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# John Maurice Clark's Contribution to the Genesis of the Multiplier Analysis

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**Abstract** - John Maurice Clark is commonly recognized as one of the most influential figures in US interwar economic thinking. This note focuses on a specific aspect of Clark's work which epitomizes his attempt to combine the rigor of traditional economic analysis with his "institutionalist" attitude, namely his contribution to the multiplier principle. First we will discuss Clark's independent discovery of the multiplier principle — by which it is meant the familiar notion that an initial increase (or decrease) in expenditures has a multiple effect on total expenditures — also providing in the appendix some unpublished evidence which seems to confirm that Clark arrived at it before reading Kahn's celebrated 1931 article, "The Relation of Home Investment to Unemployment." Then, we will dwell into some more analytical aspects of Clark's treatment of the multiplier, focusing, in particular, on his attempt to reconcile the traditional "Kahn-Keynes" approach with the so called "velocity of circulation" approach.

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> "Scientific theories are like children in that they have a life of their own. But, unlike children, they may have more than one father." [Samuelson, 1959, 183].

1. For the historian of economic thought John Maurice Clark stands as an intriguing intellectual puzzle. On the one hand, he was among the most active promoters of American institutionalism during the interwar years. Together with Walton H. Hamilton, Clark participated to the 1918 AEA conference which marked the formal public launching of the movement in the United States under that title. On that occasion he presented the paper "Economic Theory in an Era of Social Readjustment," calling for an economic science both "actively relevant to the issues of its time" and able to furnish the student "with tools of thought rather than with the finished product; with knowledge of the general features of the institutions he is studying; and with principles of a widely varying sort, embodying many ways in which business affects human desires, directly and indirectly." [Clark 1919]. On the other hand, John Maurice Clark never fully rejected John Bates Clark's theoretical contribution. He denied any discontinuity between his father's two major works, *The Philosophy of Wealth* and *The Distribution of Wealth*[1], and saw himself as attempting to continue his efforts to develop a dynamic theory[2]. As he once put it in a letter to Professor Roche-Agussoll in 1918, he was aiming at developing a "dynamic social or institutional economics or realistic economics." This to him was the third division of the field of economics. The first two, which comprised "value economics," were "1) Static value —or price— economics; 2) dynamic value —or price— economics"[3].

This ambivalent nature of John Maurice Clark's thought has been remarked by many commentators. Joseph Dorfman, for instance, speaks of Clark's economics in terms of a "constructive synthesis of tradition and new dynamics" [Dorfman 1946-1959, 438] while, in a more critical vein, Baldwin Ramson describes it as a "middle way," which "does not accept orthodox theory as adequate, but neither does it accept the need for a new theoretical domain." [Ramson 1977, 467]. This note focuses on a specific aspect of Clark's work which epitomizes his attempt to combine the rigor of traditional economic analysis with his "institutionalist" attitude, namely his contribution to the multiplier principle. First we will discuss Clark's independent discovery of the multiplier principle — by which it is meant the familiar notion that an initial increase (or decrease) in expenditures has a multiple effect on total expenditures — also providing in the appendix some unpublished evidence which seems to confirm that Clark arrived at it before reading Kahn's celebrated 1931 article, "The Relation of Home Investment to Unemployment." Then, we will dwell into some more analytical aspects of Clark's treatment of the multiplier, focusing, in particular, on his attempt to reconcile the traditional "Kahn-Keynes" approach with the so called "velocity of circulation" approach.

2. While John Maurice Clark is commonly recognized as the father, together with Aftalion, of the so called "accelerator principle" [Junankar 1987] and as an anticipator of the "multiplier-accelerator" model [Shackle 1967, 264-265], his contribution to the development of the multiplier analysis is often neglected. Hints at Clark's studies on the multiplier analysis are to be found in Joseph Dorfman's encyclopedic enterprise, *The Economic Mind in American Civilization*, and in two more recent works by Laurence Shute [1994; 1997]. Both authors insist on the fact that Clark arrived at the idea of the multiplier being unaware of Kahn's and

[1] See Clark's remarks on his father's contribution to economic theory in the letter to G. S. Hauge reproduced in the appendix.
[2] See Morgan-Rutherford [1998. 3].
[3] J. M. Clark to Roche-Agussol: Sept. 14, 1918, Jospeh Dorfman Papers, Rare Book and Manuscript Library, Columbia University. The letter is reproduced in the appendix.

Keynes' formulations of the principle. In Dorfman's own words: "Clark was one of several students who, working independently at about the same time, began a quantitative formulation of this idea [the multiplier]. While the exact formal statement of the principle became associated […] with R. F. Kahn and J. M. Keynes, Clark, in typical fashion felt that the multiplier was useful chiefly as a model." [Dorfman 1946-1959, V 762; see also Dorfman 1970].

A more detailed and insightful assessment of the role played by Clark in the development of the multiplier is provided by Ronnie Davis in his *The New Economics and The Old Economists*. Davis deals with Clark's treatment of the expansionary effects of public spending in order to support his claim that most American economists favored countercyclical fiscal policy before the publication of Keynes' *General Theory*, and that they based this advice on an understanding of income-expenditure theory. Quite curiously, the lengthy discussion by Davis neglects Clark's monograph on public works [Clark 1935b], just concentrating on a single paragraph in Clark's *Strategic Factors in Business Cycles* [Clark 1935c] on how reduction in savings can result in a dwindling series of contractions in income and spending which sum to a finite value and on the accompanying footnote stating that Clark wrote the paragraph before reading Kahn [1933]. Like Dorfman and Shute, Davis observes: "It is interesting to note that Clark's recognition and use of the multiplier principle was anterior to his seeing Kahn's publication of "Public Works and Inflation'." [Davis 1971, 66].

Davis' contention about American economists as anticipators of Keynesian deficit-financed fiscal expansion has been challenged by Robert W. Dimand. In this connection, Dimand argues that even the only two American economists with a grasp of the multiplier theory, Paul Douglas of Chicago and John Maurice Clark of Columbia, had explicitly derived their analysis from articles by Richard Kahn and J. M. Keynes, with Clark explicitly naming the multiplier "the Kahn-Keynes approach." Dimand phrases his critique of Davis as follows:

> "Davis does not mention that Clark devoted a paper to 'The Cumulative Effects of Changes in Aggregate Spending' as Illustrated by Public Works in the *American Economic Review* [1935a], reprinted under a shortened title in his *Preface to Social Economics* [1936], expanded as Chapter IX of his report on *Economics of Planning Public Works* [1935b], and presented to the American Economic Association in December 1934. Clark's presentation of the multiplier theory in his paper is striking both for its clarity and for its name: he called it 'the Kahn-Keynes approach.' Clark attributed the multiplier theory to Keynes' pamphlet *The Means to Prosperity* [1933] and to two papers by Richard Kahn [1931, 1933], and concluded that it had only limited validity, as just one of the forces affecting the velocity of circulation of money" [Dimand 1990, 45; see also Dimand 1988].

Evidence from our archival research seems to confute Dimand's critique and confirm the thesis of Clark's independent discovery of the multiplier principle. Clark's correspondence is particularly illuminating on this regard. Clark insisted on the fact that his recognition and use of the multiplier principle preceded his reading of Kahn [1933] both in a correspondence with Paul Samuelson dated 1953 (the exchange is reproduced in the appendix) and in a long letter sent two years later to his friend and colleague at Columbia, Joseph Dorfman. In both letters, however, Clark generously admitted that is Kahn the one who deserves priority:

> "When I worked out my asymptotic form of the multiplier, published in 'Economics of Planning Public Works,' I noted that it furnished another kind of mechanism whereby the increase in total income resulting from an increase in investment might tend to taper off […]. *I dreamed up this asymptotic multiplier in 1930, when R. F. Kahn was well along with his essentially similar concept, so he has priority*."[4]

Interestingly, in the same letter Clark also denied any influence from Nicholas Johannsen's early writings on the "multiplying principle:" "Johannsen's work I don't think I ever grappled with; I recall only vague &

[4] Clark to Dorfman, May 12, 1956. Dorman Papers, Rare Book and Manuscript Library, Columbia University.

general ideas about it — so when about 1924 or 1925, he wrote asking me for some public recognition of work, I was embarrassed, and I didn't feel competent to appraise it."[5]

Finally, it is worth pointing out that Clark's first formulation of the multiplier principle — although less rigorous in its mathematical formulation — did not appear in Clark [1935a] but in *The Costs of the World War to the American People* published in 1931. In that volume, discussing the United States productivity in 1916, Clark made clear the view that the volume of production may vary more widely than economists had generally conceived, as a result of changes in aggregate demand. From the letter he wrote to Dorfman we learn that the book was recognized by Nicolas Kaldor as an anticipation of Keynes' ideas: "As to multipliers, in my 'Cost of the World War' discussing the neutrality-boom of 1916, I evolved a rough foreign- trade multiplier. Kaldor found some bits of that book to be anticipations of Keynesism."[6]

3. Let us now turn our attention to more specific aspects of Clark's treatment of the multiplier. According to the *Palgrave Dictionary* the theory of the multiplier "in its pure (or static) form" can be described as follows:

> "In a capitalistic economy investment can always be realized in real terms. The necessary saving will be made available by means of corresponding variations of the level of income, given the propensity to save. With generally underutilized capacity and labour and fixed prices — the most common hypothesis — *real* income will take whatever value generates a flaw of saving equal to planned investment. Alternatively, in the presence of supply constraints, the level of prices will adjust and deflate consumption expenditure so as to make available the real resources required for investment." [Medio 1987, 565].

Although the whole argument is familiar to modern readers, there are certain features which deserve notice:

a) First of all, it is worth emphasizing that at the time Clark first formulated his version of the multiplier theory, he was chiefly concerned, not with the framing of a general theory *à la* Keynes, but rather with a realistic analysis of the upswing of a business cycle. As he explicitly admitted in his later recollections: "[m]y own first consideration of this theory [the multiplier] was as an explanation of the expansion phase of a normal business cycle […]." [Clark 1939, 201]. As a consequence of this, the American economist did not follow Kahn in assuming a given level of wage rates and perfect supply-elasticity, and spoke of the successive spendings as inflationary throughout his analysis[7]. Also Clark's treatment of "leakages" shows a fundamental difference from that of Kahn and Keynes [1933]. To our knowledge, Clark was among the first writers who accentuated that savings, which are regarded as "leakages," do not remain idle. According to Clark, the term leakage refers to "a sum of funds which does not increase production beyond what has been accounted for in the formula." [1935b, 89]. Clearly, this definition does not imply idleness of funds. For Clark, the leakages which are responsible for the failure of the multiplier to go on expanding definitely themselves represent deflationary uses of income generated from the public spendings, and these tend to offset the inflationary effect of the public borrowings: "They [the leakages] may not all come directly into the banks as loanable funds, but if they do not do that, they strengthen someone's credit position and thereby are likely to reduce the need of borrowing from the banks in the future." [Clark 1939, 202]. Starting from these premises, Clark was therefore justified in opposing the *inflationary* effect of public borrowing to the *deflationary* effect of leakages. This point will be taken up below.

b) Clark [1935a; 1935b] individuated two possible approaches to the multiplier analysis: "one *via* successive cycles of income and spending by ultimate recipients of income" — which he termed the "Kahn-Keynes" approach —, "the other via the volume of money and its velocity of circulation." As to the latter approach, Clark [1935a, 383] affirmed that "it has, so far as I am aware, not found its way into print." A clear and early statement of the view held by the exponents of the "velocity" approach is provided by a letter that Alvin

[5] Clark to Dorfman. Johannsen not only coined the expression "multiplying principle," but also was the first economist who attempted to calculate numerically the secondary effects of an initial reduction of total expenditures out of a given income. See Hagemann-Rühl [1990].
[6] Clark to Dorfman, May 12, 1956. It should be noted that Dimand [1990, 45] acknowledges "the less formal but still suggestive multiplier analysis" of Clark [1931], but doesn't attach great relevance to it.
[7] See Hegeland [1966].

Hansen sent to Clark in 1934 (the letter is reproduced in the appendix). Clark had asked Hansen for some comments on an early draft of chapter nine of his *Economics of Planning Public Works*. In his reply the Harvard economist dismissed Clark's reasoning on the ground that it "still follows too much along the Keynes lines" which, at that time, he considered "definitely wrong."[8] For Hansen the explanation of how an initial change in the rate of spending will induce further increments in the level of total income was essentially a question of (1) the volume of means of payment and (2) the income velocity of money:

> "Public works may affect either one of these two or both. It is quite correct, as Keynes said, that the public works expenditures may not enlarge the volume of "money." In this event, the effect presumably is to transfer money from idle hands to active hands. In other words, the income velocity increases. Or there may actually be injected into the market new 'money,' and the income velocity might conceivably remain on the average as before. Or there may be a combination of these two tendencies."

Also the Keynesian concept of "leakages," i.e. those portions of a given stream of spending which are not respent within the period considered, become devoid of any specific content once the "circulation" approach is adopted:

> "Keynes' 'leakages,' I think, are also dangerous. The most important 'leakage' is his saving 'leakage' and this when analyzed amounts to nothing more or less than our old friend, a change in income velocity. If all of us hold idle a half of our income which we formerly spent, — save it without investing it, in other words — the income velocity has been cut in two. Thus the total income of society in the next succeeding period has been reduced to one-half by this process of 'savings running to waste'."

As a consequence of this, there will be no secondary effects unless the primary expenditure results in an increase in the quantity of money, its velocity, or — as it seems more probable —both:

> "All that public expenditures do is to throw new funds into the market, and thereby increase the income, which action, since it is certainly not likely to reduce the income velocity of "money" is very likely to increase the total "money" income of society by more than the amount of "money" injected. It is, therefore, correct to say that public expenditures are likely to have an effect on income in excess of the expenditures. This is all there is to it, it seems to me."[9]

Clark did not share Hansen's criticisms and viewed a possible merging of the two competing approaches. In order to better understand Clark's argument, two concepts should be introduced. The first one is the concept of *income propagation period*, by which it is usually understood the time lag between successive waves of expenditure of the additional net income deriving from the primary investment expenditure[10]. Clark estimated this period at two months, which corresponds to 6 cycles of secondary effects per year, in consideration of the large amount that wage payments normally constitute of the deficit spending[11]. The second concept is the more familiar "circuit" (or "income") velocity of money, defined by Clark as "the ratio between the total amount of circulating media in the country and the total net volume of production, or the total national income, which those media of exchange serve to finance." [Clark 1935b, 96]. Drawing upon Angell's [1933] monetary studies, Clark estimated an average rate of circuit velocity of money of 1.6 per year which, in turn, corresponds to an average cycle of 7½ months for money to flow from a consumer through all the exchanges involved in producing the goods he buys and to get back to an ultimate consumer again[12]. This velocity, however, does not remain constant at 1.6 per year. Clark provided convincing

[8] On Hansen's early appraisal of Keynes' work see Asso .[1990, 62-70].

[9] Hansen to Clark, August 8, 1934. Dorfman Papers, Rare Book and Manuscript Library, Columbia University.

[10] The term was introduced by Machlup [1939].

[11] "As to the period of circulation, it may be assumed that wages are spent, for the most part, within a week, and begin to result in increased wage payments to other workers in a fairly short period. Interest and dividend payments may be received on the average, about two months after they are earned, but are slower in being spent. On this basis it would seem that an average cycle of two months would be a conservative estimate." [Clark 1935b, 87; emphasis added].

[12] As Hansen remarked in his letter to Clark, this figure includes time deposits as part of circulating money but, if they are excluded, the rate of circulation is increased to 3 per year: "I am exceedingly skeptical of Angell's 1.6 income

evidence showing that it varies in harmony with the business cycle, increasing with industrial revival and falling with industrial depression. Throughout his discussion Clark warned the reader that the "income propagation period" and the circuit velocity of circulation should not be confused. As he put it "the 6 cycles per year do not represent the velocity of circulation of anything, but rather the speed with which an increased velocity is transmitted through the economic system." [Clark 1935b, 88].[13]

c) In assuming an income propagation period of two months, whereas the average circuit velocity of money, reckoned at 1.6 per year, implies a period of 7½ months, Clark was implicitly assuming "that the volume of business increases faster than could be accounted for by the existing volume of purchasing media and their existing circuit velocity". Therefore, he argued, in order to finance a permanent increase in investment expenditure, either the circuit velocity of money must increase or business must acquire more circulating media through an expansion of credit, or both. Part of the task could be accomplished by the increased flow of savings injected into the system through the multiplier and which "may take effect in increased credit used by business in financing this expansion without bringing any increase in the volume of business beyond what the formula already calls for." But even if all the leakages were taken and "used" by business, a problem would still emerge. In fact, "[d]uring approximately the first year and a half […] the total amount of otherwise idle funds resulting from the leakages would not be sufficient to finance the indicated expansion of business at the existing rate of the circuit velocity of funds, and so an increase in the circuit velocity will be called for […]." [Clark 1935b, 95][14]. On this basis, Clark observed that the conditions described by Kahn and Keynes would themselves furnish a sufficient cause of increased circuit velocity of money, "simply because they cause an increased volume of business without a proportionate increase in volume of means of payment." [Clark 1935b, 101].

For Clark, also the analysis of the agents' behavior shows that the nature of the process can be seen to tend towards increasing circuit velocity. The actual mechanism whereby income changes in response to investment is explicated in terms of inventory accumulation or decumulation. As a consequence of the increase in effective demand caused by the new investments, sales will exceed production and producers experience an unplanned fall in inventory levels. They react by employing more workers and producing more: All of these activities — he writes — call for payments:

> "If business has sufficient funds to make these payments it will joyfully make them. In a depression, the ratio of cash balances to volume of business becomes larger than normal […]. These balances are kept, not employed to normal capacity, because nothing better offers. When something does offer, they will be promptly used and an increase in rapidity of circulation will naturally result. If a particular business does not have sufficient funds, it will try to borrow them, and in that case will call into active use some of the otherwise idle funds which have been designated as leakages in the Keynes approach to the problem. Or if a business has sufficient funds to make the payments, but not sufficient to make them without drawing down its balances lower than seems desirable, then, again, it will try to borrow, but even if it fails it will not cease to do the increased volume of business […]. Most of the funds will circulate within the business community, using them faster will not exhaust them, and business as a whole will always have funds available to make payments" [Clark 1935b, 101].

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velocity, Pigou arrives at the figure of 3.0, which seems to me about right in normal times. I do not see how it could possibly be as low as 1.6 unless one assumes, as Angell may do, that savings deposits are part of the means of payment. This view I should challenge."

[13] Clark illustrated the difference between the two concepts by a physical analogy: "suppose the production of a commodity divided into 15 stages, each requiring 3 days, while 12 days' reserve supply of materials or intermediate products is on hand at each stage in addition to goods in process. Then there will be 7½ months' supply on reserve or in process in the whole system, and the average series velocity will be 1.6 per year. But it would take only 45 days for a unit of material to move through the series if it were not kept waiting in the reserve stocks. And, what is to the present purpose, if there were a sudden increase of demand for the final product, (and if there were spare plant capacity at each stage) the speeding out of output could be transmitted back from stage to stage through the whole series, not in 7½ months but very possibly in 7½ days; indeed there is no physical impossibility in its happening in 7.½ hours." [Clark 1935b, 88].

[14] Note that this is true under Clark's assumption discussed above.

In summation, Clark argued that an increase in circuit velocity is required both to provide funds to finance an increase in investment, and to meet the higher transactions demand resulting from the consequent increase in economic activity. Furthermore, he continued, this leads to an interesting conclusion: "that there is no absolute necessity at any stage of the process for government outlays to take the form of an increase in the supply of purchasing media. If the attitude here taken toward changes in velocity is correct—and of that the writer has no doubt—this necessity disappears." [Clark 1935b, 102].

Clark, then, concluded the whole discussion affirming that since the velocity approach implies a multiplier value equal to unity (no "leakages" being involved in that case), the two methods should give similar results only if they are applied to a short length of time and if proper assumptions are made — among which, as we will see below, he included a decreasing rate of leakages in the Kahn-Keynes approach.

d) Clark's attempt to find a merging between the two approaches to the multiplier was welcomed with "mixed feelings." To our knowledge the first reference to Clark's discussion of the multiplier is to be found, as early as in 1936, in a note by H. Neisser commenting on Kahn [1931]. In a brief and rather vague passage, the German economist informed the reader that "in a paper […] published after the finishing of this article, J. M. Clark distinguishes the 'Kahn-Keynes approach' and the approach via money and its circuit velocity.'" According to Neisser, the latter approach "is identical with the method chosen in the text above and already developed in the year 1931 by various German authors, Lautenbach, Colm, Marschak, and the present writer. The discussion overlooked, however, that the scheme suggested would work only if the crisis and depressions were of a specific type." [Neisser 1936, 26].

From the institutionalist camp, Sumner H. Slichter, who reviewed *Economics of Planning Public Works* for the *American Economic Review* in the following year, found it to be "the best book available on the economics of public works." The chapter on the cumulative effects of public expenditures — he continued — "contains an excellent analysis and comparison of the Kahn-Keynes successive-spendings approach and the approach by volume and velocity of circulating medium" In particular, Slichter agreed with Clark's treatment of "leakages" as funds which play an active role in the multiplier process:

> "Incidentally, the expression 'leakage' is in a way unfortunate, because it suggests that incomes that are not employed immediately to increase production do no good. As a matter of fact the expenditures represented by 'leakages' may be the most useful of all. To a considerable extent they go to build up cash reserves or to reduce indebtedness. In the midst of depression many enterprises and individuals will not materially increase their purchase of commodities until they have built up their holdings of cash or reduced their debts." [Slichter 1937, 187].

A different opinion from Slitcher's is the one presented by Paul Samuelson four years later. In a lengthy contribution appeared on the *Quarterly Journal of Economics*, the Harvard economist polemically referred to those writers who have attempted a reconciliation of the multiplier and the velocity approach. "Unfortunately" — he wrote — "their reconciliations have been trivial, as one-sided as an agreement between a totalitarian state and one of its puppet creations, or else have been founded upon error." [Samuelson 1941, 602]. However, in dealing with Clark's contribution, Samuelson phrased his critique in a rather conciliatory vein:

> "In his path-breaking *Economics of Public Works*, J. M. Clark, almost as an afterthought attempted such a reconciliation. By refusing to push the analysis beyond a year's time, and by taking advantage of the fact that a convexly growing time shape of income can in a finite range be approximated by a straight line, the two hypotheses are given a semblance of compatibility, which will, I fear, please neither camp. *If the analysis had been continued in time, Professor Clark would have found it necessary to introduce a sudden, arbitrary, unexplained and unexplainable collapse in velocity, in order to maintain consistence with the multiplier time sequence*." [Samuelson, 603; emphasis added].

The point raised by Samuelson is pertinent and appropriate. However, a textual scrutiny of chapter nine of Clark's *Economics of Planning Public Works* reveals that the Columbia economist had realized himself that if the analysis of the multiplier effects had been continued over time (and assuming a constant marginal propensity to consume), "the conditions would be reversed," that is, that there would be an excess of savings

which would call for a sudden decrease in the velocity of circulation. Nevertheless, Clark pointed out, this would mean to carry the multiplier analysis over its proper limits of time, because "by the time this happened, the total volume of business expansion would have passed under the control of factors not accounted for in these mathematical formulas." [Clark 1935b, 95].

Still in 1941, Henry H. Villard dedicated a whole chapter of his *Deficit Spending and the National Income* [Villard 1941] to an appraisal of Clark [1935b]. Specifically, Villard turned his attention on Clark's distinction between the rate of transmission of an impulse through the system — what we termed the income propagation period — and the circuit velocity of money. "There are two ways" — he argued — "in which output could increase in less than 7½ months in the example given by Professor Clark: either the circulation of the existing output can increase or the new output can have a circulation more rapid than 7½ months." Since Clark seems to assume that the new investment will bring no change in the circuit velocity of the existing purchasing media, Villard continues, then it is not clear why the new money should pass through the system at a more rapid rate than similar existing money:

> "[W]e do not know definitely whether the different rate will be faster or slower, and in any case there seems to be no reason for believing that the income that is generated by the *subsequent* spending of the primary government expenditure will be generated any more rapidly than income generated by the spending of the similar portion of existing incomes" [Villard 1941, 152: emphasis in original].

Therefore, he argued, "it seems correct to conclude that it is V and no the 'rate of transmission of an impulse' that is important unless there is a change in the V of the existing M." [Villard 1941, 152].

A more recent assessment of Clark's treatment of the multiplier is provided by Bruno Hegeland. According to Hegeland "Clark's analysis appears to be the first .comparative study between these two approaches [the multiplier and the rate of circulation of money], but he did not succeed in convincing the reader about the possibility of such a convergence." [Hegeland 1966, 45-46]. More specifically, Hegeland observes, albeit without providing any evidence to support his claim, that in his treatment of the multiplier Clark does not seem to have recognized the so called Kahn-Meade relation, namely, the equality between the initial spending and the ensuing amount of leakages. Our opinion differs from Hegeland's. In fact, as it appears from the above discussion, Clark's concern is not with a theory of equilibrium in terms of comparative statics. The novelty of Clark's contribution lies in its pointing out that, even if the *ex post equality* of the new streams of expenditure and the new streams of saving can be demonstrated, their distribution over time differs substantially, with savings being insufficient, at the outset, to finance the higher level of investment in each period. This, in turn, would necessarily imply an increase in the circuit velocity of money, provided that "there is no literal inflation of the amount of money sufficient to handle the increase in business without an increase in velocity." [Clark 1935a, 388].

This is not the place to quarrel with the empirical validity of Clark's assumption about the behavior of velocity of circulation of money, debatable as it may be. What is relevant here is that to Clark the discussion of the so called "velocity approach" mainly served to arrive at an estimate of the time dimension of the multiplier effects, an aspect which, as he stated in his 1934 address, "has not been worked out, as far as I am aware." [Clark 1935a, 385]. After the publication of the *General Theory*, several economists, including its author, followed Clark in exploring in greater detail the dynamic implications of the multiplier, especially in relation with the so-called "finance motive." In 1939, just to cite one of the most significant examples, Nicholas Kaldor will further develop Clark's argument — albeit without mentioning the name of the American economist — showing that, if the multiplier is not instantaneous, the Kahn-Meade relation does not hold and arguing that a low propensity to save increases the size of the additional funds needed to finance a permanent increase in investment.

e) All this leads us to our final point, namely Clark's awareness of the intrinsic limitations of the multiplier analysis. Clark was in fact very skeptical towards any attempt to compute the exact numerical value of the multiplier for "[b]eside the elements acting on the factors in the formula, other outside elements are important, chiefly bearing on the effect of such a program on private business." [Clark 1935a, 387]. Among these "outside elements," Clark mentioned 1) the possible "crowding out" of private investments by public spending programs, maintaining that "when one considers the condition that the funds borrowed by

Government must be funds which would not otherwise be used by private industry, it becomes rater irrelevant to project the estimate of future stimulative effects beyond, let us say, the next industrial revival, which may come in two or three years;" [Clark 1935b, 84] [15] 2) the impairment of business confidence by "unlimited deficit financing;" [Clark 1935b, 86] and 3) the possibility of a fall in private capital investment due to the expectation that growth of demand will cease after deficit spending disappears[16]. To these it should be also added 4) Clark's skepticism upon the idea of a constant marginal propensity to consume. To Clark, this idea was based on the rather unjustified assumption that the same propensity to consume can be presumed for each subsequent stage of income generation. "Actually," he wrote, "there is […] the probability that the percentage of leakage will decrease as industrial conditions grow better, as individuals have less urgent need to get out of pressing debt, and as those with available savings find more opportunity to invest them in such ways that they will actually be spent for productive equipment." [Clark 1935b, 89]. For these reasons, Clark concluded, "estimates of stimulative effects, based on such an approach as the Kahn-Keynes formula, are hardly worth carrying beyond, let us say, one year, even as rough approximations." [Clark 1935a, 387].

4. In conclusion, Clark's treatment of the multiplier emblematically represents the tension in his thought between the search for a theoretical system which would complement his father's work, and the "institutionalist" awareness of the limitations that any mechanical formula necessarily imply. It was his eagerness to arrive at a "dynamic economics" which led Clark to the discovery of the multiplier, a theory which, as he stated in 1939, contains a "significant truth," although "some formulations need considerable qualification." [Clark 1939, 200]. Certainly, Clark's discussion of the "income-flow" analysis never reached the originality and completeness of Keynes' *General Theory* and this in part may explain why, in the late thirties, many young institutionalists found in keynesianism a more valid and fascinating alternative to orthodox economics. We may also add that, as remarked by Villard as early as in 1941, Clark's discussion of the multiplier presented a fundamental shortcoming: namely, it did not explain adequately how an increase in circuit velocity of money is transmitted throughout the system once a primary investment has taken place. Nevertheless, Clark's contribution to the multiplier deserves our attention. Not only, in fact, Clark arrived at the formulation of the principle independently from Kahn and Keynes — as the archival evidence presented in the appendix seem to confirm — but he was among the first writers who explicitly pointed out that the secondary effects do not do not occur simultaneously and to attempt an analysis of the "time dimension" of the multiplier. As he wrote in a later reappraisal of his 1935 monograph: "The reader may note that my assumptions as to time are different from those which appear to underlie Keynes's form of this theory. *The kind of adjustment I have in mind does not appear to be one that can take place instantaneously*." [Clark 1941a, 47: emphasis added]. On the other hand, as we have pointed out, Clark considered the multiplier just as a "rough approximation;" a formula which would work only under ideal conditions. In more than one occasion he did not hesitate to warn his colleagues against the risks of "blind dogmatism," caused by an unqualified application of what — after the sixties — has come to be known as "hydraulic keynesism."[17] It is the same Columbia economist who candidly admitted his peculiar "middle way" position in a memorable passage of a letter he sent to John Maynard Keynes in 1941:

> "It has seemed to me that what I call the 'income-flow analysis,' of which yours is the most noted presentation, has done something which has not been done in comparable degree since Ricardo and Marx: namely, constructed a coherent logical theoretical system or formula having the quality of a mechanism, growing directly out of current conditions and problems which are of paramount importance and furnishing a key for working out definite answers in terms of policy. On this a 'school' has grown up. All that has tremendous power; and is also exposed to the dangers of too-indiscrimating

[15] Clark [1935b,] also reminded the reader that the expansion of public works may defeat its purpose by rising the cost of private construction. To support this claim he asserted that in 1934 governmental demand helped to maintain wages and prices of materials at levels which discouraged private buildings. On these issues see also Wiles [1971].

[16] "Moreover, if an increase of business is known to be due to a public deficit-spending, which presumably must come to an end before long, business may for that reason fail to respond with increased expenditures on its durable productive equipment, such as it would make if the same increase in business came from purely private sources." [Clark 1935b].

[17] For instance, as he explicitly put it in 1942 "Keynes offers a reversed Ricardianism, of similar power and exposed to similar dangers, including that of undue dogmatism on the part of disciples." [Clark 1942, 9, quoted in Dorfman 1970]. See also Clark [1941b].

application, from which 'classical' economics suffered, and of which I think the Gilbert-Humphrey attitude is one illustration.

*I am myself enough of an 'institutionalist' (whatever that may mean) to have more than a lurking distrust of formulas and equations! But not enough of an institutionalist to ignore their importance: merely to want to think all round them and reckon with the imponderables that modify their action: and the other factors which no single formula can comprehend — for instance, the long run incidence of continue large deficit-spending."*[18]

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[18] J. M. Clark to J. M. Keynes, July 24, 1941 quoted in Dorfman 1970, 13.

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**Appendix**

Clark's correspondence is published here with the permission of the Rare Books and Manuscript Library of Columbia University. The relevant material was found among the Joseph Dorfman papers, box 11, folder F. I am indebted to Pier Francesco Asso for encouraging publication and to Bernard Crystal, the assistant librarian, for much friendly cooperation during my research.

John Maurice Clark to Roche-Agussol: Sept. 14, 1918.

My dear Professor Roche-Agussol:

I am staying here with my father, and he has been prevented from replying immediately to your letter by the fact that he was off on a motor trip. I have made a list of the articles by him which appeared in the *New Englander* and tabulated the chapters in the "Philosophy of Wealth" to which they correspond. An article entitled "The Philosophy of Value" appeared in 1881, and is practically identical with chapter V of the "Philosophy of Wealth." My father will verify other bibliographical matter and let you have the whole.

As far as the material about myself, I was born in 1884, studied at Amherst College 1901-1905 and Columbia University 1905-1908, Instructor in Economics and Sociology, Colorado College, Associate Professor of "Political Economy," University of Chicago, 1915-. I have published a doctor's dissertation entitled "Standards of Reasonableness in Local Freight Discrimination," Columbia University Studies, 1910. (I would handle the theory quite differently if I were writing now). I collaborated with my father in the second edition of his "Control of Trusts" and am collaborating with W. H. Hamilton of Amherst and H. G. Moulton of Chicago in a book of readings in "The Economics of War." Also I have published articles on topics so varied as to indicate a deplorable lack of specialization. It happens that I recently published two articles in the Journal of Political Economy, January and February, 1918, on the subject of Economics and Psychology. These are an abbreviated version of some manuscript which is not ready for publication in more extended form. These articles indicate in a general way the type of study that is needed in order to develop the economic aspects of modern psychology; and in order to take into account those sides of human nature

that are left out of existing theory though they are very relevant to modern economic issues. Another thing on the same subject is a paper by Carleton Parker, read at the meeting of the American Economic Association last Christmas. (American Economic Review Supplement, March, 1918.) I think this is significant, and because of the particular headings chosen under which to catalogue the instincts, but because of the method of treating economic problems which is suggested: e. g. the study of labor troubles as results of balked dispositions. The results of such study might be important. Rationalism says: "To satisfy a man, give him what he demands." The theory of Professor Parker says: "To satisfy a man, study him to see if his demand (perhaps in itself impracticable to gratify) is not the expression of an underlying discontent due to causes which the man itself does not know and could not formulate in words." Professor Parker an myself, working independently, have treated two complementary aspects of human nature: he the innate qualities, I the modifying elements of the environment. You doubtless know also Prof. W. C. Mitchell's papers and articles bearing on the subject of psychology and economics: American Economic Review Supplement, March 1916, and Journal of Political Economy, vol. 29, pp. 1-47; also American Economic Review, vol. 2, p. 269. Also Walton H. Hamilton's articles, Journal of Political Economy March & April, 1918. My article in the American Economic Review, December 1917, on the basis of "War-Time Collectivism" (also much abbreviated from the original manuscript) has some treatment, I think, of the weakening of individualism and the enlarged scope for collective activity which result from facts of human nature which the marginal-utility theory has left out of account.

In attempting to say anything further as to the present position of psycho-economics, I am in a delicate position, as you will appreciate, but I want to explain my views frankly. I have been trying (1) to carry on my father's projects of study, (2) to reach an independent position as to the criticisms of his work. I have come to disagree somewhat with my father as to the nature of dynamic theory and its relation to static. I divide economics broadly into three divisions:

| | | | |
|---|---|---|---|
| A | Value Theory | (1) | Static value — or price — economics |
| | | (2) | Dynamic value — or price — economics. (This includes or presupposes (1) probably.) |
| B | | (3) | Dynamic <u>social or institutional economics</u>, or "realistic" economics. |

By "value economics" I mean any system in which exchange value, or the choices of individuals as expressed in exchanges, are taken as the final measure of economic qualities, so far as the system of theory is concerned. Such system may be part of a man's thinking, but subordinated or combined somehow to other less narrowly limited standards of judgment. "Value theory" includes all the marginal equilibrium theory, including Fetter's and Davenports. Both trends are found in the work of the great men, including Adam Smith, J. S. Mill, Sidgwick, Marshall, etc. etc. (see Hamilton's articles). Germs of both are found in the "Philosophy of Wealth," and the standard set for economics to work toward is of the broadest kind. I accept this standard, but in working toward it I find the effect on the static theory to be "rather chemical than mechanical." (Philosophy of Wealth p. 34). My father regards the marginal-utility view of human nature as partial only (Philosophy of Wealth, p. 36 last half) and he has constructed a tentative system on this par4tial basis after setting a standard that demands a fully and realistic "anthropology." And he also holds that a system based on imperfect anthropology cannot be completed by "making allowances for disturbing factors." (pp. 32-35).

Frankly, I think the term "psychological school," as used by Fetter, is an incorrect description. If we ever get a <u>real</u> psychological school we still have no more left to give it. The view of human nature which this school employs is either (a) Rationalistic or hedonistic: "man satisfies his wants in order of their intensity" and price measures marginal intensity of desire. (Philosophy of Wealth, Chapter V); (b) static: "<u>Man has a fixed order in satisfying his wants</u>" (Böhm-Bawerk) or (c) Agnostic. (/davenport, Fetter and others whose theory takes "human wants for granted" and does not go behind them). Neither (a) (b) nor (c) represents real psychology or is based on the positive results of psychological science.

I think the line of development for this type of theory is to be merged in theories based on broader premises. For these broader theories, it will be good to go through a period of "casting around" in whatever direction seems promising, guided by the varied insights of different students.

Economics studies the efficiency of our system of production to attain social ends. If it is time to break away from "marginal utility" and price as final measures of such efficiency (final, that is, for the purposes of economic theory) we are forced to study social ends directly — rally study the purposes that dominate nations and other social groups, larger and smaller. The system best suited to attain the ends of an

aggressively utilitaristic nation is different from that best suited to a pacific democracy, safe from invasion, as the United States has been in the past.
The study of social purpose is sociology and the economist should use whatever results the sociologists may have established, just as he should use whatever result the psychologists have established as to human nature. He should put his work in proper relation to whatever is known about social purposes.
The Marginal Productivity analysis is a different story. Here we have more to do with finance and industrial technique and less with psychology. Here there are some important lines of development open. (1) Study of the lack of correspondence between marginal contribution to the profits of the employer and marginal contribution to social product. J. S. Mill, Sidgwick, Landry, Pigou, Hobson, Veblen, etc., have hardly exhausted this topic. (2) Study of imputation on a more realistic basis. Static theory assumes that producers get the "best combination of factors" in the existing "state of the arts." But the very fact that the state of the arts is imperfect means that producers do not have the best combination. They are using a customary combination, improved by the method of trial and error or (if not improved) growing more and more obsolete. This destroys the exactness of imputation and thereby gives scope for much latitude of economic policy, even in a competitive society. These policies are governed by principles of some sort, which become a part of dynamic economic theory. E.G. wages aiming to stimulate efficiency for the future rather than to reward exactly the efficiency attained in the past.
In a word I am largely interested in seeing economic theory (a) to become independent of the exchange value of goods as a measure of social wealth, and (b) show the relation between individual choices and underlying social institutions in its true perspective.
I do not want to appear or to be quoted as engaged in critical attacks on the validity of the general system my father has developed. A full statement of my position includes a discussion of what validity means in economic theory. Stated in skeleton form, my idea is: (1) Absolute truth is impossible in general economic theory: it must be erroneous to the extent that it must be artificially simplified. The idea that much simplified results are still true pictures of a real <u>part</u> of the real world, is delusion in most cases if not in all. It is certainly so in the matter of human nature (Philosophy of Wealth, pp. 33-34), will explain more fully what I mean. Simplified theories are hypotheses whose resemblance to facts may be close enough to make their useful in certain situation and for certain purposes. (2) Such more or less erroneous pictures of parts of the world may be useful to help meet particular sets of issues. E. G. if the individual is a less incompetent judge of his own interest than anyone else, a theory, based on marginal utility and picturing the serviceability of individualism, is useful. But if science advances so that so that it becomes practicable (it must also be politically practicable, of course!) to judge the individual's interest for him better than he can judge them himself, such a theory comes to be an obstacle to getting the possible gains from the new achievements of science. We may say that the theory was formerly pragmatically valid, but that it has become pragmatically invalid. (3) More likely the situation is not so clean-cut and the theory remains "true" with reference to certain issues and "false" with reference to others. In other words, it is usefully relevant to some issues and not to others. The truth of theories is relative to particular issues to which they may be applied. (4) A particular issue is usually a conflict of principles: individualistic, socialistic, syndicalistic, feudal, cooperative, regulatory or what not. A purely individualistic economics can be usefully relevant only to those issues where the individualistic principle is right and the others are wrong. It needs to contain all the other principles expressing the good in all the other methods in order to be capable of furnishing a "description" of society that remains useful in a variety of circumstances.
So much for the general principles indicating what I mean by "validity." As to marginal utility, it has had and still has a great deal of pragmatic validity, but it is not actively relevant to most modern issues of social reform. The effect is passive, negative, toward such questions. This may be useful in combating crude forms of social revolution, mercantilism, etc. but there are more scientific measure of social reform, and the principles underlying them need to be developed to a rank coordinate with that given to marginal utility, and not treated as subordinate things, exceptions, allowances, or simply as separate "practical" problems without any theoretical bearing whatever.
I hope the outline I have given is not too condensed to make my meaning clear.
Yours very sincerely,
J. M. Clark

Alvin H. Hansen to John Maurice Clark: August 8, 1934
Dear Clark:

Thanks very much for sending me your chapter nine dealing particularly with the cumulative effects of public expenditures.

I regret that I have not had time to read it closely, and the comments I am making below are subject to the qualification that I have not given the chapter as close study as I hope to do later.

I wonder if you happen to have noted the long footnote to my chapter in the report of the Columbia Commission on pages 211-212? This footnote is, of course, too brief to really clarify the matter and there are some points I would now phrase differently even from this brief treatment.

In general, I have the impression that your analysis still follows too much along the Keynes lines. Keynes' analysis I regard as definitely wrong, and I hoped that I would be able to make a brief article clarifying the issue, but have not ad time to do so. It is, of course, possible that once I get thoroughly into it I will find that I am wrong and he is right.

It must be clearly be kept in mind, I think, that Keynes' multiplier is really a transactions velocity of money concept. In his Means to Prosperity, he shows this clearly by giving the fallacious argument advanced by all the Fisher script money supporters that if a city put out stamp money to its unemployed, the income of the city would be increased per annum by the number of times this stamp money circulated. Keynes swallows this whole, and argues that the only thing wrong with this line of reasoning is that it overlooks the "leakage." Keynes' receipts in each transfer, in short, are not "income" but "gross income." I have the impression that there is, on this point, some confusion in your page 155.

The first thing, it seems to me, absolutely necessary is to eliminate wholly the conception of transaction velocity from this whole discussion. So long as this concept is included, confusion results. The whole thing, it seems to me, is a question of (1) the volume of means of payment, or "money" and (2) the income velocity of "money."

Public works may affect either one of these two or both. It is quite correct, as Keynes said, that the public works expenditures may not enlarge the volume of "money." In this event, the effect presumably is to transfer money from idle hands to active hands. In other words, the income velocity increases. Or there may actually be injected into the market new "money," and the income velocity might conceivably remain on the average as before. Or there may be a combination of these two tendencies.

Keynes' "leakages," I think, are also dangerous. The most important "leakage" is his saving "leakage" and this when analyzed amounts to nothing more or less than our old friend, a change in income velocity. If all of us hold idle a half of our income which we formerly spent, — save it without investing it, in other words — the income velocity has been cut in two. Thus the total income of society in the next succeeding period has been reduced to one-half by this process of "savings running to waste". These "leakages" in short, apply not only to governmental expenditures, but to all incomes in deflationary periods.

This leads me to remark that it does not seems to me that you have rightly stated the position of what you call the "income velocity group." It is certainly not my view that governmental expenditures will result in a continuos inflation as you infer on page 171a and 176a and in various other places. For example, there might be merely an increase in "money" as a result of the funds thrown into the market by public expenditures. The total income would be increased correspondingly in the next succeeding period and thereafter would remain constant unless there were a further change either in the volume of "money" or in the income velocity of "money."

You are, I think, quite right in emphasizing the point that one can lay down no mechanical rule (as Keynes does) of a progressively declining rate, one-half, one-fourth, one-eighth, etc. it is always Keynes' defect to be far too mechanical. It is quite impossible to appraise the effect of public expenditures in any such mechanical matter. Therefore, I think the whole approach is fallacious. All that public expenditures do is to throw new funds into the market, and thereby increase the income, which action, since it is certainly not likely to reduce the income velocity of "money" is very likely to increase the total "money" income of society by more than the amount of "money" injected. It is, therefore, correct to say that public expenditures are likely to have an effect on income in excess of the expenditures. This is all there is to it, it seems to me. But the precise effect upon the volume of "money" and upon the income velocity of "money" will vary enormously from one phase of the cycle to another, and in different centuries, and at different times, and therefore no formula can be laid down indicating what the results will be. If, however, one knows at the bottom of the depression what the income velocity of "money" is, it seems that one must be at least safe in assuming that public expenditures would increase the "money" income by the new "money" injected multiplied by the then prevailing velocity of "money."

I am exceedingly skeptical of Angell's 1.6 income velocity, Pigou arrives at the figure of 3.0, which seems to me about right in normal times. I do not see how it could possibly be as low as 1.6 unless one assumes, as Angell may do, that savings deposits are part of the means of payment. This view I should challenge.
I have stated the points as you will note far more dogmatically than I have any right to, for as I have said, I have not thoroughly canvassed the matter, but for your purpose this dogmatism will surely do not harm.
Very sincerely yours,
Alvin. H. Hansen

J. M. Clark to G. S. Hauge: December 16, 1937
Dear Mr. Hauge:
Your inquiry raises the question of a change in my father's attitude from the abandonment of competition in his "Philosophy if Wealth" to a defense of it as a morally justifiable system, in the "Distribution of Wealth."
I do not think my father was conscious of any change in his basic attitude. And I do not recall the earlier book as an "abandonment" of competition — rather as a recognition of evils and a plea for moral elements, with hopes of developing cooperative institutions which should themselves make their way by the competitive route; competing with competitive business, if I recall correctly.
What happened was in the first instance largely the direct and indirect consequences of the method he adopted for breaking up the manifold problem of economics and attacking it systematically, step by step. The "Philosophy of wealth" was his first reaction to the economic system as a whole; the distribution was a first step, and avowedly only that, in a more systematic analysis: in which he isolated the economic forces corresponding to the force of gravity, by the device of a static equilibrium, implying perfect markets, perfect competition and perfect fluidity. Thus the second book is not supposed to be a realistic picture of existing society, nor concerned with the grindings of the actual and imperfect market mechanism. In the "Philosophy" he contemplated the latter. Labor could be exploited by the use of bargaining advantages and a "good" bargain is morally a bad one. (Nobody could get a better bargain than anyone else in the "static state" described in the "Distribution.")
On one point you can find both the first and second stages of thinking in the earlier book; namely, on the matter of the economic conception of human nature. He demands a more realistic "anthropology," which cannot be had from the asking; and in the meantime proposes a first approximation. This turns out to be his form of marginal utility theory, which is essentially a static abstraction in the field of human nature (as I tried to show in my article on Economics and Modern Psychology, Jour. Of Pol. Econ., 1918).
The third stage of the process was to have been dynamics, conceived as putting in what statics left out. In the mean time, several things had happened. He had grown older. The time spent in developing the static abstraction presumably influenced his later perspective, tending to make him feel that his first approximation (the static state) was nearer to a representative picture than he might have thought when writing the articles which later became "The Philosophy of Wealth." It has been said by critics that static elements clung to his later dynamics. I could explain my own views on this, but hesitate to do so, for the same reasons which have made me avoid this issue in print. You may find implications bearing on this in my contribution to my father Festschrift; also in my "Social Control" volume.
Another major thing that happened was external. In the period leading up to the "Essentials" competition was threatened, not by a benevolent cooperation, but to exploitive monopoly, typified in the Standard Oil and Tobacco "Trusts" — also in restrictive labor union policies. One is influenced by the evils one reacts against! And my father had a faculty of simplifying a complex situation by picking out the element which seems to him to deserve paramount weight. In this case the need of defending competition against monopoly appears to have been that element. Even here there is continuity of attitude with the "Philosophy of Wealth," the continuing factor being the emphasis on the need of moralizing competition by developing and enforcing standards of fair competition.
It is interesting to be invited to review and formulate my ideas on this matter.
Sincerely Yours
J. M. Clark

Paul A. Samuelson to John M. Clark: April 16, 1953
Dear Professor Clark:
The Millikan volumn *Income Stabilization for Developing Democracy* has finally come out and I am enclosing a reprint of my own contribution. Perhaps you remember reading it at an earlier stage and giving

me the benefit of your criticisms. I felt that I benefited immensely from them and any remaining inadequacies must naturally be blamed on me alone.
I read in the New York Times of your going over to emeritus status. At the time I felt the urge to drop you a note telling you how much I think you have added to American economics. But as so often with these impulses, it passed off before I had gotten around to doing anything about it. May I take this occasion to reiterate these sentiments and wish you pleasant years ahead.
I greatly enjoyed your chapter in the Spiegel volumn and also your remarks in the Impact of the Union dealing with your father's role in this history of economic doctrines.
At the time of the American University meeting, you may remember mentioning that at some future date I might write asking you about the psychological genesis of the theory of the accelerator principle in your mind. I have always been interested in the process by which our ideas come to us — such extra-logical considerations as were involved in Henri Poincaré's perceiving a complete mathematical theory at the moment he stepped on a street car. There is no reason why at this late date you should remember how this idea first came to you. But if you do and if it is the sort of things that merits communication, I should find the information exceedingly interesting.
Thank you again for your help.
Sincerely yours,
Paul A. Samuelson

John M. Clark to Paul A. Samuelson: April 21, 1953
Dear Samuelson:
I appreciate your letter, with its good wishes, very much, also the reprint which I look forward to examining. I have reason to regret that I have not had enough foresight to ask in advance for the kind of reprint of things I have contributed recently to collaborative volumes. I could have made good use of such reprints if I had them.
About the genesis of the "accelerator principle" I am afraid I have no definite picture of the precise process. It grew out of my reading of Wesley Mitchell's original volume of business cycles, in which he emphasized monetary or financial factors, and it seemed to me that there was a physical or technical factor implied in his description of the cumulative processes (or if that is too strong a term, at least implicit in the process) and that it might usefully be separated out for purposes of analysis. Of course, the distinction between the two sectors of demand for capital — replacements and net additions — was well understood, so the separating out of the element of net addition and putting it into a simplified theoretical model was a pretty natural step. I had it in mind when I came across Mr. Leigh's railroad figures, and they seemed very pertinent. I did not think I realized at the time that this was a peculiarly available selection of figures, because it involved a sector of demand for capital which responded to a demand for the proximate end product — namely, freight transportation — which registered the general state of economic activity, while the item of capital demand involved was sufficiently limited so that by the reverse impact of capital demand on flow of purchasing and demand for ultimate products — Or not too much confused?
Your question reminds me of a couple of other instances: the proposition about the sum of the marginally-imputed products absorbing the total product, and the conditions necessary to this, and the working out of a rough form of the R.F. Kahn-type multiplier. I remember where I got the answer to the first: namely in a hotel room where my father and I had gone to meet David Kinley. I had been wrestling with the problem, and figured out a geometrical solution. Later, I talked it over with my friend, Charley Cobb, and he converted it into a case of Euler's theorem. Before trying to publish it, I did some investigating, and discovered Wicksteed's monograph, and Flux's review, which converted Wicksteed's demonstration into a case of Euler's theorem. So that had clearly been anticipated.
As to the multiplier, I got that idea as one answer to the problem why an expansion, with its cumulative effects which were well recognized, should turn into a contraction. And it occurred to me that if the expansionary effects of an increasing capital investment returned in cumulative fashion, but diminished by "leakages", that could produce a series of the type, the sum of which to infinity is a finite quantity, and also that the time-profile of that series would be an asymptotic curve, concave downward, increasing at a diminishing rate. Combine this with the accelerator principle, with time lags, and you could get a model that would convert the expansion into a contraction; as you showed in your article in equating the accelerator principle with the multiplier. I always wished that you had made more deviations from mathematical "elegance", and explained, in language that a non-mathematician could assimilate, how or "why" the various results that you showed in that article came from the various combinations of factors worked out. I did not do

anything at the time with this idea, being very busy with other work, and in any case, R.F. Kahn must have been well along with his multiplier study (which was published, I think, maybe a couple of months after I got the notion) so that he properly had priority.

I am afraid that none of these recollections give you the precise psychological point you are fishing for. I may not have taken note of that sort of thing at the time and at any rate I do not remember it now.

With best wishes,

Sincerely yours,

J.M. Clark