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A letter to Sylos Labini

FRANCO MODIGLIANI*

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Dear Paolo,

As promised, in this letter I am going to jot down, more or less systematically, various comments on your monograph *Oligopoly and Technical Progress*. As I already told you, due to lack of time, I cannot say I read your book word for word. However, I believe I have studied it deeply enough to get a sense of the general development of your thesis, and I have examined some parts, especially part I, in quite some detail.

In order to do justice to your work and to refer to all the ideas, either critical or favourable, that the reading has aroused I would need a space comparable to that of your book. If I could discuss the issue personally, I would try to tell you "everything". As I have to do so in writing, and since I am a very reluctant writer, I will limit myself to partial and often disorderly comments. I will begin with a general comment, followed by more specific comments on certain parts and finally I will point out in detail some individual aspects that I have noticed during my reading.

* Transcribed from the source document and translated from the original Italian, this is a letter Sylos Labini received and later returned to Modigliani to assist him in writing his 1958 review article in the *Journal of Political Economy*. We also reproduce Sylos Labini's annotated remarks on Modigliani's comments, here in the footnotes. The underlining is in Sylos Labini's hand in the original, and all words in italics are in English in the source document. We wish to thank Sergio Modigliani for permission to publish the letter and Antonella Rancan for raising it to our attention. English edition edited by Carlo D'Ippoliti. A reproduction of the original Italian letter has simultaneously been published as Modigliani ([1956] 2014), "Una lettera a Sylos Labini", *Moneta e Credito*, vol. 67 n. 267, pp. 285-309.

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1. General comment

In general I found your monograph extremely challenging, *controversial*, simple and pleasant to read (except for part II of chapter II, which I found far less readable, but certainly due to the nature of the subject). My impression is that the most important and convincing elements are to be found in part I and, especially, in the theoretical model developed in chapters II and III. However, as I will go on to explain, I believe that its validity is much more limited than you suggest, despite the fact that I think your model can be improved and generalised in various directions. Nevertheless, I have certainly found it very interesting and original, if not in its elements, then at least in the manner in which they are combined, and that it represents an important contribution to the literature on imperfect competition.

I remain however very sceptical and *unconvinced* about your thesis as it is developed in the remaining two parts and especially in part II. As I will try to show later, I believe that your analysis in chapter II, part II, sections 3-9 is essentially erroneous, at least in the way you present it. Despite all your reasoning, that is mostly not new to me, I still maintain my opinion, or better my firm conviction, that technical progress *per se* may offer an explanation for certain phenomena of chronic unemployment only in conjunction with certain assumptions – indeed fairly reasonable – on real wage rigidity. In other words, technical progress may lead to unemployment that can be re-absorbed only through a fall in real wages; if the latter are rigid, unemployment might tend to persist. This possibility exists under the hypothesis of perfect competition and we may reasonably expect it to be more likely in the situations of imperfect competition you analysed. Whether this possibility may be empirically relevant is a point on which I believe there is not much evidence. However, without reference to wage rigidity, as I already mentioned, I do not believe we may establish any causal effect between technical progress and unemployment,[1] as you try to do. I must add that

[1] Sylos Labini noted: "see pp. 143-146 of the new edition".

there is no reason to think that wages should tend to zero to re-absorb the displaced workers (as you write on p. 128), because capital accumulation works in the opposite direction, that is, it tends to increase real wages. This is also true in the case of technical progress that is *capital saving* instead of *labour saving*. Thus, it seems, the possibility that absorbing unemployment may require a reduction of real wages occurs only when capital and its accumulation are scarce. Where there is a tendency toward *oversaving* the problem does not seem to exist. It is interesting to note that, in this respect, the *stagnationists*' concern refers to a *capital saving* technical progress rather than to a *labour saving* one, which, by freeing up labour force, allows for the absorption of otherwise excessive saving, in order to equip the displaced workers.

I see that I am going into too much detail when I had promised in this first section to restrict myself to general comments, which I have already exhausted. Thus, I will now proceed more systematically with specific comments on the various parts.

2. Specific comments

A) *Part I*

As I already indicated I find very interesting and promising the essential elements of the theoretical model of "concentrated" oligopoly. (But it's a strange name that makes me think of tomato sauce!). I refer to your two central ideas: 1) that the long run equilibrium price does not attract new firms; 2) that in the case of technologies which are available only at very different scales, it is possible that the equilibrium price (as you define it) allows considerable profits for the larger firms, which cannot be eliminated by competition. Your definition of equilibrium price is an obvious extension of that applied to the classical model of competition, and this is a clear merit; the classical definition also contains the condition that it must not be advantageous for the incumbent firms to change their production. In your model this second condition is satisfied by the assumption that every firm works at its full capacity.[2] This premise contains some awkwardness and this is one of the flaws in your model, as I will soon make evident. One of the interesting effects that you correctly highlight is that at the equilibrium price the demand may well have low elasticity, a possibility that is usually excluded from the standard models of monopoly and imperfect competition. (Parenthetically, I do not see why on p. 45 you introduce a very special definition of demand elasticity, a definition that for me is annoying because it is so different from the Marshallian one. But the strangest thing is that this definition, it seems to me, is not used in your subsequent analysis; then why not eliminate it altogether?).[3]

I find that your model, in its present state, introduces a large number of extremely rigid premises and it is not easy, without a large amount of additional work, to establish if these obviously unrealistic hypotheses may be modified without demolishing its fundamental results. I do not know if I have expressed myself clearly: there can be no objection to introducing clearly unrealistic assumptions for the purpose of analysis or clarity of exposition, providing that the fundamental results remain valid even when we modify or drop them. But, if the results change as the premises change to approach reality, the value of the results will be immeasurably diminished. I have the impression that several of your assumptions may be *relaxed*; for example the assumption that every technology has a well defined scale of production:[4] I believe that it would be sufficient to assume that every technology needs a minimum scale of production, i.e. that it has an inferior limit, but not necessarily a superior one, except that when a certain scale of production is reached it will be more economical to switch to another technology (this is indeed the essential argument of Viner's *envelop*[*e*], and the origin of his most

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[2] Sylos Labini noted: "this is not my assumption: the assumption only refers to the maximum scale of production, p. 51 of the new edition".
[3] Sylos Labini noted: "yes: it is applied in the numerical examples, ≠ finite elasticity = 1 = page 54".
[4] Sylos Labini noted: "no, only a superior limit".

famous error).[5] Thus, for example, in your numerical example on p. 51, method B would be more convenient for a production run of 1000 to 6000 and method C for a production run above 6000. However, considering this matter further, it seems clear to me that if there were no *diseconomies of scale*, at least for the large scale technology, the solution would be quite different from the one proposed, because there will be a tendency to have only one firm for each production process and a price sufficient to prevent competition. It is interesting that this price could be any one of the equilibrium prices obtained under your hypothesis. For instance, in your first example on pp. 52-53, the equilibrium price would remain 19.5 since it satisfies your definition of equilibrium. Thus, various conclusions would be valid, in particular those referring to the effects of technical progress, which would only affect the maximum scale technology. However, this solution appears unsatisfactory since it obviously does not coincide with empirical observation, which indicates that a number of firms of different sizes usually coexist in the same industry.

Another potentially far more serious criticism of your model refers to the hypothesis (or rather the axiom)[6] that a potential entrant, in assessing the expediency of entering a market, does so by estimating what the price would be if it produced at full capacity and the incumbent firms also continued to produce at their full capacity, even after the entry of the new firm. This assumption is highly questionable, especially for medium size firms and when the market is relatively large in comparison to the size of the company, because the potential entrant can, in his own right, presume to obtain a market share at the current price thanks to an output reduction of the incumbent firms, especially the larger ones that are 'price leaders'.[7] (The market size in respect to a certain technology

[5] Modigliani probably refers to Viner's statement that the long-run average cost curve is the result of the minimum point of each short-run average cost curve: Viner J. (1931), "Costs Curves and Supply Curves", *Zeitschrift für Nationalölkonomie*, vol. 3 n. 1, pp. 23-46.
[6] Sylos Labini noted: "no: see the assumption on the maximum scale of production, p. 52, which does not rule out the possibility of production at a level lower than the maximum".
[7] Sylos Labini noted: "it is not necessary, p. 56, fourth paragraph, of the new edition".

may be defined as the ratio of total sales at current prices divided by the (minimum) convenient output allowed by the technology). Thus, in your example II' 2b on p. 50, a medium size firm knows that at the current price it may achieve a profit of about 8% (table on p. 51). If the other firms would insist on maintaining their output, the price would fall to about 18.7 at which point the profit of the potential entrant also disappears (that is to say it falls below the profit rate achievable in other sectors). If we are in the presence of *price leadership* this would require large firms to make such a conscious decision; but such a choice would clearly be against their own interest, since the price reduction would reduce their profits more than maintaining the price would, even though the reduction of production required to maintain the price would be entirely absorbed by the larger firms. Indeed it is easy to calculate that reducing the price to 18.7 and maintaining the production level, the total profit of larger firms would be reduced by 16800 units, while maintaining the price at 19.4 and absorbing the entire output reduction of 1000 units would mean the profit only decreases by 5400. Following this reasoning it appears that the only sure way to prevent the entry of medium-sized firms is to set the price at a level that does not make it profitable for them to enter the market, that is a price of about 18.9 (or perhaps a little higher because, if the profit achievable by a new firm is at its minimum margin and there still is excess capacity that threatens to cause a price war, there is no incentive to enter the market). But then, you would end up with firms of no more than two sizes and a large part of the 'beauty' and 'elegance' of the model would disappear.[8] What's more, following the same reasoning, we can see that in the initial situation described on p. 59, it could even be advantageous for a large firm to enter the fray since sharing their sales of 24000 units with the other three larger incumbent firms (and thus without disturbing the small firms), each one would produce 6000 units at the initial price of 19.4, with a profit on sales of about 8%, which is well above average. (It should be noted at this point

[8] Sylos Labini noted: "? He does not take into consideration the initial situation (= it depends on previous history)".

that the deficiency of your notion of the rate of profit being calculated based on sales appears clear: with the increase of the firms' size the fixed capital/sales ratio rises and a given profit on sales means a rapidly decreasing profit on fixed capital – or on fixed and circulating capital).

The conclusions which follow are quite clear and not very encouraging for your model: 1) your equilibrium price (or at least some of your possible equilibrium prices) are in fact no equilibrium prices when we consider the possibility that a new firm could tend to enter believing in the possibility of obtaining a share of the market at the expense of incumbent firms.[9] It follows that some firms tend to enter reducing the price, or forcing others (especially the larger ones) to reduce [their output]. This may be avoided only through a price which forestalls entry, even when the incumbent firms are willing to reduce it; 2) this implies a price at which at most firms of two sizes can exist;[10] 3) we tend towards an equilibrium à la Chamberlin-Robinson where extra profits are eliminated by excess capacity;[11] 4) and, in the presence of homogeneous products, we sadly go back to the problem of defining the market shares of the incumbent firms, since these are able to produce more than the market demands.

There may be a way out of this complication, which consists in supposing that the incumbent larger firms can forestall potential competitors by sending a clear message that they are willing to temporarily cut prices to make the entry of new firms unprofitable; and since they have lower costs they certainly can cut the price to a level which is still profitable for them but not for firms of smaller size. But once we choose the path of threats and strategic games, solutions become quite different and indeterminate; for example there may always be the possible solution of a single firm with a monopoly price and sufficient capacity to reduce the price to a level below the convenient point for smaller firms. (The monopoly price would be the most convenient one given the demand curve and the existence of production capacity of the

[9] Sylos Labini noted: "?".
[10] Sylos Labini noted: "?".
[11] Sylos Labini noted: "I deal with this case, p. 56".

kind specified above). Nonetheless, even this solution can be unstable because it may still attract or permit the entrance of a new large firm with the hope of forcing the existing firm to share the market at the monopoly price or something near it.

(By the way: while writing I feel that I am expressing myself in the most horribly barbaric Italian, especially when I get excited about the subject; however, I think it is better for me to write like this, in one go, rather than to waste hours rearranging the sentences. I hope that in general my primitive level does not fall below the understandable; if you would find it useful to refer publicly to some of my comments, I am giving you now the unconditional authorisation to *edit* my text within the limits of the minimum necessary).

At this point you may wonder if my criticism amounts to a complete rejection of your model. Without having reflected enough on this, I believe that the answer is negative. More precisely, I think that your model may actually have considerable value as a first approximation where we are dealing with a relatively small market, which at the minimum price allowed by the technology of maximum scale could absorb less than the output of two firms of maximum size. There are various reasons that I suggest this conclusion, some of which I will try to explain.

I) Even accepting your original model without any qualifications, one can demonstrate that the coexistence of firms of very different size is quite unlikely in a *large* market. Let the subscript j denote the technology of size j, after *ranking* the technology in order of increasing size, in a way that technology 1 is the smallest one. If p_j^m is the minimum price allowing the existence of technology j we already known that in your model, that I will label model S, the equilibrium price may not be greater than p_1^m. It is also clear that a necessary (even if not sufficient) condition for the existence of firms of size 1 is that at the price p_1^m the demand curve has to be such that with an increase in quantity x_j (where x_j indicates the minimum producible output of technology j) the price would fall below p_j^m; formally this condition can be stated as follows, where $x(p)$ denotes the saleable quantity at price p and $p(x)$ the market price of quantity x:

$$p\,[x(p_1^m) + x_j] < p_j^m \qquad \text{for any value of } j \qquad (1)$$

If this condition is not satisfied for whatever j, for example for $j = 3$, then it will be convenient for a firm using technology 3 to enter the market, this implies that under hypothesis S (that all firms maintain their production level until they do not leave the market) the price will fall below p_1^m eliminating all firms of size 1 (or probably also firms of size 2 if the price falls below p_2^m).[12] Now, if the market is large with respect to the maximum technology, it will be much larger with respect to a technology j that is inferior, or significantly inferior, to the maximum one. Thus, an increase in output of x_j will represent a quite negligible percentage increase in production, and although the demand is not very elastic, the fall in price will be negligible as well. Hence, if the cost difference among various technologies is less than negligible (which is necessary to establish important differences in the level of profits) condition (1) will not be satisfied, at least for quite small values of j. Thus, I conclude that when the market is large we will tend to have only relatively large-scale technologies and, consequently, the number of technologies that coexist (and hence also the extra-profits) will tend to decrease approaching limit 1 (and limit zero as regards extra-profits). This reasoning is only further reinforced when we take into account my previous criticisms, because if existing firms are willing to reduce their output to avoid a fall in price, it is still more likely that condition (1) will not be satisfied (this condition obviously needs to be slightly modified, p now corresponds to the price after entry and after possible output reduction on behalf of existing firms). This conclusion is extremely reasonable when we consider that at the limit of an infinitely large market, we have the case of perfect competition in which only firms of convenient size can coexist, that is those firms adopting the maximum scale (and the extra profit is zero). You as well, in the last sentence of section 6 (page 60) reached similar conclusions although on a different

[12] Sylos Labini noted: "? No, this is not my hypothesis. The interpretation in this specific point is not exact".

and perhaps less rigorous basis. However, I want you to notice that your last conclusion of section 6 is extremely inexact; <u>if many plants of maximum size exist, the industrial concentration is zero</u>. Conclusion: even if we take model *S*, the coexistence of various levels of different technologies – which is a *very important*[13] element of the rest of the analysis of model *S*, can be expected only when the market is relatively small.

II) When we abandon the pure model *S*, and we take into account my criticisms of the model, it appears that a policy of threat towards foreigners combined with a policy of *live and let live* towards incumbent firms is much more easy when there are only one or few large firms able to exercise effective *leadership* and create a record on their attitude towards new invasions. On the other hand, when the market is small, thus making it easy to forestall entry with a policy of potential threat, we can have various reasons which lead large firms to tolerate or even support the existence of smaller firms with higher costs, until they behave in a disciplined way; for example in the U.S., if industrial concentration becomes excessive, the threat of the Antitrust Act always exists; and everywhere we <u>find the convincing argument that most efficient firms cannot cut prices because this would force a lot of small and honest firms into bankruptcy</u>. It has to be noted that this argument (II) suggests that larger firms should *generally*[14] tend to maintain a certain degree of excess capacity, usable for 'bellicose' purposes.

III) I think that, in addition to these two important arguments, other ones could be developed, but I will not try to do so because they are in an intuitive state and are not sufficiently formalised. However, the above two arguments should be sufficient to suggest the utility of the model in the case of markets that are not large, which is probably already a good thing, since this case can easily and empirically found to be dominant.

[13] Italics in the original Italian letter.
[14] Italics in the original Italian letter.

(For the moment I have not tried to clearly establish how we could empirically verify the market size and the validity of the model; its size could perhaps be the ratio of sales with respect to the capacity of larger firms, and we could test the hypothesis that the number of technologies and the scales of production which coexist tend to increase when the market becomes smaller).[15]

These arguments are not in themselves sufficient to explain the progressive increase of concentration you discussed. We may note, however, the following points:

1) the increase in concentration for a single industry is not as pronounced and convincing as for industry as a whole;
2) the results for the industry as a whole may be *misleading*, reflecting changes in the structure of production rather than in the level of concentration of single industries;
3) There is no shortage of indirect arguments which may explain a rise in industrial concentration, for example (a) the market size may tend to diminish because the most efficient production scale has increased more rapidly than demand; (b) until the demand increase does not contradict condition (1) of p. 290,[16] the equilibrium price may remain constant and the entire demand increase may be absorbed by larger firms; this argument is reinforced if the production scale of the smaller technology tends to increase as well. (*This may be nonsense!*). These arguments would be quite convincing in respect to moderate changes of demand, but their value appears much more questionable when we consider the important changes that took place sometime between the beginning and the middle of the nineteenth century in the U.S. I leave to you the task of carrying on this debate between the pro-Sylos me and the anti-Sylos me, if you think it is worth it.

[15] Sylos Labini noted: “important”.
[16] Modigliani refers here to p. 7, in the original letter.

B) *Comments on part I*

I will now proceed with various minor, and more or less disconnected, comments on part I.

a) I have noted, among your quotations, the conspicuous absence of Alexander Henderson's article published in the *Quarterly Journal* of 1954, that I consider excellent and that I think I have already called to your attention.[17] His analysis is quite different since he makes specific reference to the case of differentiated oligopoly; and moreover, I consider it faulty exactly where your analysis is strongest, that is on the relevance of potential entry. However, the two analyses are mainly complementary and some of your conclusions have already been advanced by him, for example that of section 4 on pp. 74-75. Among other things, Henderson offers an explanation of the constancy of "*mark up margin over prime cost*" that is exactly based on the relationship between this *mark up* and demand elasticity. In his case, as you will see, reference to the demand elasticity is quite justified and Henderson's explanation of constant mark up is that when the demand changes during the economic cycle, its elasticity remains constant, at a first approximation. Thus, if the margin was initially optimal it remains optimal while the demand shifts. I underline this point to suggest that the relationship between elasticity and margin that you rejected as tautological and *meaningless* can be justified under different circumstances than those that apply to your model.

b) In chapter II you highlight the fact that we can have different prices which can be equilibrium prices, following your definition of the latter, and that each of these prices may correspond to a multitude of industrial structures that are achievable replacing a firm with technology *j* with an appropriate number of firms using an inferior technology (or vice versa). However, there comes the question if these different equilibrium

[17] Henderson was Modigliani's colleague at Carnegie Institute of Technology and collaborated with Modigliani at the research project on expectations. Modigliani and Herbert Simon published his article posthumously: Henderson A. (1954), "The Theory of Duopoly", *Quarterly Journal of Economics*, vol. 68 n. 4, pp. 565-584.

prices and industrial structures are all equally stable or if, instead, some are more stable than others. Situation A may be defined as more stable than B if, as a result of *random disturbances* or intentional wars, it is possible to move from B to A while the contrary is not possible, once A has been established. The problem is not without interest because, if certain positions are more stable in this sense, in the long run they will tend to prevail, at least if the market and technologies do not change too rapidly.

I have not studied the problem sufficiently to reach a definite conclusion; I believe, however, that it is demonstrable that between any two possible situations of equilibrium A and B, A is more stable than B if (and only if) the total profit of all participants is greater in A than in B.

This conclusion is suggested by certain results from game theory that I will not attempt to discuss; nonetheless, *it has intuitive appeal*. Thus, according to this criterion in your second numerical example the solution (2b) is more stable than (2a), which is more stable than (I'.2), which is more stable than (I'.1). For the same reason, there is an alternative to the solution I'.2 where there are 8 medium firms and 7 small firms, and this situation is more stable than I.2. An important consequence of this principle, if true, is that in more stable positions the number of firms adopting technology j must be inferior to the ratio $x_{j+1} / x_j = n_j$ because, if this would not be the case, n_j firms using technology j could be replaced by a firm using technology j+1, with a consequent reduction of total costs and since it implies a rise of total profit the new situation is more stable than the previous one. If it is true that the most stable situations tend to prevail this means that industries tend to have a very characteristic and easily ascertainable structure; that is to say, firms' full capacity of a certain technology should be less than the minimum capacity of the immediately superior technology scale. I suspect that an empirical investigation would show that this property is not generally valid; yet I would not be surprised if it would maintain a certain validity when the market is not large, that is to say in those situations where, for various reasons, your model should be valid. Do you think there is the possibility of doing an empirical test?

c) Your conclusions on the effects of technological changes on p. 73 and later are quite convincing within the limits in which your model is valid; however, I think they need some important clarification. A technical progress applicable to technology *j*, which is not the smallest nor the largest, may have a relevant impact on the equilibrium price, because, after the improvement, condition (1) of p. 290 may not be valid,[18] as it becomes convenient for a firm of size *j* to enter the market causing many (or even all) technologies of inferior scale to disappear. Protection of the extra profit margin of larger firms is, thus, considerably inferior than you show, because it may be shaken by technical progress at any level, and not only at the level of minimum and maximum technology as your argument seems to suggest.

It is also possible to notice that even small technological changes may *cause*[19] significant shocks to the industry, both in terms of structure and price. This conclusion, once more, is interesting and I think it may be empirically valid.

d) On p. 79 you return to the debate on whether the demand elasticity either increases or decreases during depression. Honestly, I think that it is a fruitless discussion as the only reasonable answer (apart from the dynamic argument indicating that a fall in prices creates expectations of further reductions – elastic expectations, that can easily be inverted, if expectations are inelastic, which is a priori equally possible) is that certain demands will tend to become more elastic, and others less. The demand elasticity depends especially on the elasticity of substitution and I do not think that we can say anything conclusive about its modification as the total income varies; maybe the most reasonable thing is that, on average, it should not change a lot, as suggested by Henderson. To your Schumpeterian example of cars – which incidentally is not nearly as convincing as it sounds, because the depression will certainly reduce demand, since the demand elasticity is strong in respect to income, but it is not clear whether the elasticity of the diminished

[18] Modigliani refers here to p. 7, in the original letter.
[19] Italics in the original Italian letter.

demand is lower – I can easily oppose the example of meat or coffee which at higher levels of income are consumed as 'necessities' with little attention to the price, but at lower levels of income become *luxuries* which strongly react to changes in price. However, I do not want to try to convince you, I only suggest that, if possible, you don't put too much emphasis on the elasticity reduction under depression and you avoid your sarcastic comments on Harrod, on p. 79; on the whole I believe that Harrod is more likely to be right than wrong. In the previous sentence you refer to empirical results; I do not understand what you are referring to and I would like to know more.[20]

e) The analysis on price rigidity during depression is quite convincing within the limits of validity of the model, especially when we consider that if the minimum price to enter p_j^m should fall more than the market price, the existence of excess capacity – and thus, also of low profits – would be sufficient to forestall entry. Hence, I completely agree with your conclusion that the main factor that determines the trend of prices during depression is the discipline of the group, which, again, will tend to be stronger when the market is small and firms are few. Nor will there be a rational incentive to reduce the price in order to increase sales across the entire industry because, without having to refer to any change of elasticity during the cycle, it is enough to observe that at your equilibrium price (that differs from the equilibrium price of pure monopoly) the elasticity of demand may be very low (it must be quite low for condition (1) on p. 290 to be valid).[21]

I believe that this deduction from your model may be important to explain Stigler's empirical results in his famous critical article of the *kinked demand curve* that, surprisingly, I do not see quoted (I do not have the reference with me but the article has been reprinted in one of the

[20] Sylos Labini noted: "Adams, Nelson, Keim and Mason". He was probably referring to the following works, cited in the subsequent editions of his book: Adams W. (1954), "The Steel Industry", in (id.), *The Structure of American Industry*, New York: Macmillan; Nelson S., Keim W.G. and Mason E.S. (1940), *Price Behavior and Business Policies*, Washington (DC): Temporary National Economic Committee, monograph n. 1.

[21] Modigliani refers here to p. 7, in the original letter.

American Economic Association volumes devoted to "*price*" and "*value*" *theory*).[22] I think it would be worth reading this article again, since it made a lot of noise, and could show you the relationship between Stigler's results and your model – if, as I feel, there exists a clear relationship.

f) In section 3 of chapter IV, especially on p. 94, and partly also in the last paragraph of section 5 on p. 36, I again find a confusion between <u>long and short run marginal cost</u>, a matter that I have often tried to clarify for you during some of our long discussions. I cannot enter into details, I will only remind you that even under the assumption of constant direct costs within the limits of the production capacity, these costs coincide with the <u>marginal cost only in the short run</u>, and again, only until the firm does not fully use its plant capacity. When we reach the limit of plant capacity the short run marginal cost <u>is essentially infinite</u>[23] – thus, if the price is higher than the direct cost, we can say that the marginal cost is equal to the price and both exceed the direct cost. Furthermore, the long run marginal cost cannot be equal to the direct one, because when full capacity has been reached the only way to increase production is to increase equipment and <u>thus, the marginal cost not only includes the direct cost, but also that of amortisation of equipment and of interest. It follows, in particular, that perfect competition is totally consistent with those techniques that lead to constant direct costs within the limit of plant capacity, as long as *diseconomies of scale* exist. In other words, over certain sizes the average cost, even using the equipment at full capacity, tends to increase with the increase of production</u> (and correspondingly with the rise of plant capacity). For the same reason, you have to convince yourself once and for all that the *welfare principle*, according to which price has to be equal to marginal cost, does not necessarily imply losses, even with constant direct costs, because the principle says that the price has to be equal to the marginal cost and not to the direct cost and, as I demonstrated, the marginal cost cannot be lower than the direct cost. It

[22] Sylos Labini noted: "I added a section, p. 115 new edition".
[23] Sylos Labini noted: "? But it is not so, *dC*/*dX* the traditional m. cost, is another thing: it is a jump, a limit".

can be higher, though, and it will be when the existing capacity has been fully used because, in this case, the marginal cost is just the total marginal cost and not the direct cost.

Losses may be expected only when there are economies of scale at the margin or, due to previous mistakes, there is spare capacity. I am not sure that I have made my point completely clear, but this is the best I can do for the moment. I should add, in your defence, that many who have discussed *monopoly power*, divergence from the optimum and so on make the same mistake as you, or at least don't appear to have clear ideas on the matter.

g) Your arguments on the tendency of large firms to maintain those factor prices that fall under direct costs is not completely clear or convincing because, if they really control the production of raw materials, we have once again situations of monopoly or oligopoly that have to be treated as such. I do not want to enter into details, but only raise doubts. At this point I want, however, to add a general observation: I feel that your work has some tendency to be acrimonious and polemical against large firms that you charge with all possible crimes (*except killing their grandmother* – as they say in English). Sometimes the same charges are contradictory. At a certain point, for example, you charge big firms with wreaking havoc on small ones after devoting large part of your analysis accusing them of the opposite, that is to say of keeping them under an umbrella! Your conclusions would often be more convincing *if your deep dislike did not show*![24]

And, with this, I think it is better to close my comments to part I, for me the most important, and to move to short comments to part II.

C) *Comments on part II (and, in part, III)*

a) Page 115, on monopolist behaviour. If the monopolist maximises profit, and the direct cost is constant, a fall in the direct cost necessarily

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[24] Sylos Labini noted: "different cases, considered without acrimony". Furthermore: "but, an ad hoc section, see pp. 172-173".

leads to a fall in price; furthermore, the price reduction may easily be larger than the reduction in cost and, as far as I know, might also be proportionally larger, but I am not sure of this latter outcome.

b) Your definition of price rigidity given on pages 116-117, which refers to the monetary level of prices, is completely unsatisfactory because price flexibility largely depends on monetary policy.[25] This confusion between real and monetary phenomena is the main fault of part II, as I will show later.[26]

c) The facts you referred to on pages 119-120, regarding the terms of trade between agriculture and manufacturing do not coincide with the long empirical study I carried out for my book *National Income and International Trade*.[27] Part of these results has been published in the book itself, but the main part is in an unpublished mimeograph report (to which I refer to in the book – see chapter 17). This work is partly based on statistics of English international trade and partly on more recent American and English data on international trade, and it shows that from the middle of the nineteenth century until 1929 there was a surprising long term stability in the trade relations between manufacturing and raw materials,[28] and the statistics probably underestimate the fall of manufacturing prices because they do not keep in mind the improvement in manufactured products. A similar study, based on American data and referring to the exchange rate between manufacturing and agriculture – I think from 1890 to 1920 – led to similar results and shows us that in that period productivity increased approximately by the same extent in both sectors (and, if I remember well, even more in extractive industries), although the comparison is complicated by the fact that there is no information on the trend of labour hours in agriculture. The information

[25] Sylos Labini noted: "I make the implicit assumption of the neutrality of money".
[26] Sylos Labini noted: "the entire chapter has now been radically changed: I think that these criticisms have been *met*".
[27] Neisser H. and Modigliani F. (1953), *National Income and International Trade*, Chicago: University of Illinois Press.
[28] In the margin Sylos Labini noted: "See footnote: even a constant ratio indicates the presence of oligopolistic markets".

only refers to the number of workers. It is quite difficult for us, living in our cities, to appreciate the huge technical progress there has been in agriculture, especially after the introduction of the tractor, at least here in America. Naturally, these results only refer to the long run trend; in the short run the exchange rate clearly tends to improve for industry in periods of depression and worsen during expansion phases. It is for this reason that my study, carried out soon after the war, came to a halt at 1929, in order to avoid being *biased* by the depression of the following decade.

d) I'm now coming to the last important point, criticism of chapter II, sections 3-9[29] that, as I said, I consider fundamentally erroneous (or at least unable to demonstrate what they are supposed to).

I will begin with some criticism on aspects of secondary importance that, however, have complicated my understanding of the model. My first objection is that your definition of "machine" is completely confused and inconsistent. In the table you show that the price of a machine is 10 from which it follows that in the initial position the machine sector produces 300; since there is no net capital accumulation we have to conclude that these 300 machines are used during the period itself and thus, that they do not differ from raw materials. But it gets even worse: in your text on page 132 you state that a machine lasts ten years and correspondingly in the table on p. 134 you assume that in order to keep the increased stock of capital in sector II only 30 units per period are necessary. There is clearly a serious contradiction that I find very *confusing*.[30] It is possible that this error may be easily solved assuming that the cost of a machine is 100 and that each sector initially owns 100 machines which last ten years, so that 10 is the amortisation cost of each machine. Thus, the machinery sector produces 30 machines every year that are necessary to replace 10% of the 300 machine stock owned by each sector. Clearly, this means that during the transition period sector II would buy 3 (and not 30) machines and the additional amortisation would be 30 per year as indicated in your

[29] Sylos Labini noted: "now completely rewritten".
[30] Sylos Labini noted: "in the new edition this is explicitly clarified".

subsequent columns. In this case we can observe that what happens in sector II may be explained only by an utterly huge technical change, because with an increase in the machine stock of only 3% it is possible to reduce employment by 3/7; this, in turn, implies that the new machines have to be different from the old ones and we may wonder why the substitution with new machines does not continue. I know that sometimes in order to solve a problem we create new ones. Therefore, I will stop here so that you may feel encouraged to make those revisions that will render your assumptions consistent.

A second minor objection refers to the transition period in the table on page 134. Here you make the quite absurd hypothesis that during the given period the same machines, while being produced, are already at work reducing employment. Moreover, the purchase of machines is considered a *current input* of sector II. This makes no sense if a machine lasts 10 years. It would be much more realistic and understandable for the reader if the employment and the production in sector II remained unchanged during the transition period in which the new machines are being built – that is to say we have a net investment of 300. The additional production of sector I – the investment of 300 – would then be balanced by the reduction output in the consumption sector – that is to say from the saving (since income remains unchanged) that also equals 300.[31]

However, this criticism is not so important - even though I think it is partly related to a crucial point – your confusion between monetary and real phenomena. I think that the better way to clarify the problem is to ask what your concept of "total monetary investment" precisely means and why it is of any interest to keep it constant. Your concept has nothing to do with the usual definition of investment, by which we mean the net (or even gross) addition to the stock of goods owned by society.[32] Your notion is rather close to that of 'transaction value' in the exchange equation, without however coinciding with it, because the value of goods

[31] Sylos Labini noted: "in the new edition these criticisms should be overcome".
[32] Sylos Labini noted: "That is what I say; it has to do with the Classical definition".

is calculated on the basis of direct costs rather than on market prices. It should now be clear that the monetary value of the transaction is a purely monetary phenomenon without any significance in terms of '*real economics*'; this value essentially depends on the quantity of money and on monetary habits – the set of forces that produces the "*V*" of the Fisher equation or the constant in the Cambridge equation. Without constructing complicated tables, it is perfectly clear that if the transaction's value is a constant – for instance because the quantity of money is fixed and there doesn't exist a monetary institution to regulate it – and prices are rigid, the community's real product is limited by the transaction's value and thus, by the quantity of money which determined the transaction's value. In fact, the real product is essentially determined by the ratio of transactions divided by the price index. If the real income so determined is less than the producible one, there will naturally be unemployment; this unemployment will be solved by a fall in all prices, but the problem may equally be solved – and in a much less painful way – with an adequate increase in the quantity of money. Using the same reasoning, it is easy to see that if productivity increases, prices are rigid and transaction value (the quantity of money) is fixed as well, there will necessarily be unemployment; but, as in the previous case, this unemployment essentially has monetary causes and not real ones, and is easily dealt with – that is to say by raising the quantity of money and thus the transaction value. It seems that you have a strong *bias* for a monetary policy that keeps the quantity of money fixed and forces prices to fall to maintain the equilibrium. Frankly I do not see a reason for this *bias*. My preference, for many reasons I will not attempt to discuss here, is for a monetary system that aims to keep the prices of goods constant, increasing the quantity of money as productivity rises. Such a policy is essentially neutral and it allows the rate of interest on money to reflect the real interest – that is the exchange ratio between the products of today and the products of tomorrow. A policy of falling prices leads to a money interest rate that is lower than the real one and serves to give *windfall* earnings to creditors at the expenses of borrowers – who usually are the entrepreneurs – thus discouraging initiative. But this certainly isn't the place to enter into a detailed discussion on monetary policy, I only want to establish that 1) it doesn't make sense to concentrate on maintaining an unchanged value of transactions; 2) if we do so, the conclusion that with rigid prices technical progress necessarily leads to unemployment is completely trivial, and tautological. What you should have established is that technical progress, or the introduction of machines, in the hypothesis of the kind of market forms you discuss, necessarily or probably leads to unemployment, for real reasons, independently of the monetary policy that could be followed, and thus, without resting on the premise that the transaction value has to be constant. Now, I believe that on this point your analysis on pages 129-143 has absolutely nothing to offer – and cannot have anything to offer because of the nature of the model you chose that does not even have a *production function*.[33]

You can easily realise that once we leave out the hypothesis of constant transaction value, it is very easy to construct from the table on page 134 a situation of full employment equilibrium completely similar to the one you laboriously obtained in the last column of the table on page 137. You only need, for example, to take column (I-1) from the table on page 134 and to multiply each number of this column by the quantity 2100/1800 = 7/6 which is the ratio between the employment level you obtained and the total labour force. The result is a situation of equilibrium in all sectors and total employment equal to the labour force, that is 2100. In particular the consumption value – the real consumption – will be 346.6. This number is lower than the one you obtained in the last column of the table on page 137, but only because in that table you assume an employment of 2193 rather than 2100. If you want to assume this larger employment you only have to multiply the number on page 134 by 2193/1800 and thus you will find another situation of equilibrium with a real consumption equal to that on page 137. (The number you refer to is 362.6, but following my calculations, you must have made a small calculation error and the correct number should be 361.6). Your solution on page 137 and the one I proposed, even if with the same employment

[33] Sylos Labini noted: "yes: coefficient *X*". Additionally, at the end of the page he added: "$P_m = (ma + mp + l)$".

level, are different in two respects: 1) the total value of transactions is different, but as I said this purely monetary difference is trivial; 2) the income distribution between profits and wages (and between the profits of various firms) is considerably different; profits are naturally lower in your solution of "competition" in which the price tends to cost, than in my solution in which technical progress entirely goes to increase profits, while real wages (per capital and total) remain unchanged. Thus, I have established once again the conclusion that I have often explained to you in Rome, that monopoly and oligopoly have an effect on income distribution but not on the employment level unless 1) there are purely monetary problems which can be easily solved through changes in the quantity of money, or 2) real (not money) wages are rigid.

From this fundamental error (at least in my view) in your analysis of chapter II,[34] all subsequent analysis, which is largely based on the results of chapter II, is deeply weakened and frankly I have not even tried to follow it carefully enough to see if some parts of it could be saved. I have, however, strong reasons to doubt it. In particular, I want to underline that the only argument that, it seems, could be defended, is that an increase in profits can create problems of a Keynesian type on effective demand, is subject to two serious limitations: 1) on the basis of my recent work on savings I have many reasons to doubt that an income redistribution has an important effect on the total value of savings; 2) but, even if it is true that savings increase, this can be a problem only in countries with large amounts of capital and saving and not in countries, such as Italy, where there is a great scarcity of capital and where unemployment is, at least partly, the consequence of this scarcity together with the real wage rigidity which, being already very low, cannot easily be further compressed. However, with regard to the problem of effective demand and unemployment, I would take the liberty of referring to part IV of my theory on money, interest etc., that I sent you by ordinary mail two days ago (see below).[35] In these notes I discuss the effect of rigid

[34] Sylos Labini noted: "now completely rewritten".
[35] Modigliani F. (1955), "The Theory of Money and Interest in the Framework of General Equilibrium Analysis, part IV: Dynamic with Rigid Prices", *mimeo*, Pittsburg: Carnegie

prices as well as wages, with some reference to types of imperfect competition, and I discuss various possible causes of unemployment equilibrium.

And with that I think it is more than time to put an end to these comments that have become excessively long. Here and there I found some printing errors, but I did not note them down as they were very few and of no importance.

I wonder if my comments, which arrive when your work is essentially already finished, can be of some use. I hope so and, in any case, I flatter myself that at least some of these comments, even if they cannot be used in the final revision, could be useful to carrying on your investigation. Naturally I will be very interested in your reaction to my comments and I would like to know if they are of any use to you. I must add that my plan to carry on the systematic analysis of the effect of market forms on employment, along the lines laid out in Rome, still exists but I need to continuously postpone it due to other prior commitments. We will come back to it when the time is right.

As mentioned above and in previous correspondence, I sent you a copy of my notes. It is a set of notes which cover about ten of my lectures of the last year, serving as an introduction for [a] sort of general treatise on the theory of money and interest, partly written before the classes and partly based on the course itself, which constitutes the beginning of what I hope will be a book on the subject. At the moment only the first four parts are finished (and they have been sent with the rest). The subsequent parts, which are partly in the form of notes not yet transcribed and partly to be written, will tackle the same problem under conditions of uncertainty, which involves problems such as that of the relationship between rates of interest of bonds of different duration etc. I think it is still too early to think of translating and publishing these notes in Italian; even if only as notes. However, I would be grateful if you had the time to read them and let me know your reactions. It is my intention to prepare at

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least a *rough draft* within the year and especially during the second semester when I will be teaching the new course.

I can only conclude this very long missive with my warmest wishes of success for your competition. Don't forget to keep me informed – and if the final version of your book contains important revisions I expect you will send me another copy. (I also think that you should seriously consider preparing an article for one of the American journals presenting the essential elements of your model of concentrated oligopoly).

With warm regards, from Serena and our sons too.

Franco