§ I. The childhood of every science is characterised by the prevalence of "schools," of bodies of men, that is, who swear by bodies of doctrine, which differ toto caelo from each other as to philosophic background and fundamentals of methods, and aim at preaching different "systems" and, if possible, different results in every particular—each claiming to be in exclusive possession of Truth and to fight for absolute light against absolute darkness. But when a science has "gained man's estate," these things, whilst never ceasing to exist, tend to lose importance: the common ground expands, merits and ranges of "standpoints" and "methods" become matter of communis opinio doctorum, fundamental differences shade off into each other; and what differences remain are confined within clear-cut questions of fact and of analytic machinery, and capable of being settled by exact proof.

Our science is past its childhood, but has not reached its manhood yet. On the one hand, our patience is still being tried by the phraseology of "schools" and "-isms," and there is still plenty of scope and shelter for the products of bad workmanship passing themselves off as new departures; but, on the other hand, the really living part of our science shows hopeful signs of, if I may say so, that convergence of effort, which is the necessary and sufficient condition of serious achievement. Those economists who really count do not differ so much as most people believe; they start from much the same premises; problems present themselves to them in much the same light; they attack them with much the same tools; and, although some of them have a way of laying more stress on points of difference than of points of agreement, their results mostly point towards common goals. This is not only true of fundamentals of fact and machinery, but also of what is going on within the precincts of every one of our time-honoured problems.

The problem of the business cycle is a case in point. It pre-
sented itself to the economists of the classic period and their immediate successors in the aspect of the striking fact of recurring "crises." Two first results were speedily established. The one, negative only but of the greatest "diagnostic" importance, was that there can be no such thing as a general glut. The other, that crises are—I really ought to say that it is extremely probable that crises are—an essential element of the capitalistic process and not merely occasional breakdowns to be individually explained by accidents different in each case—just breakdowns which happen if anything of sufficient importance goes wrong. ¹ But barring these two points, discussion went to pieces on fundamental differences in the views about the capitalistic process, each author drawing different conclusions from different fundamentals; and finally languished in an atmosphere of theoretic hopelessness. Then came the great impulse due to the genius of Clément Juglar. He, first, by showing that crises are only elements of a much wider and deeper cyclical movement, unearthed the real problem; he, second, succeeded in describing empirically this cyclical movement; and, third, he contributed substantially towards its explanation. Few only were his immediate successors, such as Des Essars. But later on, set in the great torrent of descriptive studies of the cycle which is one of the characteristic features of modern economics and which—not perhaps consciously inspired by Juglar, but still flowing from his source, by virtue of the logic of the scientific situation—permits us to see, where our predecessors had to guess. This torrent does not, by itself, supply us with a solution of the problem. Part of its waters are, besides, more useful in helping us to understand the peculiar features of individual cycles than in answering the great question: Why there are such things as cycles at all. But it gives endless opportunities to the analyst—whose own tools have been much improved meantime—for finding explanations and verifying them. And so we have reached a stage, perhaps for the first time, where facts and problems are before all of us in a clear and in the same light, and where analysis and description can co-operate in something like the spirit of physical science.

¹ It is part of the nature of the problem, that it is not very easy to prove this in strict logic. For there are always things happening to which an individual crisis can be attributed without any glaring absurdity. All we can say is: (1) That from our knowledge of the phenomenon we gather a strong impression that its causes are more than casual and that they operate from within the system and not from without on the system. (2) That if we find, by analysis, a cause adequate to produce the phenomenon without extraneous influences—though such influences may be operating in each case—we are justified in accepting this cause as an explanation of what we may call the "essence" or "nature" of the phenomenon.
§ 2. As a matter of fact, this is the line along which we are now moving. It stands out clearly in all the best work done, for instance, in the important theory of Spiethoff. But, hopeful as the situation is, it calls for aptitudes not often found together. And as few men unite, as Professor Pigou does, consummate mastery of statistical facts and of the art of handling them to unrivalled command of the analytic engine, it is but natural for us to approach his recent work on the subject1 with the very highest expectations. These expectations have been amply fulfilled. The book is an admirable achievement. It is impossible to give, within the limits of an article, an adequate impression of all its fruitful and original contributions. We shall, in the main, confine ourselves to Part I—“Causation”—and refrain from entering, except incidentally, upon a discussion of the vast and complex issues dealt with in Part II—“Remedies”.

There are really—subject to a qualification to be introduced later—four groups of problems which come under the head of “Industrial Fluctuations”: the seasonal fluctuations, the “cycle,” the “long waves,” and the secular trend. Professor Pigou’s analysis is limited to the second, and we shall follow his example, although we confess to a feeling that it is the two last-named which will before long absorb the attention of the workers in this field, and that the problems of the cycle cannot be dealt with quite satisfactorily without reference to them.

Now a distinction occurs at the outset, the triviality of which does not deduct anything from its importance. By “‘theory of the business cycle” we may mean, first, an analysis of any single one of the cycles which history records, or, arising out of such analysis of many or all recorded cycles, a reasoned history of the phenomenon. The most eminent instance of this type is Professor Mitchell’s book. Second, we may mean by that expression a general theory, as exhaustive as may be, of all the elements contributing, or likely to contribute, to the phenomena we observe and of their interaction. Third, we may mean something different again, viz., a theory of what we conceive to be the fundamental cause. Neglect of this distinction has repeatedly led to misunderstanding. So, for instance, Pareto held that there is no sense in asking the question, what “the” cause of interest is—interest being evidently the result of all elements of the economic system. But although it is, of course, true that the rate of interest, at any given moment, is a function of all other economic quantities existing at that moment, it does not follow that that question is

futile, and saying so only serves to confuse issues. Nowhere is the fallacy alluded to more specious and dangerous than in the particular case of the theory of cycles. For nowhere is it more difficult to disentangle the fundamental from the accidental, or more easy to cover the shortcomings of an explanation by a wealth of secondary considerations, and insufficiency of analysis by an appeal either to the complex mass of detail, always so convincing to the "practical-minded," or to the great principle of economic interdependence, which occasionally covers a multitude of analytic sins.

Professor Pigou's theory is of the second type. I do not mean to imply that he fails to offer a "fundamental explanation" of the phenomenon. But he aims at more than that, and it is precisely this comprehensiveness of survey which makes the book so valuable—not only to the student but also to the business man, whose needs are but little served by a mere "pure" theory of the cause or causes of the cycle—and assures it of a high place in the first rank of the contributions to its subjects. To discuss only the fundamental explanation of the cycle which it offers, and to neglect that it clears up many problems of industrial movement in general as its argument unfolds itself, is to do much less than justice to it. This is, however, what limits of space in some measure compel us to do. We can only mention in passing that the author deals with important subjects, relevant to industrial movement of any sort besides the cyclical one, with a mastery all his own, for instance, with the "autonomous monetary causes of industrial fluctuations" (Ch. VIII)—which he quite rightly distinguishes from those monetary causes which belong in a special sense to the mechanism of the cycle and will come in for discussion in the second part of this paper—or with the part played by rigidity in wage-rates or by the imperfect mobility of labour (Chaps. XIX and XX), subjects, to be sure, without which there is no understanding of all that happens in the cycle and which, therefore, form part of its "causation" in Professor Pigou's sense, but subjects too, I submit, which do not form part of its "causation" in the sense which we mean here and which will be made clearer presently. The writer begs leave to avail himself of the Editor's invitation to start, in discussing Professor Pigou's theory, from his own views, arrived at in 1909, first published in 1912 and capable of being compressed into a very few propositions:

§ 3. (x) The first of these is that there would be no cycles under "static" conditions. This seems self-evident, because we are still in the habit of mixing up "static" and "stationary"
conditions, a habit, by the way, responsible for much that is unsatisfactory in our apparatus of analysis. What I want to say is: Those elements of the economic process, the description of the interaction of which makes up the theory of economic equilibrium, do not contain anything out of which a tendency towards cyclical movement could automatically arise. This is not self-evident, but still need not be proved to readers familiar with Marshallian analysis.

(2) "Static" conditions are compatible with continuous "growth" (or decline) such as would be the consequence of the mere fact of an increase (or decrease) of population and capital. For it is no part of the system of assumptions of "static" theory that there should be no shifting of the centre of gravitation of the economic cosmos. All that is required is that the economic process should adapt itself to such shifting simply by trying to find the new equilibrium by small alterations of quantities. We may, then, speak either of an equilibrium of growth or—as we prefer—of an equilibrium which, though continually disturbed by growth, continually tends to be re-established. There is nothing in this which, by itself, could produce the business cycle. Professor Pigou lends the weight of his authority to this proposition, by word as well as tacendo. And indeed theories looking for an explanation of the cycle in the increase either of population or of capital seem to me hardly worth discussing.

(3) I always thought, and still think, that in order to find out whether or not cycles are a phenomenon sui generis, clearly standing out as such from the rest of industrial fluctuations and arising from within the economic system, we ought, in the first instance, to assume the absence of outside disturbances—non-economic ones, or economic ones which cannot be produced or avoided by economic action, both of which we are going to call "casual"—acting on the system. We shall, then, see either that the economic system never (and not only not under "static" conditions) evolves that particular kind of fluctuations of itself, in which case outside disturbances must be looked upon as responsible for them; or else that the economic system would of itself display "cyclical" movement, in which case we should have to recognise the presence of a problem of a "normal cycle"; we should, moreover, have to conclude that the whole of purely economic phenomena cannot be exhausted by means of the "static" apparatus; and we should, finally, have to look upon the influence of outside disturbances as a fifth set of problems within the genus of industrial fluctuations, which would, indeed, also form part of any comprehensive survey.
of all that happens in cycles (because outside disturbances of some kind never fail to arise and always must react upon the cyclical movement), but which would have to be kept aloof in a theory of causation, in a sense which I hope is now quite clear.

Professor Pigou, after having rather severely reproved those who uncritically look for "causes" of industrial fluctuations, proceeds to draw a distinction similar to and still fundamentally different from ours. He distinguishes the problem of "initiating impulses" from what I may term the mechanism of the cycle. His "initiating impulses" being substantially what I mean by outside disturbances, such as exceptional harvests, wars, social unrest and so on, he merges what are our second and fifth sets of problems into one; and his views seem to us to come to holding that there are no causes within the system sufficient to produce the cycle and that its theory can only consist in describing the mechanism through which initiating impulses act as they arise, some of them sporadically, others periodically. This does not, indeed, diminish the value of Professor Pigou's contributions to our knowledge of that mechanism—which proves to be applicable to even a much wider range of facts beyond the cycle—but it does interfere with his doing justice to the fundamental problem, and puts a gulf between him and what seems to us the line of advance chalked out by the best work done so far: and in fact, we grieve to say, he does not so much as mention Juglar, and even the name of Spiethoff is absent from his pages.

We only need, however, look at the way in which any disturbing element acts in order to be confronted with a distinction, both natural and important, which points in our direction. If, say, a war breaks out and upsets existing equilibrium, people can try to adapt themselves to altered conditions by infinitesimal steps, reducing, for instance, their consumption or, in their business, accepting the higher takings they get and paying their higher expenses, adjusting the quantity of their product accordingly. They may not be able to so adapt themselves and perish. They will, in so adapting themselves, be of course subject to all sorts of error. Still, we have here a well-defined type of behaviour admirably fitting in with "static" theory; and a type of behaviour, too,

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1 I do not know that anyone ever did look for the cause of industrial fluctuations in general. But I submit, that no looseness of thinking need be implied in looking for the cause of the cycle in the sense I have been at pains to define.

2 He also includes invention, of which more presently. He is careful, moreover, not to rely merely on these disturbing elements themselves, but on the expectations they give rise to. But this makes no difference just here.
which we have before our eyes in real life, for this is the only way in which the majority of people do act and are capable of acting. But however high we may put the explanatory value of error and friction, this is emphatically *not* the way in which booms arise, and not the kind of events of which booms fundamentally consist, as will be seen as we go on.

There evidently is another way of reacting, clearly distinguishable from this, although shading off into this on the border. People can also drop their attitude of passive adaptation, they can react by doing new things or things in a new way, incompatible with the fundamental arrangements that exist. The clerk, instead of reducing consumption, can go into business for himself; the manufacturer can change his cotton-mill into an ammunition factory. Some people—never all nor ever more than a minority—do that. This is a different kind of behaviour and not within reach of marginal variations; and it is productive of different consequences.

Now, on the one hand, although, if distinguishable, these two kinds of reaction are both of them invariably set into motion by any "initiating impulse," it is only the first of them which can be said to follow automatically from the outside impulse by virtue of a causal connection exhaustively described—and determined—by theory. The second kind of reactions is not gripped by our analytic machine—although of course their consequences are—unless we "put a new arm" to it, which is precisely what I have been trying to do since 1912; and they cannot, with any certainty, be relied on to happen, or be predicted to happen, in any definite way in practice: they *could* fail to show up, in which case there would be no boom; whilst *if* they show up, it is never the mere occurrence of the disturbance which produces them, but a *certain attitude of certain people*.

Again this attitude, on the other hand, exists and shows itself quite independently of the presence of any disturbance. To avoid misunderstanding—of course, the type of behaviour we are glancing at now always has to do with a given environment, and environment always includes some sort of disturbance. But if there were not the one disturbance there would be another. And if there be none, the "impulse" would be *created* by our type. There is always scope for this. Industrial and commercial methods are never perfect in any sense except relatively to the average light and energy of the business community. Knowledge—scientific and other—is always far in advance of actual practice, not only in things which it could not, or not yet, pay to carry out, but also
in things which it would. Results of invention—not only, again, impracticable ones—are always offering themselves, but may lie unused indefinitely. Why? Because doing what has not yet stood the test of experience is no mere act of ordinary business practice, such as we primarily think of when applying our theoretic apparatus, and such as the average man of business can be relied on to do promptly, but something else which wants an attitude and an aptitude, different indeed from what is required for the act of invention, but equally rare—an attitude and aptitude more of character—"power," "leadership"—than of intellect. Hence there are always great prizes to be won by those who have them, the business community does not, and cannot, proceed to new methods, as it were, in line: some rush ahead, others lag behind; and the latter are forced onwards or ruined by competition setting in from those who lead. Nor are these things mere frictions such as theory can afford to neglect; fundamental phenomena of modern industrial life depend on them for explanation—the business cycle among them, for the explanation of the nature of which this set of facts—which lies outside the domain of static theory but still within the economic system itself—is both necessary and sufficient, as I hope to show. Meanwhile, we only want to point out that willingness and capacity to do new things will always and necessarily find, or be able to create, the opportunity on which to act, being, in fact, itself the one fundamental "initial impulse" of industrial and commercial change.

In this sense, therefore, I claim "independence" of the cycle and of those booms and depressions which form the normal cycle of impulses from without: in the face of the facts, first, that such events do also lead to booms and depressions displaying a very similar mechanism and very similar features; second, that every one of the "normal cycles" is, as a matter of fact, powerfully influenced and coloured by some disturbances from without—any given situation being subject to such disturbances, which may help on, or rein in, any given upward or downward movement, and offer, as it were, part of the material of which the fabric of every boom consists, but which, if absent, would be supplemented by other material always at hand.

I also submit that this distinction of phenomena, which in

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1 The attitude and aptitude making up willingness and capacity of carrying out new things—which may be very trivial of course—is probably distributed among people according to the normal law of error. We must not be understood to hold that some people have it all in a high degree whilst the rest are entirely without it. But this does not affect the gist of our argument, and we cannot go into details.
reality always go on together and react upon one another, is no matter of theoretic nicety. For to the distinction in theory corresponds a distinction in reality. If we are furnished with sufficient details of a case, we are always able to tell whether it belongs to the static or non-static sphere—a movement of the rate of interest, for instance. Nor is this all. It is of very considerable practical importance to distinguish between booms of different nature, and it makes a great difference both to diagnosis and to remedial policy, whether we have to do, say, with a crisis of deflation or with the depression of a normal cycle. Neglect of this distinction vitiates, I think, part of what I otherwise consider most valuable results of recent research.

(4) Our fourth proposition is the one due to Juglar: "La cause unique de la dépression c'est la prospérité." That is to say, that the phenomena which we have got in the habit of calling "depression" are no irregular heap of disturbances, but can be understood as the reaction of business life to the situation created by the boom or, more precisely, as the movement of business life towards a new state of equilibrium conforming to the data created by the boom—such being what I may term "normal" depression as distinguished from "abnormal" havoc, incidentally wrought by panic, and productive of consequences of its own. It is important to note that by reaction I do not mean a psychological one, although this, too, must always play an important, though secondary, part, a part, that is, which is secondary not only in importance but also as to its position in the chain of causation. The new data, created by the boom and upsetting all the bases of industrial and commercial calculation, are an "objective" fact. As such they enforce "objective" adjustments. And these and the losses they entail would account for what happens in the period of depression, even if nobody lost his head or turned, by zoological miracle, into a "bear."

It may not be superfluous to ask the reader to bear in mind two more points: we should not, of course, be justified in applying the same sort of reasoning to the boom. There are authorities who barely escape this sort of perpetuum mobile reasoning, according to which there would be booms because there are depressions, and depressions because there are booms. This reasoning derives some support from the fact that depression, by lowering prices of materials, machines, labour and "going concerns," affords the opportunity of buying cheaply. I need not stay to show why this support is insufficient. But I want to emphasise that we are doing nothing of the sort.
As will be readily seen, moreover, all theories of the cycle—including those of, say, Marx, Hawtrey, Pigou—are at liberty to accept this proposition. Whatever their explanations may be, they all consider what happens in depression to be the consequence of something which happened in the boom, or, anyhow, before the crisis or depression itself.

(5) We shall, therefore, have explained the cycle when we have explained those booms which are so clearly before our eyes ever since (at least) the Napoleonic wars, which we can so well distinguish from other fluctuations, and in which we can, I think, equally well distinguish what they owe to their own and to extraneous impulses. Those booms consist in the carrying out of innovations in the industrial and commercial organism. By innovations I understand such changes of the combinations of the factors of production as cannot be effected by infinitesimal steps or variations on the margin. They consist primarily in changes in methods of production and transportation, or in changes in industrial organisation, or in the production of a new article, or in the opening up of new markets or of new sources of material. The recurring periods of prosperity of the cyclical movement are the form progress takes in capitalist society.

By saying this we mean to state a fact requiring both proof and explanation. Whilst we hope to be able to contribute, by our two last propositions, something towards the latter, it is impossible here to satisfy the reader as to the former. But the fact is becoming recognised more and more, and it is, for instance, clearly hinted at by Mr. Robertson—with that amiable diffidence of his—on p. ii of his important little book, to which we shall return in the second part of this paper. The reader needs only to make the experiment. If he cares to survey industrial history from, say, 1760 onwards, he will discover two things; he will find, first, that very many booms are unmistakably characterised by revolutionary changes in some branch of industry which, in consequence, leads the boom—railways for instance in the 'forties, or steel in the 'eighties, or electricity in the 'nineties—and that, if he will take a bird's-eye view of our industrial organism, he will be able to follow up every one of its leading features to a source originating in a boom. And he will find, secondly, that all the booms which he may find himself unable so to characterise can be shown, by other and independent reasons, to be casual phenomena outside the cyclical movement and distinguishable from it, such as the booms ending in the collapses of 1793, 1799, 1810 and 1922, which, to my mind, lead to the most palpable mistakes both of analysis and policy if mixed up with the
cyclical ones. It is equally important—and possible—to distinguish cyclical depressions from mere "breaks" such as the crises of 1866 and 1901—even as a doctor must distinguish between the going down of the temperature of his patient owing to his progress towards health, and the breaks the curve of temperature may occasionally display for all sorts of reasons.

Further corroboration is afforded our proposition by the fact, brought out beyond doubt by statistical investigation and quite universally admitted, of the prominence of the constructional trades, both as to priority in time and as to amplitude of fluctuation, within the events of the cycle. I do not know one modern writer who would deny it. But if the fact be undeniable, it evidently fits in admirably with our thesis: it could not indeed prove it, for there is no such thing as statistical proof. But it is eminently apt to serve as verification; for it derives a very natural explanation by our thesis, which alone, in fact, gives it its proper significance and sheds on it its true light.

It is instructive to look at Professor Pigou's treatment of these points from this angle. He overlooks none of them. The question of instrumental industries he deals with in §9 of his second, and again in his ninth chapter, and "inventions" come up for discussion in §§11-13 of Chapter IV. But he arrives at results substantially negative in much less space than he devotes to what seem to me secondary points. I submit, with due deference, that this would have been impossible, if he had:

First, used the Spiethoff index, which brings out the salient features much more clearly than Professor Pigou's figures do, or those of the authorities he quotes on page 20; then he would hardly have called the evidence "less clear" than that which establishes other characteristics of the cycle.

Secondly, taken hold of the link obviously existing between what he calls "invention" or, rather, the putting into practice of it and the constructional industries or, to use Spiethoff's\textsuperscript{1} term, goods of reproductive consumption; for although it would still have been logically possible to dismiss, as he does in Chapter IX, the significance of the fluctuations of these industries on the ground that these fluctuations being larger does not prove that the cycle originates there, that significance, by being connected with new improvements, would have more strongly impressed itself. Whatever the nature of the innovation actually being carried out, there will

\textsuperscript{1} In emphasising the importance of the work of my eminent friend and colleague, I must be careful not to attribute to him any views of mine. His theory is different from mine in several points, and what we both consider the fundamental "cause" is among them.
be always the necessity of providing new buildings, machines and so on, which means that innovation or reorganisation must always, in the first instance, show itself in an increase in the consumption of iron and steel.

*Thirdly*, not focussed his attention so strongly on the element of "invention," which it is, indeed, easy to dismiss by pointing out that it is not invention that matters, but its adoption and actual working (p. 44). This very fact points in our direction. And so does his saying that we are not justified in inferring from the fact that without Stephenson's invention there could have been no railway mania, that there would have been no boom in 1845-7; for railway development may have been merely a channel into which industrial activity, caused in some quite different way and due to come into play, found it convenient to flow. Quite so. But does not this apply to any initiating impulse? If no, why then to this one? If yes, is it not imperative to develop for the purposes of fundamental explanation an analysis independent of the occurrence of impulses from without—an analysis of the way in which new things come to be done in industrial life, and old methods come to be eliminated, together with those firms who cannot rise above them?

(6) But innovations would be powerless to produce booms, if they went on continuously in time. By this we mean, that if it were possible to choose units of time such that to each of them would correspond one new thing done—it need of course be no "invention" carried out—then the disturbances which would still be caused would be small as compared with the whole of the industrial life of a nation, so that they would be capable of being continuously absorbed—just as simple "growth" is—without producing consequences important enough to show. There would be no cycles, though still, of course, irregular disturbances owing to wars, earthquakes, and the like.

*Therefore, the problem of causation of the cycles reduces itself to the question (the answer to which contains what we shall call in a sense not now admitting any more of ambiguity, the only "cause" of cycles): Why is it that industrial and commercial change is not continuously distributed in time, but proceeds by leaps which, it is easy to understand, must fundamentally alter the bases of calculation and upset the existing equilibrium beyond the possibility of all people adapting themselves successfully by marginal variations?*

(7) It is simply because as soon as any step in a new direction has been successfully made, it at once and thereby becomes easy to follow. Business life, like any other, consists mainly of routine work based on well-tried experience, partly ancestral; only within the
boundaries of routine do people function both promptly and similarly; it is only to routine work that received theory applies; outside routine most people find it difficult—and are often unable to act; those who can are rare and therefore not subject to competitive conditions, whence the phenomenon of profit; but whenever in a given situation (which theory has the right and the duty to assume to be in the first instance "static") new things have been successfully done by some, others can, on the one hand, copy their behaviour in the same line—whence prominence of one industry at the time—and on the other hand, get the courage to do similar things in other lines, the spell being broken and many details of the behaviour of the first leaders being applicable outside their own field of action. And therefore the first success draws other people in its wake and finally crowds of them,1 which is what the boom consists in.

§ 4. I beg leave to ask the reader not to be deterred by what must necessarily look like a highly abstract if not one-sided view of the thing. Of course this is no theory of the cycle, if we understand by this a complete explanation of all that happens. This can only be found in a reasoned history of industrial life. It is only the backbone of it. But I submit that Professor Pigou's analysis of detail, or Professor Mitchell's, or, indeed, Professor Clapham's facts, do fit in exactly with the view explained—in some points much better than with the fundamental views of those eminent authors themselves. I also submit that our propositions, whilst strikingly verified by experience, are hardly open to objections on theoretical grounds. Propositions (1), (2), (4), (6) I do not even see a possibility of denying. Proposition (3) does no more than introduce a distinction, which might be useless, but could not possibly be false. For Proposition (5) I can point both to statistical evidence and to what we have before our eyes in real life. It is true, as has often been pointed out, and as Professor Pigou points out again, that there seems to be more "brain" in business during depression. But this does not prove anything against our theory, for it is but natural that competition setting in from the side of innovators should force the "crowd" to try their best to save themselves by improving their methods. Proposition (7) seems to me to be a very natural way of explaining what remains to be explained when once we accept the six others. I may also say that, as far as I can claim having had any practical insight at all, it seems to me but to formulate what I think I have

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1 Let me ask the reader to bear in mind that these "crowds" need not necessarily be making any mistakes in following a lead. They are, of course, very likely to. But depression, though milder, would still come about even without any errors.
seen. A sort of verification is finally afforded by two facts, which I am sorry not to be able to go into more thoroughly.

There is, first, the fact of booms as well as depressions becoming milder—the last real "crash" in Europe having taken place in 1873. Now there are many ways of accounting for this, all of them compatible with our theory. But there is one which we can, I think, directly derive from it, viz., the steadying influence of great units, especially of trusts. As the industrial units tend to grow, the management tends more and more to be divorced from ownership. Therefore, whereas the rising men had, in the times of our fathers, typically to found new businesses and to get their things done by under-selling the old ones, the rising men of a later period are not confined to this method, but can and do conquer leading positions in the new big units now existing, and impose on them their plans. It is evident that, as far as this is being done and as far as, consequently, the new things tend more and more to grow out of the units already existing, the simple change of the managers does what formerly had to be done by a struggle in the markets, conducive to bankruptcies and other well-known features of depressions; and this of course tends to mitigate them and to prevent many losses.

There is, second, the rhythm. Evidently if our explanation be true, we should be justified in seeing more in periodicity than Professor Pigou naturally sees in it in Chapter XXII. Similarly, the period of gestation of instruments, already adverted to by Marx and Mr. Robertson and discussed by Professor Pigou,¹ would then acquire an additional and deeper significance. And inasmuch as there are probably only few people to agree with Mr Hull's saying, that there is literally nothing in periodicity, we may perhaps point to the fact that the phenomenon of periodicity would be without difficulty explained by our theory, as not irrelevant.

The features of depression explain themselves not wholly by equilibrium being upset by new enterprise pouring forth new products at prices with which all firms cannot compete, and driving up prices of means of production beyond what they can afford to pay, nor even by the secondary waves, which it would be easy to insert in our picture. We ought to take account of all

¹ I am afraid, however, I have not quite caught his meaning. For as far as I can make out, his argument (p. 207-8) would suppose one boom to have happened, after which secondary booms would have a tendency to recur periodically. But this cannot be his meaning, as the quotation from Fisher shows, which would amply refute it. I also have to confess that I cannot get over a similar difficulty in Mr. Robertson's argument.
sorts of frictional elements in order to put the necessary flesh on the bare bones of our argument—in fact, we ought to superimpose on it the whole of Professor Pigou's mighty structure. This we cannot do and, indeed, we need not, feeling that the best we can do for the reader is to ask him to follow into all the complications of the subject an analyst so much our superior. We want to draw his attention especially to Chapters VI and VII, where Professor Pigou deals with the element of error—which acquires from our standpoint added importance by the fact that action outside of routine, and action in a situation disturbed by action outside of routine, evidently is exposed to error in a way amounting, as compared to error within routine, to a difference in kind. This particular importance of error can hardly stand out as it ought to without account being taken of the facts covered by our argument; for without them the explanatory value of error is much reduced by observing that, what looks like error after depression has set in, need not have been error at all before—so that of all the errors we think we see, a large part does not range with causes but with consequences.

But although we must discard whatever we possibly can, we yet must enter into some discussion of the part played by the machinery of credit, for what we consider the fundamental cause acts through it, and is in acting so much bound up with it, that quite essential things would be either missing altogether or going on differently if that machinery did not function as it does; and that without going into it, we could not claim completeness for our argument even in the limited sense we wish to.

§ 5. If the members of a community which has so far known no methods of payments other than the physical handing over of gold coins suddenly elect to deposit their holdings in a "bank," and to effect henceforth payments to one another by means of cheques, the managers of the bank will find that a great part of the coins in the tills will show a habit of staying with them for good—all in fact, except what may be required for shipment to other communities and, perhaps, for small payments which may still be effected by the old method. If managers feel sure of the confidence of depositors they will, therefore, be able to lend out a considerable part of the deposits and if borrowers, again, leave the sums borrowed with them and behave, as they probably would under the circumstances, exactly like the other depositors, a similar proportion of the loans granted will again be available for further lending and so on, as S. Newcombe pointed out long ago. The same thing could be shown by means of any other form...
of bank money, uncovered bank notes for instance, or bills accepted by banks and so on.

Now everyone knows that what I have said is but a way of describing what actually happens. The point to get hold of is not, of course, the mere fact of what has been called the "manufacture" by banks of means of payment, but the fact that they can and do, not by way of mistake or aberration from sound principles, but systematically and "significantly" create "credits" exceeding on the one hand the sum of savings existing and entrusted to them and, on the other, the value of commodities existing at the moment. A short footnote is all we can contribute here towards sheltering this statement from the misunderstandings to which it is exposed.¹ In fighting the fact and the important inferences flowing from it, economists seem to have felt themselves to be under a well-nigh moral obligation since the time of A. Smith. They were right in many respects. But they went much too far and now they block the very road they themselves have opened up.

There are, of course, limits to this creation of additional purchasing power: to this purchasing power, that is, which is additional to the sum of legal tender, to the sum of saving and to the sum of purchasing power represented by the value of existing commodities. They are obvious in our case of a perfect

¹ Even if banks, to begin with, only lent out what customers entrusted to them, there would be "manufacture of credit" as far as current accounts are concerned. For current accounts are as good as cash, they are cash, for the depositor. And if part of the sums paid in on current accounts be loaned to other customers, that part would be cash for these too. So there would be duplication of existing, or creation of new purchasing power, even if banks did only lend what they receive.

But this is not the case. No doubt, savings in the hand of savers or banks are, normally, the backbone of the supply of "money" in the sense of the money-market article: of mobile resources. But over and above this supply at their command, banks can and do extend their credits; and that part of the savings entrusted to them, which they can count upon for lending, is not so much the fund they have to lend, but the reserve against the sum they actually lend. I may also refer to the fact that "Bankakzepte" were in Germany, 1900-14, about five times the banknotes outstanding, or even to the methods of financing American crops before the War.

Since Fullarton's days, or longer, it is commonly held that creations of credit only correspond to the volume of commodity transactions, the classic instance being the three-months commercial paper, and that, therefore, the sphere of money and credit cannot harbour anything which could escape us, if we deal with the economic process in terms of goods only, except indeed, technical disturbances. There is truth in this, but not the whole truth. Even if strict parallelism of the volumes of "regular" banking credit and of the social product were assured—which it is not—there would still be a "non-regular" credit not displaying this parallelism, and covered neither by savings nor by goods; and without this credit, of which the old Scotch "cash-credit" is a well-known instance, business could not be carried on as it is. The question of collateral does not, of course, enter into this argument: if a loan be secured on something which is not normally meant to change hands, such as an industrial plant, the effect of the purchasing power created is the same as if there were no cover at all.
gold standard and equally present, if less obvious, in the case of any other standard. But within these limits, credit or money can be and is being "manufactured," to use Mr. Hartley Withers' or Sir J. Stamp's expression. The reason why this is possible in the sphere of money whilst it is impossible in the sphere of goods does not concern us here. I always explain it to my students by saying that whilst you cannot ride on the claim to a horse you can pay with a claim to money.\(^1\) It is for this reason too that I do not think it advisable to speak of money as a commodity or to apply the ordinary apparatus of supply and demand to it.\(^2\)

Now whenever purchasing power is created in such a way, there being no additional commodities and no reductions of money-expenditure by savers to correspond to it, prices must rise, first the prices of the commodities on which that new purchasing power is expended, later on all or nearly all of them. Parallelism between the flow of money and the flow of goods being destroyed, we have inflation, the features of which can be in the first instance best explained by the example of inflation by Government paper money. But it is a peculiar sort of inflation. Whilst Government paper inflation produces a state of things which lasts indefinitely unless remedied by a distinct and painful operation, inflation by banking credit normally rectifies itself automatically—ending normally in a process of "self-deflation." Business men apply for credit to banks, they spend it on the markets of the "factors of production"; as far as the sums so spent have been newly created \(\text{ad hoc}\), the existing money demand increases, therefore the prices of labour and so on rise, and incomes of workmen, owners of natural agents or of "capital-goods" will increase in consequence;\(^3\) and prices on the markets of articles of consumption will rise too, the process going on until enough means of production have been, by the rise of prices, wrung from those firms which had

\(^1\) It may be replied that you could occasionally discharge a debt with your claim to a horse. This is true but off the point. It is no part of the function of horses to serve as means of payment, but to do so is the only function of money.

\(^2\) The question of limits of "created" purchasing power is of great interest. But here I can do no more than disclaim any sympathy with the exaggerated statements sometimes met with. Although not quite agreeing with everything in Mr. Crick's paper in the June number of this journal, I yet need not ask the reader to grant more than what he finds there.

\(^3\) There is, therefore, nothing in the general theory of this process which would warrant any general assumption of wages lagging behind if we mean by "wages" the sum total of real wages. Cases of such lags in wages must be dealt with as they arise on their individual merits.

This is, although in a lesser degree, true even in case of the quantity of money being increased by an increase in the production of gold as pointed out by Professor Pigou in his masterly paper on Prices and Wages from 1896 to 1914. It is still less true, and even not true at all, in the case of the issue of Government paper money.
been in the habit of buying them to satisfy the additional demand of the newcomers at the new prices. Now there would be no sense for newcomers (including people already in the market but desirous of extending their business) to borrow in order to buy means of production in markets rising against them, if they only intended to produce what is already being produced by the same methods. For if we start, as for clearness sake we must, from a state of perfect equilibrium, it will be readily seen that they could, by doing so, never earn the interest they have to pay: they could indeed earn the capital sums borrowed, if prices of products rose in proportion, but under competitive conditions, in the absence of friction, and production having already been everywhere carried up to the margin, never more than that. But if they happen to be those innovators we met with in the first part of this paper, then things are different. By means of “new combinations,” the same flow of quantities of factors of production which had been regularly bought and used before by other people may now be used to greater advantage and may produce what will not only fetch a sum equal to capital and interest but normally also—as long as competition has not caught them up—profits. Sooner or later, therefore, if things go right, they will be able to pay back what they borrowed with interest, which is synonymous with saying that the “created” purchasing power will automatically eliminate itself.¹ This we mean by “self-deflation,” which then comes about, first, by the new products appearing after a time on the markets of articles of consumption—the nature of the thing is best seen if we imagine the improvements to consist of new methods by which more of a product already produced before can be got out of the same quantity of labour and natural agents—and, secondly, by the repayment of loans, thereby re-establishing the parallelism previously destroyed by the “creation of credit.” There will be, in fact, more than compensation of the previous inflation, as I perhaps need not stay to show. And this is indeed the true explanation of the secular downward trend of prices during the period of Capitalism, only partially and occasionally offset by the vagaries of the production of gold.

§ 6. I have now to submit two theorems which have been

¹ Perhaps I ought not, even when writing for English readers, to be so short as this. Of course cases must be very rare in which the whole capital could be repaid out of earnings within one boom; but the argument is not altered if we extend the period of amortisation, nor by the fact that repayment to the bankers or financiers takes the form in the first instance of saved up capital stepping in to relieve the banks in the form of the taking up of shares for instance, nor finally, by what I should have to say, if I had space, to fit the theory to some of the peculiarities of the English financial system.
since 1912 and are still the objects of many attacks in my country:

First, in a perfectly static state, there would not be this scope, barring Government inflation, for the creation by banks or other agencies of such purchasing power. There would not be even the possibility of it for, as shown above, there would be no demand.¹ The process of production could and would be financed by previous takings and only small and relatively unimportant discrepancies, properly included among "frictions," would occasionally have to be smoothed over by banks. It is only the fact that society is not "static," that the industrial and commercial process is always being reorganised and revolutionised, that accounts for the phenomenon of a sort of money which is indeed still a "ticket"—in J. Stuart Mill's sense—admitting holders to the "national heap" of goods, but not or not yet also a "certificate" representing productive service rendered. Although the device once evolved will then serve many purposes, it would never have been evolved without the innovators', the entrepreneurs' demand for mobile resources, which always remains its raison d'être.

Second, just as in strict theory there is no other demand for the creation ad hoc of purchasing power but that of the entrepreneur, there would in strict theory be no other sources from which to satisfy it but such creation. It is not so in practice because that constant revolution of industrial and commercial methods is constantly yielding profits, the first, most natural and most important source of "mobile resources" or of "savings," which however would not exist in a static state, from which we have to start in order to avoid explaining things by what are their consequences; nor would there be in a static state nearly as much motive to save out of other resources besides profits as there actually is. The analytic value of this proposition does not depend on what important or unimportant rôle "creation of credit" may play in a given country at a given time. Situations are possible of great wealth and little activity in which this rôle would be nil. To point to such, or nearly such, situations would be easy but irrelevant.

But as the innovators' or entrepreneurs' demand for credit could not, in the highly abstract case we are considering, be met by other resources, so it always could be met by this. That is to

¹ It might be objected that there would be always something to be gained by inflation. This is true of inflation of the Government paper pattern but not of banking credit under competitive conditions, under which no producer and no banker could, by his own single act, raise the level of prices, and if they cannot do it the producer would ex hypothesi be producing what he could only sell at a loss and the banker would risk insolvency.
say, saving, though still of primary importance, turns out to be
a shade less important than one would think. "Mobile resources"
are not necessarily the result of previous saving, just as economic
progress is not primarily the result of an increase in factors of
production, but the result of applying the quantities of them
already existing to ever new ends and by ever changing methods.
As we have seen, this is done by withdrawing them from the uses
they are serving and the persons who manage them, in order to
hand them over to those who will use them better, by means of
purchasing power created in favour of the latter and of a conse-
quent rise in prices which cuts down the demand of the former. It
remains true, as Ricardo knew, and as we do not deny, that no
wealth can be created by "banking operations." It even remains
ture, although in a sense not quite natural, that productive forces
must be saved before there can be new production. But those
"banking operations" are an important device for bringing about
a better arrangement of productive forces; and if saving there be
it is not the usual sort of saving, but what we may term "forced
saving" (erzwungenes Sparen).
§ 7. It is not possible here to unfold all the applications by
which this analysis lights up many points of the theory of money,
credit, interest and other matters, which cannot, I submit, be
dealt with satisfactorily without it. I only wish to show or
rather hint at how it links up with the theory of the cycle.
The periods of prosperity or booms being the periods in which
"innovations" in, or reorganisations of, the productive process
are mainly taken in hand, they consequently are the periods of
creation of new purchasing power as, in fact, is shown by statistics.¹
This, and not simply fluctuation of the "K" in the Marshall-
Pigou-Keynes² formula, accounts for the rise of prices in every
boom, which could hardly be explained otherwise. There may be
a lag because of the presence of accumulated stock, and a rise in
articles of consumption before a rise in the rate of wages, because
of the presence, at the beginning, of unemployment. This is why

¹ It is true that statistics are more likely to show more secondary phenomena
such as the issue of shares and so on; but as everyone knows who has seen these
things being done, no big issue is ever made without the help of purchasing power
created ad hoc.
² Mr. Keynes is, however, quite right in emphasising the importance of the
movements of "K" in dealing with the post-War situation, with which we are
not here concerned. Much must, moreover, remain unsaid in a sketch like this,
but I want to mention (1) that movements of "K" can, and often do, accentuate
or mitigate the effects of credit-creation; (2) that movements of "K" may, and
often do, effect what otherwise would have to be effected by credit-creation; (3)
that movements of "K" may sometimes technically enforce and so cause credit-
creation, whence a very complex tissue of mutual interaction.
the Spiethoff index is so much better than some others, but this does not alter any fundamentals.

The periods of depression, being typically the periods in which the changes in the productive organism, especially those embodied in new industrial plants which now have got into working order—the theoretical turning-point—begin to make themselves felt and to exert their pressure on the rest of the community, are consequently periods of deflation. This explains the downward movement of prices we observe. It is, first, deflation of the sort we have been describing as self-deflation. But it is, naturally, aggravated by what I may term autonomous deflation by frightened banks, who not only see and expect difficulties arising with their debtors but also anticipate difficulties with depositors and sources of rediscount. Here, too, there may be lags through producers trying to keep prices up and through frozen credits defying the endeavours to contract them, but here, too, this does not affect the basic argument, although it very much does affect the situation.

No more need be said about the function which this movement of general prices—the upward one as well as the downward one—actually fulfils. It is clear enough. Nor need we stay to explain how far and why we are unable to accept Mr. Hawtrey's dictum by which he so gallantly exposed himself to attack, viz., that the cycle is a "purely monetary phenomenon," which most undoubtedly it is not. We rather think it our duty to explain how far we do agree with him.

We agree with him, first, in recognising that the fundamental cause, whilst in its nature independent of the machinery of money and credit, could not without it produce the particular kind of effects it does. Booms and consequently depressions are not the work of banks:¹ their "cause" is a non-monetary one and entrepreneurs' demand is the initiating cause even of so much of the cycle as can be said to be added by the act of banks. But booms

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¹ Nor is it in their interest to favour them. There is much misunderstanding about this. Of course a boom affords the chance of unloading such investments as banks would rather be without or any investments at great profit; there are many similar sources of gains, differing very much in importance in different countries. But the banking business itself would probably prosper more under stable conditions than it does in conditions in which the higher rate of interest and the lucrative deals of booms are constantly being offset by the slackness of business and by the losses in crises and depression. As a matter of fact I think I have observed that the banker of high standing does not relish booms particularly, and that he actually does something towards stopping them within his own sphere.

But whilst the banks, if acting in concert, could probably keep down any boom, they cannot be said to have the power of entirely preventing depression. The mere "injection" of credit is powerless to do so, even if it were possible; but producers will simply refuse credits at ever so favourable conditions, in certain circumstances, as shown by post-War experience in many countries and as is rightly pointed out by Professor Gregory.
and depressions would not without banks be what they are, and it remains utterly misleading to say, as has been said ever since Fullarton, that banks are only following the lead of demand and unable “to force their money on people”; for it lies with them to satisfy this and to create additional demand in a sense quite different from the sense in which it would be true of sellers of a commodity: the latter acting under the pressure of cost which is absent, within limits, in the case of the former.\(^1\) So there is for banks a range of freedom of action to which nothing corresponds in other branches of business: this range would exist even without national or international understandings which, however, powerfully extend it.

It follows—which is indeed a second point of agreement—that banks can and do, even without knowing it, exert influence on the pace of prosperity and depression, although, for the reason given in a footnote, more on the former than on the latter; and they do more than this. They not only finance innovators’ or entrepreneurs’ demand, but also the demand of other people, who simply want more credit because they see prices rise. They are even specially willing to give in to those people, for they are their old customers. Hence, they help the coming up of a secondary wave of the boom to which, although it also increases forced savings, it is impossible to attribute the function of the “primary wave.” Other waves may and often do follow, and among them the great wave of mere speculative punting, all of which makes prices rise still more.\(^2\) It is these things which make up the physiognomy of both boom and depression, and which we have looked to when warning the reader not to judge our theory merely from what we said in the first part of this paper. They are, in fact, the bridge which leads from what we consider the keystone to the complexities of the “real” phenomenon. Now I believe that it is these things, too, that Mr. Hawtrey—and Mr. Bellerby still more—has had primarily in mind. And as to them we, within wide limits, agree with him, just as we think that we could get him to agree in some measure with our view of the initiating impulse, if indeed he were so unfortunate as to fall into our hands.

Third, we also agree as to the practical possibility of stopping any normal boom by a proper management of credit. It may be

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\(^1\) There are, we repeat, limits other than cost, and there is also cost, but there are no additional prime costs incident to, say, the carrying out of a decision to be content with a smaller reserve-proportion and to extend loans and advances correspondingly.

\(^2\) In this sense it is true that according to a saying of Marshall’s, prices rise because they have risen and conversely in depression. It is also true that gains and losses consequent upon this have no, or nearly no function.
difficult, and discount policy may be insufficient to effect it except in quiet times. Into this we cannot enter; but it is surely possible. This does not imply indeed that it is in any sense desirable. But inasmuch as I am strongly under the impression that these discussions and the theoretic views of the parties to them are influenced by views held as to policy, and that such views are suspected to be at the bottom of every theory pronounced, I am most anxious to say that I do not wish to advocate or fight any policy whatever. And although I feel debarrled from entering upon questions of desirability of measures by the purely scientific character of my argument—the mixing up of which with practical policy I should indeed look upon as a misdemeanour—I still wish, in order to appease suspicions, to say that if I think that the cycle cannot be successfully held to be merely an "evil," serving no social interests whatever, I do not mean thereby to imply that it is to be complimented on the way it fulfils what we have seen to be its "function." It may well be argued that it does its work at very great costs, that these costs might be saved or reduced by proper arrangements and that a policy of keeping the level of prices stable might do but little harm to improvement, while greatly reining-in secondary phenomena which are universally (or nearly so) felt to be evils and are the main source of error, losses, unemployment, and so on. Some slackening down of improvement might even be held to be no more than a reasonable price to pay for benefits such as these. Without receding from our protest against using post-war phenomena when discussing points of general theory, we may still point to the instance of recent American experience as a proof that booming activity is

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1 If the efficiency of discount policy has often been grossly exaggerated it has been also sometimes underrated. Thus by writers using the argument that so small an alteration of conditions of production as is implied in the rate going up by, say, 1 per cent., must be powerless to put an effective brake on activity in a boom. This is never quite true, not in quiet nor in disturbed times. In quiet times, when money markets function properly, there is a class of speculators who act on the basis of comparison between the yield of shares and the rate they have to pay for loans. This kind of operation is calculated to a nicety and a rise of even ½ per cent. may turn it the other way; and from this point of attack, insignificant as it seems, effects of restrictive credit policy may expand to reach much farther than one would think. It is this point, too, which well-nigh immediately acts on the interest of Mr. Robertson's "long lacking," for first, many long-time purposes are being directly financed by short-term money, and secondly, interest of long-term investment is being indirectly affected because long-term securities and shares usually financed in part by short credits will be offered and, therefore, the interest they bear raised, by the class of speculators mentioned. In disturbed times such as war booms small changes in the rate of interest do not act like this; discount policy is really an instrument only for fighting disturbances which are small and do not last too long. But still, the effect of raising the bank-rate is seldom quite lost, the business community knowing that if a small rise would prove ineffective, larger ones would follow, and, moreover, although no central bank could very well go up to, say, 50 per cent., other sources of credit can and do.
quite possible without booming prices. This is no problem for us, nor any instantia contraria against us. Having said this much, we can venture to say, without danger of being misunderstood, that Mr. Bellerby seems to us to have injured an admirable argument by over-statement and by failing to distinguish sufficiently "normal" and post-War phenomena. But in our argument there is nothing to get on the nerves of our money-reforming friends.

§ 8. Nor is there anything in it to get on other nerves, viz., on the sound-money nerves of those to whom the very word of "credit-creation" is abomination, savouring as it does to them of old popular fallacies on the one hand and of advocating inflation on the other. We respect all theoretical, political and moral views implied. We do not stand for John Law. We do not advocate anything, but least of all inflation, although we do draw a distinction between inflation which does, and inflation which does not, automatically "deflate" just as, without advocating the use of morphine, we draw a distinction between the use of morphine for mitigating pain and the use of morphine for the pleasure of it. We admit that the term "credit-creation" is open to objection. Any term is. But we are doing nothing but analyse patent and undeniable facts. And as they seem to us important, we should now try to convince an authority so very highly respected by us as is Professor Cannan of the fact that what seems startling in an over-simplified and at the same time highly abstract argument like this may still be perfectly compatible with sound (theoretical) conservatism, which we value very much ourselves.

But we need not do it, for Professor Pigou has undertaken the task (Chaps XII, XIII, XIV). It is a very great pleasure to us to be able to state that we find ourselves in perfect accord with him on important points of this important subject. I have explained as best I could, and it was indeed but ill, part of what I wrote in 1912. But I should probably have served my purpose better if I had started from Professor Pigou's chapters or from Mr. Robertson's powerful argument in his book on Banking Policy and the Price Level. And I think that by these chapters the new

1 I usually say "creation of purchasing power" (Kaufkraftschaffung).
2 Priority of publication of the essential points seems to be Mr. Robertson's, although there are hints at them in Professor Pigou's contribution to the volume, Is Unemployment Inevitable? which appeared in 1925. But in his article in the Economic Journal for June, 1926, Professor Pigou made himself the interpreter of Mr. Robertson's views in such a way as it is hardly given to Man receiving truth really new to him.

Now I think Mr. Robertson's book a most original, fruitful and suggestive performance, making a new departure which may lead very far. And it is a most amiable book, sincere to a degree, never slurring over perplexities, never trying
theory of credit has come to stay—not the facts, from which it starts, for they were always known and made use of in the limited sphere of money and banking, but the theory of them as a necessary part of the general theory of the economic process.

There is, first, the recognition of the importance of credit-creation and of the theoretic issues raised by it. It is seen to be a "levy" by Professor Pigou and as producing forced savings, or, as Mr. Robertson says, "imposed lacking." Many consequences, explaining much of what has been hitherto looked at as inexplicable deviations of real life from theory, are immediately drawn.

Both authors, but Professor Pigou especially, go on, secondly, to explain points of detail not only of technique but also of theory, and, in so doing, already leave the present writer far behind, who never so much as attempted to touch some of them,¹ such as the question of how large a real levy banks will achieve—the first of the three problems distinguished by Professor Pigou in his paper quoted in our footnote.

But there is more than that. In propounding the thesis, in §§ 4-6 of Chapter XIII, that any year's addition to bank deposit, is, subject to qualifications, a rough index of the quantity of bank credit for "industrialists," Professor Pigou comes near to one essential element of our argument. And Mr. Robertson in his book, and Professor Pigou in his article dealing with it (Subproblem the third) hold that, "sudden additions to the supply of circulating capital can, in fact, only be obtained through the creation of new money by the banks." This is, to be sure, only meant as a result of analysis interesting in itself and practically important as it stands, and must not, of course, impute to those to be original—although being it—strictly fair to its problems and to co-workers. I, for one, find it easy reading, although I do not quite like its terminology in every particular, especially where it implies approval or disapproval of facts analysed. I may as well say at once, that I also admire his thought for depth more than his technique for elegance. I should not indeed quite feel at ease if I had to answer Mr. Harrod's attack on him in the last number of ECONOMICA.

Finally, I hope Mr. Robertson will not take it unhappily from a sincere admirer of his most valuable gifts to science, if this admirer ventures to say that Mr. Robertson, whilst negotiating his hurdles most neatly, has a way of stopping dead after the jump: He does not, I thereby mean, really take hold of the points he makes. This explains, I think, that, although we find all elements needful for a complete theory in the pages of this book and of other publications of his, the constructional trades and the period of gestation and so on, on the one hand, and "imposed lacking" and so on, on the other, they do not somehow work up into a whole. Even the "secondary cases" are there and still there is no getting hold of the cause of the primary ones. But the book is nevertheless full of new truth and its argument emerges, I think, entirely unscathed from Mr. Hawtrey's criticisms levelled at it in the Economic Journal for September, 1926.

¹ He begs leave, however, to refer to his papers: "The Control of Credit" (Kreditkontrolle, Archiv. f. Sozialw., 1925) and "The Golden Brake of the Machine of Credit" (Die goldene Bremse der Kreditmaschine, Kölner Vorträge, 1926).
eminent authors any tenets of mine. But the connection between statements such as these and my theory of the cycle is obvious—it is even obvious that creation of new money must have something to do with new transactions, and these with "innovation." And ἐσσεραὶ ἡμῶν ὅταν, the positions so taken up will shade off into those I have been trying to sketch out.

If this should happen there will be, I think, reason to expect some repercussion on the theory of interest;¹ and other points may then be found to need readjustment, if we are to have, one day, a really satisfactory analysis of the capitalist process. But I do not now want to add to what, as it is, seems to me a stroke of temerity.

¹ I even think I see some signs already. Thus there is much more than occurs at first sight in Mr. Hawtrey's statements (The Economic Problem, p. 221), that "capital is accumulated mainly out of profits" and that "in so far as capital is used in business, the interest upon it is paid out of profits." If we link this up to a theory of profits different from Mr. Hawtrey's, and take account of the element of credit creation, we might be able to advance considerably beyond the positions held at present.