salaries were higher and rising impressively, contributed to their decision to move. Thus both groups felt that their decisions were career oriented. It is interesting that the Americans did not seem to experience the difficulties that Canadians had in dealing with Canadian institutions. No doubt, the time factor contributed to this situation. The Canadians entered the job market earlier, when opportunities were not too plentiful in Canada. When the Americans came into the market, the Canadians were already climbing American career ladders and Canadian universities were expanding. What will happen when economists trained by those universities enter the market in quantity remains to be seen.

In conclusion it should be remembered that most of the respondents are academicians, who have made career-oriented decisions. There is such a coincidence of factors considered important that it is tempting to suggest that Canada and the United States approach a unified labor market for the professional economists polled. Tendencies in this direction are of course subject to modification due to governmental policies in the two nations.

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"Marshall, Sraffa and Keynes: Incompatible Bedfellows?"

G. C. HARCOURT

I want to make my principal theme, the role of the (Classical) concept of centres of gravity in the work of three great Cambridge economists: Marshall, Sraffa and Keynes. There are a number of obvious connections among them apart from their Cambridge connection for both Sraffa and Keynes spent much of their professional life either using, amending, criticising, overthrowing and/or evaluating Marshall's work. Thus Sraffa's 1925 paper and, especially, his 1926 paper specifically were directed to a critique—a devastating one, I would say—of Marshall's contributions to value theory. (Sraffa makes this perfectly clear in his reply to D. H. Robertson in the 1930 Economic Journal Symposium on "Increasing Returns and the Representative Firm." "We seem to be agreed that [Marshall's] theory cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconciles it with the facts it sets out to explain. Mr. Robertson's remedy is to discard mathematics, and he suggests that my remedy is to discard the facts; perhaps I ought to have explained that ... I think it is Marshall's theory that should be discarded." [p. 93]) Keynes, used, developed and ultimately rejected much of Marshall's theory of money and interest. He attempted to absorb Marshallian value theory into his theory of the general level of prices in the General Theory in which his purpose was to integrate value, production and monetary theory into one system. He abandoned the specifically Marshallian aspects in 1939 (unless we adopt the Cambridge maxim that it is all in Marshall—it probably was, see pp. 374–77 of The Principles) when in reply to Dunlop and Tarshis, he outlined in rudimentary form the normal cost pricing hypothesis. Keynes wrote a superb biographical essay on Marshall before he was fully into his own revolutionary stride and so was more accepting of Marshall's monetary theory than he subsequently was to be. Finally, Marshall belonged to, or at least, thought that, or presented himself as belonging to, the Classical tradition which Sraffa inherited (and for which in his 1960 book, Production of Commodities ..., he provided the foundations both for a revival and an updating).

One of Sraffa's purposes, though, was, to attack the inner logic of Marshall's system of thought because, whether Marshall really believed he was fulfilling what the Old Masters had sensed but not quite rights or had not fully developed—Joan Robinson for
one thinks that Marshall was foxing on that—but certainly, in Sruffa’s view, his effect was both to subvert and to emasculate the robust Classical tradition on which he drew. When I say in Sruffa’s view, I think that there is direct evidence for this in Sruffa’s own writings and that it can be read indirectly into them as well from the hints and asides which he gives. Even more, there is another source of indirect evidence which is provided in the writings of Pierangelo Garegnani and Krishna Bharadwaj who, of Sruffa’s colleagues and friends, are among the most in accord with Sruffa’s own views. In Krishna Bharadwaj’s fine 1976 R. C. Dutt Lectures, Classical Political Economy and Rise to Dominance of Supply and Demand Theories, she explicitly acknowledges that the themes developed there owe much to discussions over the years with Sruffa. She refers in many places, especially in the second lecture, to the 1925 and 1926 papers. Sruffa gave her access to Marshall’s copy of Pigou’s Wealth and Welfare in which Marshall’s misgivings about Pigou’s extension of his—Marshall’s—mode of analysis are only too plainly expressed.

There is also her paper on Marshall’s early work on value, edited by J. K. Whitaker.

I have drawn literally upon the writings of Piero Sraffa and on the innumerable and illuminating discussions we have had over a long period.


The substitution referred to in the title assumed existence of things which were left unnoticed by the objective facts of production and which so absorbed the subjection of, or even its value, distribution and production theory, and in amputating the central concept of the surplus—its creation, taxation and alienation—in favour of a theory of the allocation of given resources, in which the role of specialization in consumption and production could have little or no place. The production and the distribution exchange in which the surplus was created—was not of any importance, and the factor of value, in contrast to the early Marxian theory, played no role.

I think it is entailed in the notion(s) of centre of gravity. (Discussions with John Eatwell, Pierangelo Garegnani, Bertram Scheffold and Ian Steedman, together with a run along the Cam (very greatly helped me to sort these out.) The first three are analogies drawn from physics, the fourth, an analogy drawn from meteorology. The first relates to a frictionless pendulum which always swings but always passes the same minimum point on its path to and fro. The second is a pendulum, the notion of which grew to a great deal because of friction, and which, through the influence of another body, so actually settles at the minimum point of its path i.e. at the centre of gravity of the first notion. (As we shall see, this notion is most closely related to Marshall’s concept of a long-period position or at least to the end point of the process.) The third is, I think, David Chalmers’ example of a dog always running towards its master who is riding a bike. The bike is the centre of gravity which itself is moving but the dog’s direction of movement at any point in time can be predicted by knowledge of where the bike (and its master) is, at that point in time. The fourth analogy is, in many ways, I think, the most illuminating. We all know that various places and seasons may be characterized (in part) by the concept of their average temperature, even though, on any one day, because of the influence of special or temporary factors, the actual temperature may not coincide with the average. Yet the average is a good sustained predictor and description over the years, from year to year, and as between places. Similarly, the average temperature itself is explicit in terms of relationships between the average of these factors which determine temperature, even though the actual value of each one of these factors in turn may depart from its average value on any particular day. The average temperature thus is a centre of gravity in the sense of a central tendency to which actual temperatures will tend, the outcome of sustained and fundamental forces, that is to say, the average serves to explain most of the orders of magnitude of the values actually observed from day to day.

What of Marshall’s work have I refrained in order to write this paper? I suppose that I have concentrated on the Prefaces and Vol V of the Principles invaluable Appendix H on increasing returns and long-run normal supply price; and Appendix I on Ricardo’s theory of value, on how Marshall came not to bury Ricardo but to fulfill him. In addition, I have looked at Marshall’s early essays on international and domestic values, noting, as I think Keynes did, that he worked back from the demand and supply schedules to what he thought they underlying—ultimately stability or disequilibrium—rather than, as the Austrian did, and Wicksteed also, starting from subjective introspection and working outwards to the market phenomena. I also reread the symposium on increasing returns in the 1930 Economic Journal and I thought again how remarkably acute Gerald Shore was—I suspect that Keynes asked him to contribute in order to put out of him a substantial chunk of that “unpublished study of the relations of cost and output on which [Shove] had been engaged for some years,” which formed part of the Cambridge oral tradition at that time). In addition to the work of Krishna Bharadwaj, I read Keynes on Marshall, Mary Marshall (I liked it—or perhaps her—best) and Jevons; and I have had access to a number of unpublished papers by Garegnani and Joan Robinson in which some of the issues discussed are relevant for the present theme. Part of what Garegnani has to say may be found in my comment on
II

I return now to Marshall himself. It is clear, I believe, that he wanted to make the concept of normal values, and especially long-run normal values, the centre pieces of his theory of value and (distribution). The long-run equilibrium positions and the prices associated with them were the partial equilibrium, firm and industry, versions of the long-run positions of the economy as a whole which are found in the classical scheme (and, of course, in Marx). Marshall realized that he would eventually need to have a theory of the levels of the normal profits and normal wages to fit into his theory of the composition of the individual prices of the commodities of individual industries—otherwise there would not be an explanation of a general level to which the prices, et al, of each industry would have to measure up—but he kept backing away from going the whole way in supplying them because he sensed the logical difficulties contained therein. Where Marshall differed from the classicals was that his long-run prices were the outcome of the symmetrical and opposing forces of supply and demand. The forces which affected prices conventionally could be placed in one or other of these categories: at the level of a firm or an industry, one set reasonably could be regarded as independent of the other; the prices would settle or tend to settle at levels which the two blades of the scissors (or balls in the bowl) dictated; and in normal conditions the forces were sustained and fundamental enough to make it reasonable to talk of an equilibrium being struck, or being potentially there to be struck, providing the est. par. clauses held long enough in practice. A normal price is a price "which any one set of conditions tends to produce" (p. 372). If we were to insert "ultimately" before "tends", we would have our second analogy, the pendulum the motion of which is subject to friction.

Marshall wanted to use the concept of equilibrium because, despite his monitory anxieties and protestations about biology being the Mecca of economists, in his actual analysis (which he was very reluctant to apply directly to actual situations), he really did want to use mechanical analogies—pendulums, etc. He wanted to argue that the state of rest which ultimately would be reached had an effect on what was happening at any moment even if, in fact, that movement itself was away from the equilibrium position. It is true that he tried to be careful, or rather he knew that he should be careful, to keep his statements confined to what would happen near or at an equilibrium, i.e., that any displacements considered were from an already achieved equilibrium—hence his starting point of analysis of a stationary state—but, nevertheless, many of his applications had to occur well away from the intersections and take in the curves (and the areas under them) associated with those positions away from them. There is no doubt that as Marshall ventured beyond his equilibrium and neutrality assumptions, he went off course and became more and more inconsistent as he proceeded.

It was on these very points that Sraffa went after Marshall in his 1926 article—trying to find those situations where the assumptions of partial equilibrium analysis were actually met so that the dependence of supply factors on demand, and vice versa, really was "of the second order of smalls" and thus could be ignored: "to what extent the supply curves based on the laws of returns satisfy the conditions necessary to enable them to be employed in the study of single commodities under competitive conditions" (p. 538). The answer was, as we know, disappointing—only in those cases where economies were external to the firm but internal to the industry, "precisely the class which is most seldom to be met with" (p. 540), were the conditions likely to be met. Marshall also wanted to introduce the concept of substitution—"the dynamical principle of substitution... seen ever at work" (p. xv)—the universality of the theory of value, distribution and production (instead of their separation as in Classical political economy), i.e., that all could be embraced in...


effect within exchange theory; and his adaptations of what he took to be the essential truths of the laws of diminishing and increasing returns into his supply and demand analysis. He wanted, as Joan Robinson has stressed, to analyze an economy moving through time with accumulation going on, yet still use the supply and demand apparatus as the principal illuminator of the process. This illustrates another conflict in Marshall's thought and approach: he was at one and the same time the master of empirical generalizations and also a superb user of deductive reasoning. His attempt to centralize the principle of substitution and his equal stress on continuity led him to develop marginal analysis "in economics, as in physics, changes are generally continuous" [p. 409, n. 11], choice and change at the margin, drawing, he argued, on Classical uses to justify this, as it first arose in the theory of rent.

However, as Krishna Bharadwaj shows, he still revered the essentially different concepts of the margin in Classical political economy—those associated with different qualities of land, where there is no change of a neoclassical sort occurring, no counterparts as Sen characterizes what Sraffa is doing in Production of Commodity...

6 Sraffa's methodology can be seen as exploring how it is possible to be said instead of the inter-relationships between prices, distribution and quantitative magnitudes using only directly observed data, without making any use of counterfactuals, [the use of which is an essential part of "marginal analysis" (what would have happened if the facts had been different, e.g. if one more unit of labor had been applied)? A.K. Sen, "On the Labour Theory of Value: Some Methodological Issues," Cambridge Journal of Economics, 2, June 1978, pp. 101-81]. What, though, are we to call Sraffa's procedures when he considers different values of (say) the wage, while holding the methods of production constant?

...system and Standard commodity in order to...
Because Marshall was wedded to demand and supply analysis, and to having his long-period equilibrium positions determined at the intersections of supply and demand curves, he had to concern himself with problems of uniqueness and stability in a manner which the Classical economists could avoid. This had the further implication, as Krishna Bharadwaj argues, that neoclassical analysis only applies in situations where the conditions for stability are met—there is not the freedom of action in this framework which exists in Classical analysis and its modern counterparts, the “horses for courses” approach of Joan Robinson and Kalecki, for example. [The classical theory of value] is not constrained to permit only some specific changes of the many possible ones as alone consistent with the theory... does not have to presume more than is necessary for the limited objective of determining relative values at one ‘observed’ position of the economic system... The supply and demand systems sought to explain a single observed position in terms of potential changes... brought about by the balancing of marginal quantities operating through the principle of substitution... to be consistent with these explanations, the changes had to be in a direction... of the type postulated by theory... (p. 67). Moreover, once Marshall had committed himself to a long-period position which is the outcome of the forces of supply and demand, and to having schedules, he was committed to not allowing the approach to equilibrium to affect the equilibriums position itself. More, he was committed to an assumption of reversibility, if the supply curves were to mean what he said they meant, which, as he knew, and Sraffa made very plain in his 1926 Economic Journal article, makes the concept of the long-run supply curve a very fuzzy one indeed. (That was why Marshall was so worried about Pigou using a long-run supply curve in Wealth and Welfare.) Marshall discusses these issues in Appendix II and wants to dodge the conclusion that the long-run supply curve is irreversible, that it can only be an historical statement as opposed to a conditional one. But certainly Sraffa does not let him get away with this, either by use of the device of the representative firm or by other means.

I do not wish to imply that Sraffa himself does not believe in the value of what he calls the statical method, the use of the concepts of long-period positions and their comparisons. When he introduced the downward sloping demand curve analysis in 1926, with each firm its own little monopoly and so on, he said that he was dealing with situations where events stay steady enough for long enough to allow the statistical method to be used. "Many of the obstacles which break up that unity of the market which is the essential condition of competition are not of the nature of "frictions," but are themselves active forces which produce permanent and even cumulative effects... frequently... endowed with sufficient stability to enable them to be the subject of analysis based on statistical assumptions." (p. 542). It was Marshall’s use of the method in conjunction with supply and demand curves which he found objectionable. That is to say, Sraffa is not attacking the centres of gravity concept as such and, indeed, his entire book (1960) is about them—the pattern of the prices of production implied by the levels of activity and techniques of production in an economy at a given instant of time, when we make an assumption of a uniform wage rate (or rate of profit) to be included in them. Some usually orthodox economists have argued that Sraffa’s systems are already in, or are necessarily in, balanced or appropriate states, with the input-output arrangements and the prices of production being consistent with reproduction (and/or expansion). This, for example, is Joan Robinson’s interpretation of Sraffa’s system10; it is as if we had already reached the end of Marshall’s long period, with long-run values established (but not by the forces of supply and demand) and all fossils from previous epochs removed, and all production processes in their most appropriate form. But the fact that Sraffa stresses that there is no implication that what happened in the last period need necessarily happen again, or had happened before, or that the means of production used in this period necessarily came from the last period, or that those produced this period will necessarily be used to replace those used up this period, nor that there is any historical significance to be attached to his procedure of reduction to dated quantities of labour, suggests that this interpretation, though not unreasonable, is not necessarily what he had in mind. Rather, he takes the economy just as it is and asks: What pattern of prices of production is implied if we know (say) the wage rate and assume that a uniform rate of profit is to be received in each activity? And how do they alter when we alter the value of the wage rate? Of course, it can be argued, what is the point of such a procedure if there is not an implication of stability, so that those positions can serve to underlie the patterns of actual prices. These, of course, are influenced by temporary factors as well, yet we have isolated the effects of stable and fundamental forces by our procedures—this certainly is Garegnani’s argument and it is at this point that Joan Robinson (and Levine) part company with him.20

III

The same problems arise when considering Keynes’s system of thought in the General Theory, and as it has been adopted and developed in the Post-Keynesian macro theories of distribution. If his theories are to have operational content, it is argued, and, in particular, if they are to guide model builders and policy makers, we need to be able to say that the actual observations on national production, expenditure and income, and their components, are pretty good approximations to their theoretical counterparts. The latter themselves are the values that would be associated with the short-run rest states of the system that are implied in actual situations at any moment of time.21

We all know how we make this point in our first year classes: we draw the aggregate planned expenditure schedules (not Keynes’s aggregate demand schedule, which shows, rather, the total of what businessmen expect their receipts will be, for any given level of employment) and the 45 line (which, given certain, not unreasonable, assumptions about expectations and businessmen’s behaviour, may be interpreted as a functional relationship between the total of sales expected at any moment of time, including desired stock accumulation and the total of the production and, therefore, employment

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11Indeed, I knew from personal experience that it is not: it was at Sraffa’s insistence that Vincent Massey and I put the following paragraph in our 1964 Economic Journal notes on Sraffa’s Subject systems: "it must be stressed that the relationships in the economic system described here relate to one year only. They occur within the bounds of that year, and there is no necessary connection between them and the relationships that exist in other years... no implied that the means of production come from the immediately preceding year... that these parts are the gross product which are "used" to the means of production will be used as means of production in the following year." (p. 716).

20As Peter Groenewegen again reminds me, "this is the case of the problem of the interpretation and use of natural prices in economics, at least as Ricardo saw it."

21But see below for a discussion of whether they are short-run states.
that they will call forth from the existing short-run utilization functions. We argue that if the economy is not at the point of intersection of the two when we start the analysis, so that unintended changes in stocks occur, what changes are not of either of our two schedules but the businessmen's perceptions of what their expected sales are. They change their production and employment levels accordingly, collectively taking the economy closer to the point where what is expected and what actually happens coincide.

In other words, the initial non-fulfilment of investment plans leads not to their change but to a change in the perceptions of what sales are. In this way, there is a reasonable chance that the economy will not be too far away from its implied real states at any eoscient of time, unless, of course, it has been buffeted by a large shock or shocks.23 The point I want to make is that Keynes (and we) are using the concept of centres of gravity to tell this story, a procedure which Joan Robinson would regard as illegitimate, at least in some instances. Thus, she criticizes IS/LM analysis because unless the economy is actually at the intersection of the IS and LM curves, its actual movement will disturb the certainty factors underlying their construction; or to return to the textbook model, she argues that if the time income has moved to a level where planned saving is equal to a previous level of planned investment, the level of planned investment may well have changed. We then are required to scrap the short cut of using the statistical method and tell a much more sophisticated story of the initial failure to reach an implied real state changing the real state itself, by, for example, changing the rate of planned investment, especially in stocks. That is to say, we let expectations have their way.

23The same story may be told in terms of Keynes's aggregate demand and supply schedules, and the three cases of possible relations between short-run and long-run expectations and their feedbacks or lack thereof, which Kregel outlines in his 1976 Economic Journal article.

The Classical concept of a sustained rest state, a long-period position around which the actual positions fluctuated, may be inferred from, for example, the passages at the end of Book IV of the General Theory (Chapter 18, Part III, pp. 246-54). There, he states that "it is an outstanding characteristic of the economic system in which we live that, whilst it is subject to severe fluctuations in respect of output and employment, it is not violently unstable...seems capable of remaining in a chronic condition of subnormal activity for a considerable period without any marked tendency either towards recovery or towards complete collapse" (p. 249). Keynes discusses four conditions (relating to the characteristics of the consumption function, the marginal efficiency of capital schedule, the money-wage rate and the impact of achieved investment rates on the marginal efficiency of capital schedule) which "together are adequate to explain [why]...we oscillate, avoiding the gravest extremes of fluctuation in employment and in prices in both directions, around an intermediate position appropriately below full employment and appreciably above the minimum employment a decline below which would endanger life...a mean position...determined by "natural" tendencies...which are likely to persist" (p. 254). Sadly we see today the consequences of his views about the upward stability of money-wages (and therefore prices) not being borne out.24

24As everyone is indebted to Murray Milgate for bringing this point of view to my notice and making me re-read the General Theory with it in mind. John Kregel ("The Long Period According to Keynes", p. 5-6) interprets Keynes in this context as alternating in his analysis from the effects of accumulation (other than on employment), i.e., from changes in the dominant and persistent forces which lead to secular movements in the real states and the other centers of gravity e.g. normal prices. If we accept this interpretation, what I have called a short-run rest state, he calls a long-period position: "whence long-period is meant not that which occurs in a long period of time, but rather that which is determined by the dominant forces of the system within a period in which these forces are constant or changing slowly" (p. 4).

The same trouble with handling time, which as we have seen troubled Marshall too, also may be illustrated by Keynes's discussion of the marginal efficiency of capital and the reasons why it is a declining schedule. You will remember that he argued that the shorter is the time period involved, the stronger will be the influence of rising supply prices of capital goods is reducing the marginal efficiencies of particular assets; while the longer is the time period that we have in view, the greater will be the influence of expected? actual? falls in the prices of the products which the assets are to help to produce, as larger and larger supplies of the products come onto markets to be sold. Obviously, Marshall's influence is of major importance here. Equally obviously, the analysis won't do because we are trying to determine now what the rate of planned investment expenditure will be—there is not the time to wait for these processes to work themselves out, which they never would in fact, because of the factors that would escape from the cet. par. pound in the meantime.

IV

Exactly the same puzzles bound the concept of prices of production, especially when we try to incorporate them as operational concepts in an analysis of modern oligopolistic economies. It is not just that the existence of a tendency to equality of rates of profit in all activities may be questioned because of oligopolistic structures, barriers to entry and all the other paraphernalia of modern industrial organization analysis. It is, rather, that the dynamic nature of capitalists development with the embodiment of technical advances through investment expenditures is so rapid in most periods as not to allow sufficient historical time for centres of gravity of a lasting nature to be formed. There is not the time, as Joan Robinson puts it, for the traders to become familiar through actual
experience, with what is the norm, so that when their bearings are cut loose, they literally are all at sea, rudderless, not knowing where they are heading, either back or to. The factors that we need theoretically to take as constant in order to allow the centres of gravity which they imply to be struck (for example, by the forces making for the formation of normal prices) are changing as fast or even faster than the outcomes that the relationships between them are intended to determine. In making this point, though, we should notice Eastwell's comment (p. 2) that the forces which determine the centres of gravity are the more dominant, systematic and persistent and that "[w]hether this centre of gravity is a temporal constant, or takes different values through time, does not affect the essence of the method."

And yet: there must be a grain of truth—or more than—in the centres of gravity concept. It is true that Solow is sceptical of the validity of the great ratios, the stylized facts which it was his and Kaldor's, Joan Robinson's and Henry Phelps Brown's purpose to explain, i.e., he asks: Does what is explained exist or not? But we do know that well-established rules of thumb exist in the business world, that pay-off periods, or desired rates of return, for example, fall within definite ranges, where exactly depending on expectations, confidence and the extent and intensity of uncertainty at any moment of time. We know that wage earners have concepts of what is fair in relative wage structures, and in the overall share of wages, too, and that conservative politicians remind us, as they attempt to cut real wages, that they are trying to re-establish the historical share of profits without which it is not possible to expect the accumulators to accumulate. All of these notions derive from the macroeconomic characteristics of the system, are imposed by its total workings on the individual decision-makers and therefore have a macroeconomic basis, as Pasinetti, for example, often has stressed. Hence, we have had in the Keynesian-Kaleckian tradition the development of macro theories of distribution, the Post-Keynesian theories of the rate of profits, where $r - g$, with $g$ assumed to be independently given, mistakenly in my view, and corrected, of course, in von Neumann's model and in Joan Robinson's models. It is suggested that these factors feed through, in a very general way, to the determination of individual prices, and so on, and that it is when there are wide departures from them, that instability and crises are most in evidence, because microeconomic behaviour is not in accord with the macroeconomic constraints imposed by the structure of the system. Moreover, if we ally some of these notions with Srabia's contribution of production inter-relations, we get close to the normal cost pricing hypotheses where the expected trend values of costs are married to profit margins, sometimes constant, sometimes variable in order to take in investment expenditures and their finance. All of this grows out of Srabia's and then Keynes's criticisms of the original and simplest form of Marshall's explanation of pricing.

Thus, in Joan Robinson's version, $g = \frac{1}{2} g$, $g > 0$, $r - g, g = \frac{1}{3} g$ when $g > 0$, and $g = \frac{1}{2} g$ and $g$ may be selected for.

Nayudu, we now seem to have entered a stage in the history of political economy where both the centres of gravity themselves change rather rapidly and departures from them into the range of values where instability arises are more often the rule than the exception. I am indebted to Donlan Prosser for some very perceptive comments on this boldly stated theme.

This book critically surveys the neo-classical literature on sex differentials in hours of work, work per year, levels and types of education, job experience, on-the-job training, earnings, and unemployment. Chapter 1 summarizes the competitive neo-classical explanation for the secondary labor market status of women. Women allegedly approach the labor market "as tourists" and, therefore, choose 'little investment in education or skill development. Since they only desire work for a portion of any time period, women voluntarily shift the timing of their labor market activity in response to wage rate changes. Thus, sex differentials reflect labor supply-side factors resulting from sex differences in preference orderings. According to the authors, the competitive neo-classical explanation conflicts with a comprehensive analysis of the data. For example, while the neo-classical view contends that recent increases in sex differentials reflected the increased labor force participation rate (LFPR) of women with weak attachment to the labor market, the data indicates that the increased female LFPR was due to declining female labor force turnover.

At a theoretical level, the authors contend that only by including labor demand-side sex differentials, which they associate with a narrower version of labor market segmentation theory, it is possible to explain the size and changes in sex differentials. The next three chapters detail neo-classical supply-side explanations for the various sex differentials. Some of the more interesting claims made by the authors are: (1) the decline in birth rates is not the major explanation for the increasing female LFPR (p. 36); (2) most studies overestimate the short-run female labor supply elasticity (p. 52); (3) males and females are complements in child-rearing (p. 75); (4) men as well as women financially benefit from marriage (p. 121); and (5) neo-classical conceptions concerning the effect of rising education on age-earnings profiles are not consistent with the data (p. 170).

Chapter 5 presents the authors' views on why demand-side factors must be given an important place in any explanation of sex differentials. They critically evaluate virtually all econometric attempts to measure sex discrimination and contend that most estimates are too low because they standardize for factors which themselves reflect discrimination. The authors claim that statistical discrimination not only crowds women into the secondary labor market, which explains the positive correlation between female LFPRs and income differentials, but also results in biased promotion decisions within the primary sector, which explains sex differences in age-earnings profiles after standardizing for education and occupation.

Chapter 6 reviews various government policies. The authors note that present income tax provisions for married households result in the wife's income being taxed at the highest marginal tax rate. Similarly, social security provisions provide a strong disincentive for female labor market activity. The authors reject the mainstream contention that unemployment insurance differentially affects the duration of unemployment of married women. In their discussion of macroeconomic issues they tend to fall back on the traditional