

**JOSEPH AGASSI, THE M<sup>2</sup>T SEMINAR,  
AND HIS INFLUENCE ON MY WORK**

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## ABSTRACT

This paper discusses the influence on my research and writings of several methodological principles that we, the members of the LSE Staff Seminar on Methodology, Measurement and Testing learned directly from Joseph Agassi and indirectly from Karl Popper. It begins with the origins of the seminar and my text book, *An Introduction to Positive Economics*. It goes on to cover methodological issues that arose in my subsequent papers including: the importance of having empirical content in economic theories, the poverty of theories that are built only to pass sunrise tests, why non-robust assumptions need to be tested, the concept of refutability, the fussy distinction between normative and positive statements, the impossibility of giving purely positive policy advice, the testing of existential statements, fallacious attempts to deduce empirical propositions from definitional identities, the distinction between internally and externally driven research programs, the poverty of modern welfare economics as a guide to policy and the possibility of deriving policy advice without such guidance. It concludes with a short discussion of the revolutionary implications of accepting technological change as being generated endogenously under conditions of genuine uncertainty rather than measurable risk.

I am a professional economist with only a rudimentary knowledge of some basic methodological principles. Most of these I learned from Joseph Agassi, either directly or through his friend and my colleague, Kurt Klappholz. I never met Karl Popper but, through Joseph, we were also introduced to many of Popper's ideas. All of this happened through the vehicle of the LSE Staff Seminar on Measurement and Testing in Economics that became known as the M<sup>2</sup>T Seminar. In it we half dozen founding members, as well as several later additions, learned much from each other as we sought to apply what we were learning about methodology to our own work in economics. Since I have nothing new to offer by way of methodological insights, I have been asked to discuss how Joseph influenced my subsequent work. I hope to persuade you that his influence was not insignificant.

## I. M<sup>2</sup>T AND AGASSI

I have told in several places the story of how I became one of the founding fathers of the M<sup>2</sup>T Seminar. (See in particular Lipsey 1997.) So I can be brief here.

As an undergraduate in honours economics at the University of British Columbia in the late 1940s one of the prescribed books was Lionel Robbins' *Essay on the Nature and Significance of Economic Science*. It impressed me greatly until I came to his discussion of the place of empirical data in economics. There I read: "The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and *indisputable* facts of experience....(1935, 78 Italics added). If the premises relate to reality the deductions from them must have a similar point of reference" (1935, 104). I was surprised at reading this and thought that it could not be right. Surely, I thought, scientific truths (as I then thought of them) could not be arrived at by mere introspection based on personal experience; empirical observation should play a more important role than just illustrating propositions that intuition tells us are true.

In 1953 I entered the LSE as a PhD student. I attended Lionel Robbins' great Wednesday afternoon seminar first as a student and then from 1955 as a staff member. My contemporaries and I admired Lionel for his many great qualities, but we found ourselves increasingly dissatisfied with his methodology. Assessing theories by whether or not their assumptions appealed to our intuition seemed increasingly unsatisfactory. We cast around for an alternative method of assessing the increasingly complex theories to which we were being exposed. A small group of us met informally for a year to discuss matters of methodology. One of our original members, Kurt Klappholz, introduced us to Joseph Agassi or Joske [?] as we came to know him. Joseph in turn introduced us to Popper's works. Although Popper was on the LSE staff, being allergic to cigarette smoke, which the young staff produced in quantity, he never came to the Senior Common Room and was unknown to those of us who did not already have a knowledge of formal methodology. After a year, we decided to create a seminar devoted to the new (to us) methodology of testing and refutation that we had discovered under Agassi's tutelage, and so M<sup>2</sup>T was born.

The primary methodological influences on we economists were Popper, as seen through Agassi's eyes, and Agassi's own many insights, as well, a bit later on, Imre Lakatos. Although many of Lakatos' ideas have been criticized, I always found the concept of an untestable core and a testable protective belt helpful and used it in my essay "IS-LM, Keynesianism and the New

Classicism". To Joseph, more than any other single person, we owe our conversion from the Austrian methodology as expounded by Robbins to a view that the mark of a good theory was its ability to pass tests where there was a real change of finding conflicting evidence.

Although I am not a methodologist and have not followed the modern controversies among that profession, what we learned in M<sup>2</sup>T times has helped me enormously in subsequent work and although there are no doubt many areas in methodology where I am uninformed, I believe that if modern economists knew as much methodology as Joseph taught us, their work would be much more productive than it often now is. In the remaining part of this essay, I will illustrate this by noting a number of methodological ideas that we mainly learned from Joseph and that I have put to what I hope is good use.

## II. AN INTRODUCTION TO POSITIVE ECONOMICS

In 1960, fired with the enthusiasm that I had developed through the M<sup>2</sup>T seminar, I decided to write a text book that would expound our ideas in contrast to those that certainly prevailed in the UK, and probably in many other places as well. At the same time, I volunteered to teach the first year economic course in LSE's newly revised BSc Econ degree; so the teaching and writing complemented each other. It should have taken me about two years from start to publication but, as discussed below, I ran into the problem of the Keynesian identities, which delayed me for about a year. The first edition was published in October 1963 and ran to only 559 pages. I was still working on the index when the limousine called to take us to Southampton where my family and I were sailing for a sabbatical year as visiting professor at UC Berkeley California. I called the book *An Introduction to Positive Economics* partly in an allusion to positive versus normative and partly to emphasise testability. It was known as *IPE* for years and only latterly as 'Lipsey'. Some economists misconstrued the title as indicating that I was a logical positivist, which I certainly was not, and I did write at the time an essay (Lipsey 1964) explaining my main objectives in writing the book.

The IPE's introduction began with a long chapter on methodology, stressing the importance of testing and evaluating theories according to their empirical relevance rather than their intuitive appeal. It also dealt with many of the questions that had plagued my students earlier, such as: How can we have a science concerning something so erratic as free-will based human behaviour? The microeconomics half was divided into four parts. Part 1 stated such basic material on how competitive markets worked that I did not think a chapter on criticisms and tests was called for. So I ended instead with a chapter on applications which showed how useful the theory was and how well it stood up to being exposed to data in its everyday use.

The other three micro parts, the theory of demand, production and distribution, covered material that was open to debate in places so I ended each part with a chapter on criticisms and tests. The object was to show students that what they learned was not necessarily absolute truth and might be replaced by better knowledge in the future—this in contrast to the revealed-truth approach of many of the then-current text books. In the chapter on the theory of demand I took on Lionel Robbins' contention that economic measurements were of little use to economists other than to record transient historical events. I have discussed this issue in greater detail in Lipsey (2009) an essay written to commemorate the 50<sup>th</sup> anniversary of the publication of *The Nature and Significance*". The macro half of the book dealt with Keynesian theory, but devoid of the then ubiquitous definitional identities, about which more below. It ended with two chapters on a recurring interest of mine, the relation between economic analysis and economic

policy. The book was an instantaneous success, being reprinted 5 times in its first three years, being translated into over 15 foreign languages, and becoming an established text in several places beyond the UK, such as Spain, Italy and India.

The book was so different from any existing text that I am sure it would never have succeeded in today's world where the course lecturer, or a committee of his peers, selects a text and students are told to buy it. In the 1950s and 1960s the typical UK instructor would give his own set of lectures and tell students at day one: "There are several good textbooks around and you should complement my lectures by reading the text that best suits your needs; here is a list of some of the good ones." So when I wrote an unorthodox book, students could, and did, vote with their wallets to make it a market leader within months, long before some instructors had opened its covers. Indeed it was published some weeks after the beginning of the 1963-4 academic year, which would spell certain death for a modern textbook but which saw its first printing selling out before the end of that academic year.

The UK edition evolved along the lines I had established for the first five editions but came under increasing market pressure from adopters as the selection system for textbooks superseded the previous textbook anarchy. Under relentless market pressure, about which I am not proud for succumbing to, the chapters on measurements and tests were dropped. The methodology chapter was slowly downgraded on the grounds that much of it was now old hat, rather than revolutionary. However, when I see some of the methodological confusions among today's economists, I am not sure that this was good advice. At the 8<sup>th</sup> edition, the publisher was changed from Weidenfeld and Nicolson, to the Oxford University Press and a co-author, Alec Chrystal, was added. Starting with the 9<sup>th</sup> edition the book was re-titled *Economics*. The 13<sup>th</sup> edition was published in 2015. The American edition, originally co-authored with Peter Steiner was always titled *Economics* and went through twelve editions. Although this did not have the same sweeping success as did IPE, it did do quite well. I recall getting a gold bound edition commemorating the one millionth copy sold and it certainly made my American co-author rich.

I think the first few editions of IPE did have a major influence in persuading at least a generation of non-American students of the value of adopting a fact-controlled view of the subject; that nothing in economics is revealed truth, including the virtues of a market-oriented system, but is subject to alterations in the face of new conflicts with the facts; and that nothing could be learned about the world by manipulating definitional identities. It is not for me to evaluate these claims of influence. But anyone who does try should not be misled by the fact that the later versions of *Economics* are not radically different in approach from its many modern competitors. It is the first four or five editions that need to be studied to see the nature of the non-orthodox approach and to assess its impact.

Be that as it may, I could never have written *IPE* without the inspiration provided by all of the members of the M<sup>2</sup>T seminar, and the lessons in basic methodology that I got from Joske, either directly from him, or indirectly from the others.

### III. METHODOLOGICAL APPLICATIONS

Not surprisingly, since Robins had made intuitively obvious assumptions a major component of economic theory, we spent much of the seminar's first year considering the whole issue of the place of assumptions in economy theory.

## The Primacy of Popperian Predictions Rather Than Robbinsian Assumptions

Early on we came to what was to us a great revelation: a theory that was consistent with all states of the universe was empirically empty and so could not explain anything. One aspect of this understanding was that a theory that included an unspecified set of *ceteris paribus* conditions was empty, since any apparent refutation could be explained by one of the items on the (unspecified) list having not remained constant. The economic journals are replete with models that are consistent with all possible observations and a great improvement in relevance would be achieved if editors asked, before publishing any theory, that its author explain what set of observation would conflict with the theory.

Here is a case in point. Not long after our group began our methodological discussions, Kurt Klappholz asked me if I remembered the two of us criticising a student's theory because it could not explain (was not consistent with) some observations that we conjectured. So we were criticising him because his theory was not consistent with all conceivable observations and hence not empirically empty. It was an enormous step for all of us from thinking that a good theory had to be consistent with all conceivable observations to accepting that the more conceivable observations with which it was inconsistent, the more its empirical content. (I still remember how resistant we were when Joske first made such a statement.)

Closely related is the issue of sunrise texts: developing and testing a theory whose only predictions are already well known observations. This is one of economists' favourite occupations. The concept of a stylised fact is relevant here: an alleged 'fact' that captures in generalised form some commonly made set of observations. Having stated one such 'fact', theorists often set out to build quite elaborate models that predict that fact and nothing more. We learned from Popper that building a theory that only passed sunrise tests did not add to our understanding of the world. Of course, if the theory also produces some other predictions, either explicitly, or in the nature of some of its empirical assumptions, knowledge can be advanced by testing these. But all too often such added bonuses are non-existent. As the editor of the *Economic Journal*, wrote in his retiring essay (Hey 1997: 4):

"It often appears that the model has been constructed for no other purpose than to produce a result which is a stylised fact observed by the author. Simply producing a model that comes up with a desired result is a test of the cleverness of the author not a test of the relevance of the theory."

In all subsequent work my co-authors and I have been careful to ensure that any theory that we propounded made predictions that were capable of being confronted with conflicting observations.

## Testing of Assumptions

Early on we had to consider Milton Friedman's (1953) essay on "The Methodology of Positive Economics", which is the only material on methodology that many American economists read. Critical in this context was Friedman's contention that only a theory's predictions, and not its assumptions, should be subject to test. After long consideration, we rejected that contention and much of the rest of Friedman's methodology. (For details of our collective views see Klappholz and Agassi (1959).) Mark Blaug, who joined the M<sup>2</sup>T seminar not long after its inception, has criticised "...the license that Friedman's 'methodology of positive economics' gave economists to make any and all unrealistic assumptions, provided only

that their theories yielded verifiable implications. (Blaug 1998: 20)<sup>1</sup> Since one of the key purposes of behavioural assumptions is to link a theory's predictions to observable behaviour, the view of the extreme irrelevance of assumptions reduces theory to mere operationalism. It then does not make sense to ask which of the assumptions is causing the trouble if a theory that seemed to accord with the facts no longer does so.

I argued in several places that "...if empirically correct predictions were deduced from a set of empirically false assumptions, this called for further serious study, not complacency." (Lipsey, 2008,526).

But can we test behavioural assumptions? Here is an example that I have used in several places. (See for example Lipsey 2013.)

"It is commonly assumed in micro economics that firms have positively sloped marginal cost curves. Statistically estimated cost curves show, however, that many such curves are often horizontal. It makes sense to ask what caused this difference between the assumption and the empirical evidence. It is then found that the assumption is in fact derived from two prior ones: that the law of diminishing returns (or variable proportions) holds and that the fixed factor is subject to an equality constraint so that the firm must use all of it as output is varied over the short run. If so, the ratio of the fixed to the variable factor must vary as output varies, causing costs to vary according to the law. But evidence shows that in many (possibly most) firms the fixed factor is subject to an inequality constraint so that the firm can alter the use of its fixed factor up to its maximum capacity by leaving some it unemployed. In this case, the use of the fixed and variable factors can be altered as output varies, allowing the ratio of the two to be held constant at its optimal value and so avoiding the conditions under which the law holds.

Consider what we have done here. We started by investigating a commonly made assumption about the shape of a firm's cost curve; one that is often used in both micro and macro theorizing. We found that the evidence was in conflict with the assumption. When we sought the source of the problem, we found that this assumption was in fact a prediction derived from two prior assumptions and that one of these, that the fixed factor was subject to an equality constraint, was often not correct. Now we have learned to be cautious of any theory for which the shape of the firm's short-run cost curve... [plays a critical part]."

## Robustness of Assumptions

In his discussion of assumptions, Friedman did not recognise the several different senses in which assumptions are used in economics and the different implicit ways in which they should be assessed. Some assumptions are merely designed to simplify the problem without altering it in substantial ways. For example, the neoclassical theory of production takes place on the head of a pin, in which spatial location does not matter and in many actual cases it does not. But if we are considering competition among retail establishments, their locations in relation to each other matters a great deal and needs to be introduced into the theory if it is to cover variables that matter. (For a full study of these issues see Lipsey and Eaton 1977.) These two cases illustrate the important requirement that assumptions that are patently counterfactual be robust in the sense that they can be relaxed without seriously altering the theory's predictions.

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<sup>1</sup> For a full discussion of this issue from an economists point of view see Blaug 1992: 191-205)

A modern example of this issue can be found in the literature on what are called General Purpose Technologies, GPTs. My co-authors and I have defined this concept as follows: *A GPT is a single technology, or closely related group of technologies, that is widely used across most of the economy, is technologically dynamic in the sense that it evolves in efficiency and range of use in its own right, and is an input in many downstream sectors where those uses enable a cascade of further inventions and innovations.* (Bekar, Carlaw and Lipsey 2016). Early on, the introduction of one of these GPTs, electricity, was shown by Paul David (1990) to be associated with a slowdown and then an acceleration of productivity growth. It soon came to be believed that this macro sequence of slowdowns followed by booms might be a general result of the introduction of all new GPTs. Several theories were developed to produce that result, usually using patently counter-factual assumptions to do the job. (See for example Aghion and Howitt (1998) and Helpman and Trajtenberg 1998a and 1998b.) These assumptions were not robust because removing them removed the phenomena being considered. Later theories (especially Carlaw and Lipsey 2011) were constructed with assumptions that captured as many as possible of the characteristics of these technologies and then asked if the patterns of slowdowns and booms emerged as predictions. In these theories, the pattern occurred only sometimes, thus raising the interesting question of what were the circumstances that led to this pattern of macro behaviour, a question that could not be studied in theories that were expressly constructed always to produce that behaviour.

### Naïve versus Conditional Refutations

Early on many of us became naïve falsificationists. (I like to think that I was not one but cannot point to printed evidence and so must accept that I was probably one these.) Of course theories in the natural sciences do for all intents and purposes get refuted. For example, the evidence against that biblical account of the age of the earth and the simultaneous creation of all species is refuted by an overwhelming body of evidence. But such finality is rare in the social sciences. Whatever we now believe, there is always the possibility that new evidence, or a new interpretation of existing evidence will upset one of our currently cherished beliefs. Where we have conflicting theories the best that can usually be done is to assess the balance of evidence for one theory over the other, and where there is only one theory the test can always be against the hypothesis that the behaviour in question is determined by a random process. One of our founding members, Chris Archibald, wrote a valuable article reflecting much of our thinking on this issue (Archibald 1967).

### Two Alleged Dichotomies

I had been raised on two dichotomies that Joske taught me to look at in a more nuanced way.

#### *Normative and positive statements*

The division between normative and positive statements was one of the great early simplifying distinctions in my life. But under Joske's persistent criticisms I came to see that this distinction was not as clear as I had thought at first. Eventually I came to the conclusion that I stated in the 3<sup>rd</sup> edition of *Positive Economics* (1971, 5, n.1).

“Having grasped this distinction, the student must beware that he does not turn it into an enquiry-stopping, dogmatic rule. From the fact that positive economics does not include normative questions (because its tools are inappropriate to them) does *not* follow



that the student of positive economics must stop his enquiry as soon as someone says the word ought. Consider the statement: 'It is my value judgment that we *ought to have* rent control because controls are *good*.' Now it is quite in order for you as a practitioner of positive economics to ask 'Why?' It may then be argued that controls have certain consequences and it is these consequences which are judged to be good. But the statement about the consequences of rent control will be positive testable statements. Thus the pursuit of what appears to be a normative statement will often turn up positive hypotheses on which our *ought* conclusion depends....

Philosopher friends have persuaded me that, when pushed to limits, the distinction between positive and normative becomes blurred, or else breaks down completely. The reason for this is that when examined carefully most apparently normative statements reveal some positive underpinning (e.g. 'Unemployment is worse than inflation *because* the (measurable) effects of unemployment on human beings are judged by the majority of adult citizens to be more serious than the (measurable) effects of inflation'). I remain convinced, however, that, at this stage of the development of economics, the distinction is a necessary working rule the present abandonment of which would contribute more to confusion than to clarity. The justification for this view is that although we are not sure what to make of an apparently normative statement (because it may have a positive underpinning) we do know a purely positive statement when we see one."

### *Knowledge-based Advice and Value-judgment-motivated Decisions.*

Another of the dichotomies that were important in my early development as an economist was the one that I think I first got from Robbins in *The Nature and Significance*, that economists advised how the world worked while decision makers combined that knowledge with their value judgments to make decisions about which economists qua professionals were neutral. Joske and other philosopher friends criticised this sharp division but I was unimpressed by their arguments until I got inside Whitehall as an economic analyst in the newly formed National Economic Development Office (the so-called NEDY) in 1961-3. When I returned to the LSE, I presented a paper to the M<sup>2</sup>T seminar to the effect that this strict division between neutral analysts and committed policy makers did not stand up. It was a long time later that I put these reservations into print as Lipsey (1981). Sadly, by including it in a collection of essays directed at Canadian economists, it did not get the attention that I think it deserved since its message is quite general. The argument stretches over 20 pages so I can only mention here its general conclusion.

Robbins could make this clear dichotomy because he believed that economic knowledge was certain knowledge. As soon as we admit that all knowledge is subject to being upset by subsequent observations; that what we think we know now, we know with less than 100 percent certainty; and that economists and policy makers must communicate with each other through the imperfect media of language not mathematics, the relationship between the economic analyst and the policy maker becomes much more opaque.

"The economic advisor and the policy maker are involved in complex human relationship, entangled in various uncertainties and communicating with each other through an inevitable haze of emotional reactions. Economists may strive towards an ideal of communicating their knowledge as objectively as possible, but objectivity remains an ideal that guides their actions, not a reality that fully describes them, (Lipsey 1981, 35 )

## Testing Existential Statements

Early on in our M<sup>2</sup>T seminar deliberations we encountered Popper's distinction between a prediction that was refutable by finding conflicting evidence but not provable because one could never rule out the possibility of finding conflicting evidence in the future and an existential statement that was provable by finding the postulated item but not refutable because one could never rule out finding it through some later more embracing study.

This issue never arose for me in a practical situation until much later when I became involved in the development of a new idea in growth theory: the importance of the General Purpose Technologies (GPTs) discussed in an earlier section. Two economists (Moser and Nicholas, 2004) studied the patents associated with electricity and found them not to be anything like as far reaching as GPTs were supposed to spread their influence. From these observations they concluded that electricity was not a GPT. However, if there ever was a GPT, it is electricity because it enables the great bulk of all modern technologies that would not be possible without it. (If you doubt this, observe what stops functioning when electricity is cut off by some power failure.) The problem these researchers had was that patents do not cover many of the places in which electricity enabled the development of other new technologies. More generally, the proposition that electricity is a GPT is an existential statement and the correct conclusion of their study was that *their patent data was not sufficient to establish that electricity was a GPT but not that the data established that electricity was not a GPT.*

This led me to consider when an existential statement could be disproved and to conclude that if it was circumscribed enough so that all possible places where it was postulated to exist could be located, then it could be disproved. So to re-use Popper's famous example, the statement that 'angels exist' can be proven by finding one but cannot not be disproved, while the statement that 'angels exist in observable form and are in this room' can be disproved by not finding one after an exhaustive search of the room. The importance of this distinction lies in the criticism of GPT theory by some economic historians. They criticise the concept because they argue that it does not rule out such trivial but widely used technologies as screws as being GPTs. However, one of the characteristics of a GPT is that it does not have close substitutes and it is clear that screws do have these. All of their uses can be enumerated and it can then be shown that in each case there are close substitutes. So the statement that electricity is a GPT cannot be refuted by evidence while the statement that screws are GPTs can be.

We may agree on this but many journal editors apparently do not – after all the refutation of electricity being a GPT was published in one of the most influential economic journals, the *American Economic Review*.

## Identifies and Behavioural Equations

One of the most difficult conceptual issues that I ever had to grapple with, and one that still confuses many in the profession, concerns the formulation—starting from Keynes himself and running up to the 1970s—of Keynesian macro-economic theory using identities rather than behavioural equations. I well remember sitting in the LSE cafeteria and hearing a famous American economist, who was much older, and we presumed much wiser than us, saying that one of Keynes' great contributions was to 'discover' the important set of identities that underlay his theory.

Indeed during the period from the end of WWII through the 1960s, the Keynesian definitional identities permeated the text books at all levels with empirical propositions being ‘derived’ from their manipulation! The first year textbook version took the form of the famous diagonal cross diagram. Expenditure was divided between an endogenous component that varied with national income,  $Y$ , and an exogenous component that was independent of income. In the simplest version the endogenous component was consumption expenditure,  $C$ , while the exogenous component was investment expenditure,  $I$ .

$$(1) E = C(Y) + I$$

and in the simplest linear form

$$(2) C = cY$$

where  $c$  is the fraction of income spent on consumption. Then by substitution

$$(3) E = cY + I$$

which was the expenditure function.

The ‘model’ was then completed by the definitional identity

$$(4) E \equiv Y.$$

(Indeed these two variables are defined as identical in national income accounting.)

Plotting (3) and (4) produced the diagonal cross diagram as shown in Figure 1. It should have been obvious that this was not a valid prediction-generating model by observing that if  $E$  and  $Y$  were just two different symbols for the same thing, (4) could be substituted into (3) to yield

$$(5) (1-c)Y \equiv I$$

which states that, given  $I$ , there is *only one possible value for  $Y$* ,  $(I/(1-c))$ , which is  $y_I \equiv e_I$  in the figure.

[Figures 1 &2 about here please]

One other common Keynesian proposition of the time is implicit in the above. The fraction  $(1-c)$  represents the fraction of income not spent on consumption, which in Keynesian theory is the fraction saved, i.e.,  $(1-c)Y \equiv S$ . So (5) can be rewritten as

$$(6) S \equiv Y$$

telling us one of the early Keynesian dogmas: that savings must always be equal to investment, not just in equilibrium but always. Of course there is nothing wrong in defining them that way but it is an error to then think this tells us anything about real world expenditure on saving and investment when they are not defined to be the same thing.

I had been exposed as a graduate student to Bill Phillips’ water machine model of the circular flow of income and expenditure between firms and households. Firms create income for households by producing the national income,  $Y$ , which is divided between consumption and investment production. Households allocate their expenditure between consumption, which creates income for firms, and savings. The income and expenditure related to consumption circulates in a closed circuit while investment is modelled as an injection into this circular flow and savings as a withdrawal from it. In the machine, these two flows are only equal when income

is unchanging. When it is rising, the injection of investment expenditure exceeds the withdrawal of savings and when it is falling the opposite relation holds between savings and investment.

When I came in 1961 to begin the macro half of *Positive Economics* I laid out a numerical example of the circular flow model as in Figure 2 and tried to reconcile the flows with the Keynesian identities. Try as I might, I could not do this and I spent much time in an intellectual fog. A recourse to the major texts did not help because they all used the identities with neither hesitations nor qualifications. Finally, I realised that the identities could not tell us anything that was happening in the actual circular flow model nor in the actual world. All of this may sound quaint to today's readers but to me it was a great insight and it took much time to convince my colleagues that the text books were all wrong on this issue.

I wrote this up in an article in which my RA and I surveyed the literature to locate the many unacceptable uses of the identities. I started the paper with the following list of incorrect statements all drawn from the contemporary literature Lipsey (1972, 303-4).

#### *I. The Static Model in Equilibrium*

1. The equilibrium of a Keynesian model is given by the intersection of the aggregate demand function and the 45° line which expresses the accounting identity  $E \equiv Y$ .

#### *II. The Static Model in Disequilibrium*

2. Although people may try to save different amounts from what people try to invest, this is impossible; thus actual realised savings must always equal actual realised investment.
3. Out of equilibrium, planned savings do not equal planned investment, and it follows from (2) that someone's plans must be being frustrated; thus there must be some *unintended* savings or dissavings, and investment or disinvestment.
4. The simultaneous fulfilment of the plans of savers and investors can occur only when income is at its equilibrium level in just the same way as the plans of buyers and sellers can only be simultaneously fulfilled at the equilibrium price.

#### *III. The Dynamic Behaviour of the Model*

5. Whenever savers plan to save a different amount than investors plan to invest, a mechanism works to ensure that actual savings remain equal to actual investment, in spite of people's attempts to make it otherwise. Indeed, this mechanism is the source of dynamic change in the circular flow of income.
6. Since the real world, unlike the simple textbook model, contains a very complex set of inter-relations, it is not an easy matter to see how it is that savings stay equal to investment even in situations of the worst disequilibrium and the most rapid change.
7. The dynamic behaviour of a Keynesian circular flow model in which disequilibrium implies unintended investment or disinvestment can be analysed by moving upwards or downwards along a diagonal cross diagram in the 'step-fashion' illustrated in figure 1 [Figure not shown here].

Each of these statements can be found in the majority of standard treatments of national income theory and there are few books that do not make some of them. The statements would appear to be regarded as part of established knowledge in the subject. Yet every one of them is wrong.”

The paper went on to show how each was mistaken in predicting real world behaviour derived from the identities. Here I only mention one of the many absurdities. In Figure 1 where income and expenditure are defined to be the same thing and are shown by the 45° line, there is only one level of income consistent with the way the curves are drawn in the figure,  $y_1$ . Since the identity means that we have the same variable on each axis, there cannot be actual points off the 45° line. Usually implicitly, but sometimes explicitly, the expenditure function was interpreted as a desired function whose points could lie off the 45° line while all actual points had to lie on that line. But then the text books invariably went on to consider what was happening when income took on other values, such as  $y_2$  or  $y_3$  in the figure and expenditure was not equal to income, all of which were ruled out by the definition that  $Y$  and  $E$  were the same thing. In the paper we go into the many confusions that stemmed from trying to do the impossible of dealing with disequilibrium situations when no actual points can lie off the 45° line, as well as dealing in detail with the other errors listed above.

The resolution of this problem was to regard  $E$  as a desired expenditure and  $Y$  as GDP actually produced. Equilibrium income would then occur when people desired to purchase the whole GDP, no more and no less. Thus  $E = Y$  became an equilibrium condition stating that agents desired to purchase just what was produced, no more and no less.

My research assistant, John Sitwell, and I co-authored an appendix to my paper titled “A Short History of the Middle.” We surveyed the pre WWII savings-investment debate in which many of the correct points were made and contrasted this with the almost total lack of the correct analysis in the literature of the several decades following WWII. Even Paul Samuelson got it wrong when he wrote: “measured S and measured I stay identical even during the worst disequilibrium — by virtue of some people experiencing unintended losses or gains and/or experiencing unintended investment or disinvestment. (Samuelson 6<sup>th</sup> edn. 261). It appears that according to Samuelsson that during a disequilibrium real behaviour is being affected in order to keep the identity fulfilled!

We concluded with the following observations (Stilwell and Lipsey, 339-340)

“We have, therefore, the two facts that the majority of modern elementary and intermediate textbooks, together with parts of much more advanced literature, treat the Keynesian identity as an inexorably self-establishing equilibrium condition, and that the pre-war saving and investment debate, although giving rise to much confusion, contained a number of articles which correctly exposed the errors and changes - notably those of Robertson, Haberler and Lutz. How can these be reconciled? The error did not enter the most influential textbooks, Benham and Samuelson, until relatively late. The fourth edition of Benham (1948) was free of the errors that first appeared in the fifth' edition (1955). Benham's book was, however, innocent of Keynesian economics until 1955 so that the errors are introduced the first time Benham attempts to expound Keynesian theory. The case of Samuelson's extremely influential textbook is more interesting. Samuelson's book was of course Keynesian in spirit from the outset but it is error free until the fourth [1958] edition

Perhaps the error in its modern form originated somewhat independently of Keynes. It might have stemmed from the combination in most textbooks of a chapter dealing with elementary national income accounting, and a chapter dealing with income flows where the simplest model, that of one injection, one withdrawal, system is considered, the quantities being labelled respectively investment and saving. The definitions used in the accounting chapter may have crept over into the latter parts of the other.

Alternatively it is possible, and perhaps more likely, that by the middle fifties the salutary effects of the debate which followed the publication of the General Theory had been forgotten and the reputation of Keynes led to the inclusion in textbooks of the crudest expressions of the errors. This is only speculation; the fact which we have is that the customary exposition of simple macro-static models is worse than it was twenty years ago.”

My Article was rejected by *Economica* and, preoccupied with other things, I did not try to get it published elsewhere, (for which I am now sorry). Then when a festschrift was announced for Lionel Robbins’ 80<sup>th</sup> birthday, I dug out the article and it was included in his volume—and there met a fate that was at the time usual for pieces in conference volumes: almost totally ignored.

But I did get it right in the first edition of *Positive Economics* where I introduced macro theory in terms of circular flow diagrams in which  $S$  and  $I$  were equal only in equilibrium; and where  $E = Y$  was an equilibrium condition expressing an equality between what is actually produced,  $Y$ , and what agents *desire* to purchase,  $E$ . I also added an appendix “National Income Accounting” where I explained in detail the distinction between the definitional identity of  $E$  and  $Y$  in national income accounting and the use of these as desired variables that were only equal in equilibrium in economic theory. I concluded (IPE 1<sup>st</sup> edn, 360): “The student needs to operate in a theory of income with concepts of savings and investment which do not make these two magnitudes the same thing. The student need not worry further about the use of saving and investment that makes those two magnitudes identical.”

Given IPE’s instant success as mentioned in Section II, I like to think that I had some part in the transformation over the next couple of decades from using  $E \equiv Y$  as an equilibrium condition to interpreting that identity as the way *ex post* measurements were made and using  $E = Y$  as an equilibrium condition.

### **Internally and Externally Driven Research Programs**

Another important methodological issue was not discussed directly in the M<sup>2</sup>T seminar, although all of its substance was implicit in our deliberations. I came upon this issue directly when I was asked to give a plenary lecture to the meeting of “The International Network of Economic Method” at The University of British Columbia in June 2000, subsequently published as Lipsey (2001).

I chose as my subject the mathematizing of economic theory in the second half of the 20<sup>th</sup> century. Most economists were aware of how much the power of mathematical analysis allows them to do that could not be done with verbal and geometrical analysis, the main analytical tools when I was an undergraduate in the long-ago 1940s. So I concentrated on what had been lost. One important loss has been much of the knowledge of the world about which today’s students

are theorising. Such knowledge that was once part of the education of economists has gone to clear time for learning more and more complex mathematical analysis. While students of Industrial Organisation can develop complex theorems in Games Theory, many of them have only the slightest knowledge of the world to which these models are meant to apply. In a different graduate-school box, students of advanced micro economic theory see the world's production capacity as typified by a single representative price-taking firm (i.e., one without market power) who encounters constant returns to scale as it expands its activities. Micro economics text books once had a lot to say about the highly important phenomenon of increasing returns, but since these create non-convexities that are not easy to incorporate in the analysis based on the assumption of universal convexity, they have been largely eliminated from the text books. For every 100 students in either of these groups who can handle the complex theory to which they have been exposed, maybe two or three have some idea of how firms actually behave: how they set their prices, taking account of what they can and cannot change; how their behaviour is constrained by governments, how much by the need to pacify shareholders, and how much by the need to shade behaviour in the light of strong public concerns about environmental concerns. In short, those in micro economic theory spend so much time studying models of the workings of the hidden hand they have no time to study the workings of the visible hand—the title of Alfred Chandler's great book (1977) on how businesses actually do behave.

Much though I believe that Joske would have joined eagerly in such a discussion if it had been relevant then rather than now, the point I want to raise here is the distinction between two types of research program. I define an internally driven research program, an IDRP as a research program that is driven by its own internal logic; investigators seek to understand problems created by the models that they are using, rather than deriving their problems from observations. In contrast, an “externally driven research program” (EDRP), is one that is driven by, and constrained by, observed facts.

An IDRP is not always conducted without any appeal to facts. It often begins with some factual question and later, if any tests are used at all, they typically take the form of trying to make the model track the data. Since investigators are usually free to alter the detailed specifications of such key components as variables, and lags, tracking the facts is a very weak test. With a bit of ingenuity almost any model can be made to track a set of "relevant" facts.<sup>2</sup> Many of the fads and fashions that sweep economics are aspects of one internally driven research program or another.<sup>3</sup>

Early post WWII growth theory was typical of an IDRP in that at least one of the originators, Evsey Domar, had empirical concerns—about the possibility of maintaining full employment in a capitalist economy—concerns that were raised by the experiences of the Great Depression and the Second World War. His simple model (Domar 1946) assumed a fixed ratio of

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<sup>2</sup> A particularly worrying habit of economists, when they are making models track the data, is to try many alternative specifications and then publish only the one that has the highest  $t$  statistic (or the best value of whatever test of goodness of fit is relevant to the techniques being used). Then, with a straight face, report these statistics as if they were genuine tests of the adequacy of the model.

<sup>3</sup> This is not to say that internally driven programs never produce interesting results; they sometimes do. The probability of their producing empirically relevant results—even when there is a demand that their models be made to track the data—is, however, quite low.

capital to labour and produced a knife-edge equilibrium, departure from which caused the economy either to fly into unctrollable inflation or uncontrollable depression. Robert Solow (1956) in a path-breaking article showed that if substitution between the two factors of capital and labour was allowed for, the model could settle into a balanced growth path. This was an important contribution but it led to an IDR that lasted nearly two decades. What if there are more than two factors of production? What if there is more than one output? What is the most efficient way to move to a balanced growth path if the economy is off it originally, and so on and so on for more than 15 years.

At the end of it all this effort the Nobel Prize winning economist, Amartya Sen, had this to say about the enterprise that had absorbed the attention of some of the subject's greatest minds for so long.

"The policy issues related to economic growth are numerous and intricate. ... While the logical aspects involved in these exercises are much better understood now than they used to be, perhaps the weakest link in the chain is the set of empirical theories of growth that underlie the logical exercises. Possible improvement of policies towards growth that could be achieved through a better understanding of the actual process of growth remains substantially unexplored. [!] It is partly a measure of the complexity of economic growth that the phenomenon of growth should remain, after three decades of intensive intellectual study, such an enigma. It is, however, also a reflection of our sense of values, particularly of the preoccupation with brain-twisters. Part of the difficulty arises undoubtedly from the fact that the selection of topics for work in growth economics is guided much more by logical curiosity than by a taste for relevance. The character of the subject owes much to that." (Sen, 1970:33)

Economics is replete with this type of IDR. Someone comes up with an interesting theory that predicts a surprising result. One or two researchers seek to test the prediction, while most seek to determine if the model is robust in the sense that the prediction will survive making the model from which it is derived more complex in any number of ways, including making it 'more realistic'. Soon this activity becomes dominant and rounds of theorising ensue. Eventually, the original issue is forgotten while efforts go into making more and more elaborate models that began with the original predictor. Eventually, a consensus evolves that this effort is going nowhere and it peters out – often with a conference summing up and asking why so little came from so much effort. Whereas Joske and the other members of the M<sup>2</sup>T seminar could have told them, once the IDR rather than the EDR nature of the program had been established, that this would be the end result.

### **Economic Policy With and Without Welfare Rules**

Finally I come to my biggest issue. It began with my undergraduate training, was fully discussed at M<sup>2</sup>T, and for me is still bearing unexpected fruit. Classic text-book economic policy is based on a highly idealised model of the working of the price system. It is a static equilibrium model with given technology and in which all markets contain so many buyers (households) and so many sellers (firms) that no one of them can exert any significant influence on the market – what economists call perfectly competitive markets. Everyone must accept the market prices as they are set by the impersonal forces of demand and supply (i.e., set by the collective behaviour



of all buyers and sellers each motivated by self-interest). A large number of other conditions are required, such as that there be a market for every single good and service; that there are no externalities where behaviour in one market has effects beyond that market (think pollution!), that there be no public goods such as parks where one person's use does not preclude another person's use, to mention just a few of the many stringent conditions. A.C. Pigou, the founder of formal welfare economics, argued in his path breaking book *The Economics of Welfare* that this model produced an optimal allocation resources that maximised the sum of wellbeing.

During our time in the M<sup>2</sup>T seminar, the profession was just going over from the verbal geometrical analysis of this model, to a formal mathematical proof of the existence of the model's equilibrium as pioneered by Arrow and Debreu's formulation of it as a general equilibrium mathematical model. Pigou's argument for the optimality of this model was replaced by this more formal proof which led to what came to be called the two fundamental theorems of welfare economics that graduate student devote much effort to proving. According to Mark Blaug who, although not a founding member, joined our seminar shortly after to become a frequent contributor to our ideas: "The first fundamental theorem states that, subject to certain exceptions—such as externalities, public goods, economies of scale, and imperfect information—every competitive equilibrium is Pareto optimal." (Blaug 2007: 185) A Pareto optimal allocation of resources is one in which it is impossible to make any change that would result in making someone better off without making someone else worse off.<sup>4</sup> Note Blaug's irony here as he knows full well that the 'exceptions' to which he refers are found almost universally in real economies. "The second fundamental theorem states that every Pareto-optimal allocation of resources is an equilibrium for a perfectly competitive economy, provided a redistribution of initial endowments and property rights is permitted; alternatively expressed, every Pareto-optimal allocation of resources can be realized as the outcome of competitive equilibrium after a lump-sum transfer of claims on income." (Blaug 2007: 185)

Standard, text-book economic policy consisted at that time (and much of it still does) in identifying the existence of things that would prevent the establishment of a Pareto efficient allocation of resources in the model; calling them 'distortions' or 'market failures' and then advocating removing them piecemeal wherever they were found in the real world (i.e. virtually everywhere). (This analysis used to be part of what was then called 'welfare economics' but is now called 'public economics'.) This policy had already been given a severe blow on its own terms by the publication of my first article titled, "The General Second Best" (Lipsey and Lancaster 1988), which showed that even in this abstract model, the piecemeal establishment of any number of the conditions needed for a full optimum would not necessarily make people better off (actually or potentially) as long as not all of the conditions were fulfilled. In other words the conditions for maximising any objective function do not provide rules for piecemeal increases in the value of that function when its maximum is not achieved.

A deeper criticism, and one we discussed in the seminar, is that it is odd to argue that the conditions needed for an optimum allocation of resources in a model so far from reality should be a guide to what should be done in the real economy. Indeed, the members of the seminar were in general agreement with the messages of the several critical books on welfare economics written before the two fundamental theorems had taken fully over from the older form of

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<sup>4</sup> In this context a person's welfare is indicated by his utility function that orders for him/her all possible consumption states.

analysis (e.g., Little 1950 and de Graaff 1957). These books argued in depth that the conditions required for the abstract model to produce an optimum allocation were so very far from the reality of any real market economy as to make it a poor guide for real-world economic policy. One of our founding members, Kurt Klappholz, gave a series of lectures on welfare economics and the irrelevance of the standard approach to policy derived from it. (I urged him to publish this excellent set but he had a severe writer's block that possibly came from being the only one of his family to survive a Nazi concentration camp.) It is an interesting comment on the point raised in an earlier section that when welfare economics was taught verbally and graphically, students were routinely exposed to criticisms of its applicability. But when it became mathematised in the form of the two fundamental theorems, students get almost no exposure in the text books to criticisms similar to those in the earlier books that argued that the conditions necessary for an optimum allocation of resources in the neoclassical model are so far from reality that they provide an imperfect or even are irrelevant guide for policy.

Most real policies have effects on both efficiency and distribution, there being winners and losers. The second fundamental theorem is often taught to students as a way of separating issues of efficiency from those of allocation. You suggest a change that raises efficiency and then notionally make lump sum transfers from the winners to the losers so that no one loses. Of course the idea of doing this in practice is, as Baumol (2002, 143) says a "fairy tale" that should be discarded. It is, however, a tale that all too many students repeat when they say that the distributional effects of this efficiency policy are to be offset by lump sum transfers. Even when real-world payments are made to compensate losers, these do not (indeed could not) take the form of lump sum transfers. So in reality, if not in the classroom, efficiency and distribution are inevitably intertwined when any actual policy measure is considered.

There is a vast amount of literature relating to second best and economic policy, some of which is summarised and critiqued in Lipsey (2007) long after it was published. Fortunately we can bypass this material and begin in 1990 when I had a major epiphany on these issues that took me well beyond any M<sup>2</sup>T discussions. In that year I was asked by the Canadian Institute for Advanced Research to lead their international program on "Economic Growth and Policy," which I did in 1990, resigning as leader after it was fully set up and continuing as a member well into the 2000s. My great insight was that technology and technological change had to be understood in a much more nuanced form than in the way it occurs in all macro growth models — whether as exogenous (as in the Solow growth models) or endogenous (as in the Romer and Lucas models) — where it appears as a scalar parameter or variable. It came as a great insight to me that we need to treat technology as endogenous and something that needs to be understood in detail, as did Nathan Rosenberg (see e.g., 1982 and 1984) rather than as a single broad aggregate, as do virtually all models based on an aggregate production function. It took a long time to see how much of what is taught in economics depends on technology being exogenous and how many cherished propositions change as soon as its endogeneity is accepted. (There is no space to go into all of these changes here, changes I have discussed in some detail in Lipsey 2013.)

Stated baldly: because we live in a dynamic rather than a static world, the concept of a static, optimal allocation of resources has no policy relevance. Even if we were given full information about everything that can be known, an optimum cannot be defined, let alone attained, because of endogenously generated technological change undertaken under conditions of pervasive uncertainty. In a world of uncertainty, agents must make what are at best informed guesses about the values of different lines of activity, including R&D. Different agents will make

different guesses, even under identical conditions. In such a world, static maximisation is impossible and, as I have many times observed, firms are more correctly seen as groping into an uncertain world in a manner that is profit seeking but not profit maximising. Here the concept of a static optimum allocation of resources is undefined since no one can know in advance the actual or expected outcomes of each possible resource allocation.

Many of the ‘distortions’ that are seen as undesirable sources of market failures in the neoclassical theory are the very things that drive economic growth in a dynamic, evolving and uncertain world. They are thus to be encouraged not suppressed. For just one example, firms compete with innovations designed to produce the large temporary profits that are needed to persuade them to undertake the risky ventures —the vast majority of which fail — of developing and introducing the new products, new processes and new forms of organisation that are the essence of economic growth.

The surprising fact is that so many economists have accepted the view that the conditions that would make a wholly imaginary static world optional are likely to be the conditions that would improve matters in the dynamic real world. The critical contrast between the policy advice of neoclassical and evolutionary theories is well stated by Mariana Mazzucato, the R.M. Phillips Professor of Economics at the Science Policy Research Unit, Sussex England (2015: 5-6):

“According to neoclassical economic theory...the goal of government policy is simply to correct market failures...once the sources of failures have been addressed...market forces will efficiently allocate resources...[Instead] nearly all the technological revolutions in the past—from the Internet to today’s green tech revolution—required a massive push by the State. Silicon Valley’s techno-libertarians might be surprised to find out that Uncle Sam funded many of the innovations behind the information technology revolution”

Many economists have argued that if we abandon the policy rules derived from welfare economics and the neoclassical theory of the optimum (static) allocation of resources, as e.g., Mazzucato had advocated, there can be no rationally based economic policy. (See for example Ng 2017) In disagreeing (see Lipsey 2017a and b) I join Mark Blaug in arguing that although the appearance of scientific certainty is removed when the welfare rules are no longer seen as the underpinnings of economic policy, economists still have much to add to policy discussions — just as they have been doing for decades. Just because we cannot be absolutely sure that anything we do will unambiguously raise the entire community’s welfare, policy need not be without rational guidance, being based on a mixture of evidence, theory and judgement. As Mark Blaug put it (2007, p. 202-3: italics added).

“Virtually all economists are in favor of road pricing because they believe that the potential Pareto improvements created by reducing road congestion and saving travel time greatly outweigh the costs of installing the necessary hardware, including the enforcement costs of policing whatever system is installed. There will be many gainers but there will also be many losers,... Here once again we have the classic difficulty of separating efficiency from equity. What do economists in fact bring to such an argument? First of all, a considerable familiarity with the facts regarding the use of public and private vehicles. Secondly, a considerable familiarity with the facts regarding family income and transport expenditure patterns, including the possession of private cars, allowing for accurate estimates of the price elasticity of demand for more or less fuel-efficient vehicles, not to mention the price elasticity of demand for gasoline. Thirdly,

considerable experience with survey evidence in large-scale social experiments comparing families and individuals with unequal access to transport to gauge the effect of, say, a miles-travelled tax rather than a fuel-based tax, possibly varying across people with different risks of causing accidents. *None of this will provide neat answers to the revolutionary introduction of road pricing. All it will do is to add one more highly informed voice to the squabble and that, I say, is what modern welfare economics is about and ought to be about, rather than teaching and learning a set of mathematically expressed fundamental theorems.*”

I could not agree more. This is just what I do when involved in the many policy issues that occupy my time—and something that I do without any recourse to the welfare conditions for achieving an optimum allocation of resources in static and imaginary world.

#### **IV. CONCLUDING REMARKS**

Philosophers debate many subtle and esoteric issues on methodology, issues that are beyond my ability to understand fully — at least given the time I must devote to my own speciality. But surely it is not without interest to methodologists to see, as illustrated in this essay, that many relatively straight forward methodological principles that might be found in a methodology 101 course can, if understood and applied properly, have big effects on economics and, I suspect, would also do so on many other social sciences.

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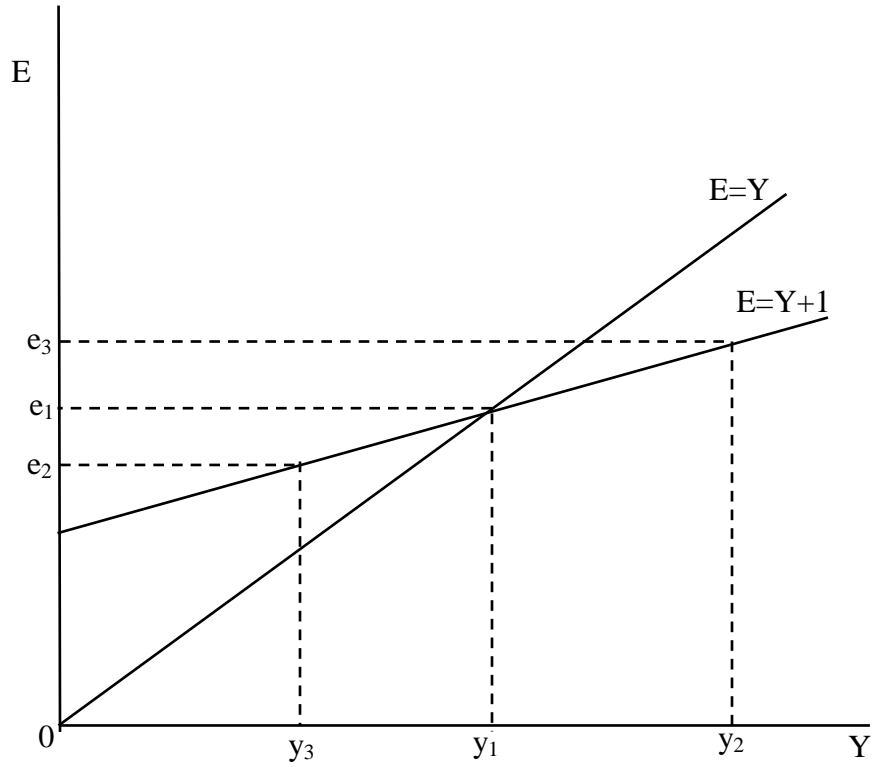


Figure 1: The Diagonal Cross

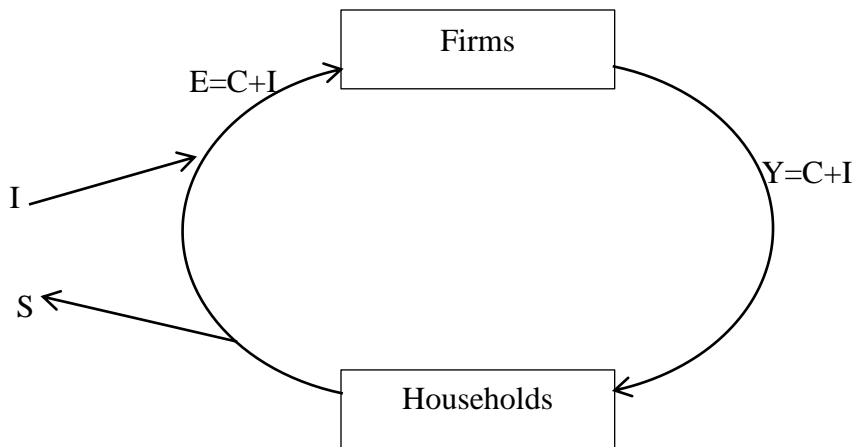


Figure 2: The Circular Flow of Income and Expenditure