in a passage written later than the one I have just quoted, does however contain a detailed account of my visit to the Ford Motor Company but this was only because I wanted to tell Fowler how it

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was possible to make such a visit and obtain the information which I was seeking without having a letter of introduction. This is what I said: "I went over to the Ford Motor Company today. You may be interested in the tactics I adopted to get the information I wanted. You must first understand that I had no letter of introduction to the Ford Company and since the type of information I want can only be obtained by seeing more or less responsible officials I was in a little doubt as to how to approach the company. A certain Mr. Lansburgh of Detroit whose book on Factory Management you may or may not have read advised me to go about the job in the following way. When I arrived at the Administration Building of the Ford Company, I went in and asked if I could see Mr. Edsel Ford. Edsel is Henry Ford's son and he is now President of the Ford Motor Company and is worth in his own right about 500,000,000 dollars. I was told that he was out of town—I did not expect that he would be in town, of course. So I asked if I could see his secretary. I then filled out a form in which I said I was from England—I had come all the way to see Edsel sort of thing. A few minutes later a man arrived to see me and I then explained exactly who I was. You see, once you are in a firm, that is, have seen someone of importance, you are quite all right. I then had a short talk to this man about integration. By the way, I am quite a lawyer in my craftiness in putting questions. I can get admissions regarding costs out of them without them realizing that they have done so. Of course, I am aided in this by the fact that I do not want statistics. All I want are statements that are suggestive from the point of view of fitting into a theory of integration. After this I had lunch in the Ford cafeteria and was afterwards provided with a special man to show me anything I wanted to see in the plant. After going round the plant, I said I wanted to see someone in the purchasing department. So I was handed over to a gentleman who is, I imagine, the head of the purchasing department. I then discussed the problems connected with contracting for supplies, purchasing schedules and the like for about an hour and a half. Why I am mentioning this is not because of my treatment there being exceptional—I can always get almost whatever I want—but because I got all this without any introduction at all. . . . I really did get some most interesting points with regard to the relations of the Ford Company with its suppliers. Tomorrow, I am going to get the other side of the picture—I am going to visit one of Ford's suppliers. Of course, the Ford Company did not know this."

My next letter came from Chicago. While there I visited the steel works at Gary as well as Sears Roebuck, Montgomery Ward, and the stockyards in Chicago itself. I also went to Milwaukee where I visited Allis Chalmers and, what made a great impression on me, the A. O. Smith plant. But I did not omit to visit the University of Chicago. I attended a few classes of Frank Knight, who was talking about planning. I quote in my letter what I describe as a characteristic statement, "Property, competition and freedom are names for the same thing," a remark which did not do much to aid me in the search

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for a theory of integration. The only person with whom I discussed this was Jacob Viner. Viner had the reputation of being sharp with students but he was very kind to me. However, I'm not sure what I gained from him. I report in the letter to Fowler from Chicago that Viner said in discussing integration that it was not right to assume that producers were rational. In response I argued that one could normally assume rationality on the part of producers since if *one* producer were rational, his competitors would have to be (presumably because a rational producer would be more profitable and would displace his irrational and less profitable competitors). Of course I did not mean that firms would not sometimes follow mistaken policies. I report to Fowler a study I made of the purchases of rubber plantations by a rubber company, in which they purchased plantations when the price of rubber was high and disposed of them when the price was low. What I meant in not accepting Viner's point was that practices persistently followed in an industry would be those that were profitable. I also recall (which is not mentioned in the correspondence) that Viner suggested that there might be cases, such as the construction of a bridge, in which, say, a choice between two locations was a matter of indifference to almost everyone except one producer whose profits would be much affected by the location selected. This producer would have an incentive to build the bridge in the location most profitable to him. Viner, as I remember it, did not explain why the producer had to become the operator of the bridge rather than make a financial arrangement with the person or organization proposing to build it. It may well be that reflecting on this example had an important influence on the development of my views—but of this I cannot be sure.

As Fowler was still disputing my argument that integration might reduce risk, I set out in a letter from Chicago on May 7, 1932, a careful restatement of my position, with the accompanying remark that Viner thought it was sound. "Assume a producer of some finished product finds he needs a special part. Then, he has two alternatives. One, to produce it himself and two, to let a supplier produce it. Now, this specialized part requires a large capital equipment which is highly specialized, e.g., a special die [misspelt "dye" in my letter]. Now to an outside supplier, there is the risk that the consumer, that is, the producer of the finished product, may change his demand to another producer. If the consumer transfers his demand elsewhere, then the outside producer will find that he has a large amount of capital invested on which he is unable to get a return. Because of this risk, he will expect a higher return on his capital. But there will not be this risk in the case where the consumer, that is, the producer of the finished product, makes the part himself. Insofar as this one risk is removed, then to this extent, a saving is made." However, in this same letter are some remarks which very much reduce the practical importance of this argument: "As a matter of fact, my queries about the form of contracts for products requiring large capital equipment has shown me that contractual arrangements can be made to avoid this

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risk. Thus, the consuming firm may buy the particular equipment itself, even though it is in another company's plant. There are a number of other contractual devices which tend to get over this difficulty." To prevent Fowler from concluding that my argument about risk had no practical significance, I instance the case in which the "most economic production would occupy the whole of one plant" and I ask, "Is the consuming firm to buy the whole plant?" I also point out that if only one customer is supplied, there will be much capital investment which is of an intangible character. "When one is engaged in supplying only one producer the whole of the energies of the firm are turned in one direction—think of the goodwill invested in your market—I am thinking of goodwill as being created by capital investment." In such a case, "the only solution seems to be for the producing company to be bought by the consuming company." I add: "There is no doubt of the fact that suppliers will refuse to take too great a proportion of their business from one consumer even though there would be an actual saving in operating expenses if they did so. The risks could be too great! I have come across many instances of this."

There is only one more letter from America after the one I have just quoted (or at any rate only one that has been preserved), and this does not add much to what has gone before. The next letter in the correspondence is the one from Dundee from which I quoted at the beginning of this lecture. Lest I give a false impression of the extent of this correspondence, I should explain that there are only seven letters totaling sixty-four pages sent from America. The first letter, in which I talked of the risks of bilateral monopoly and the need to investigate long-term contracting between firms, was written on February 28, 1932, and the last letter from which I quoted, in which I spoke of the existence of contractual arrangements which avoided this risk, was dated May 7, 1932, so the period involved in the correspondence from which I have quoted was just over two months long.

The position at the beginning of the summer of 1932 was that I had developed this theory about the risk inherent in a situation in which a firm, in order to supply a particular customer, had to make a large capital investment. Viner thought the argument was sound but, what is more to the point, I thought it was sound. Furthermore, my inquiries had uncovered evidence to support it. I had come across "many instances" of suppliers refusing to take too great a proportion of their business from one customer. However, my inquiries had uncovered something else. They had shown that there were "many contractual devices" which would avoid the risk associated with making capital investments to supply one customer, and in fact independent firms commonly made such investments. The A. O. Smith plant furnished an example of this. On the other hand it seemed likely that this situation did sometimes lead to integration, and the acquisition of Fisher Body by General Motors might well have been an example of this.

The puzzle needing to be solved was clear. What was it that determined

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whether this problem would be handled by contractual arrangements between independent firms or by integration, the alternative which was already in my mind by the end of February 1932? There was also the problem posed by the usual economic analysis in which it is assumed that, in Salter's phrase, "the normal economic system works itself," and which apparently left no room for firms. I cannot say exactly how I came to hit upon the solution; getting an idea is the kind of process in which you don't know what you're doing until you've done it. The solution was to realize that there were costs of making transactions in a market economy and that it was necessary to incorporate them into the analysis. This was not done in economics at that time—nor, I may add, is it in most present-day economic theory. However, once taken into account, everything falls into place. The quest on which I had sailed across the Atlantic had been successful. I had reached China. Whether a transaction would be organized within the firm (in the terminology of the letters, whether there would be integration) or whether it would be carried out on the market by independent contractors depended on a comparison of the costs of carrying out these market transactions with the costs of carrying out these transactions within an organization, the firm. As I said in my letter of October 10, 1932, my approach "succeeded in linking up organization with cost."

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