

CONSUMERS' EXPENDITURE

by

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0. Introduction.

The study of consumers' expenditure, both in total and in composition, has always been of major concern to economists. Neoclassical economics sees the delivery of individual consumption as the main object of the economic system, so that the efficiency with which the economy achieves this goal is the criterion by which alternative systems, institutions and policies are to be judged. Within a capitalist economy, such considerations lead to an examination of the relationship between prices and consumption behavior, and theoretical development and empirical analysis have been a major continuous activity since the middle of the last century. Even older is the tradition of using individual household budgets to dramatize poverty, and the relationship between household incomes and household expenditure patterns has occupied social reformers, statisticians and econometricians since at least the eighteenth century. In more modern times, it has been recognized that the study of public finance and of taxation depends on a knowledge of how price changes affect the welfare and behavior of individuals, and the recent development of optimal tax theory and of tax reform analysis has placed additional demands on our understanding of the links between prices, expenditures, and welfare.

In the last fifty years, aggregate consumption has become as much as an object of attention as has its composition, and in spite of a common theoretical structure, there has been a considerable division of labor between macro economists, interested in aggregate consumption and saving, and micro economists whose main concern has been with composition, and with the study of the effects of relative prices on demand. The interest of macroeconomics reflects both long-term and short-term interests. What is not consumed is

saved, saving is thrift and the basis for capital formation, so that the determinants of saving are the determinants of future growth and prosperity. More immediately, aggregate consumption accounts for a large share of national income, typically more than three-quarters, so that fluctuations in behavior or "consumption shocks" have important consequences for output, employment, and the business cycle. Since Keynes' general theory, the consumption function, the relationship between consumption and income, has played a central role in the study of the macroeconomy. Since the 1930's, there has been a continuous flow of theoretical and empirical developments in consumption function research, and some of the outstanding scientific achievements in economics have been in this field.

In this essay, my major themes will be the interplay between theory and evidence in the study of consumers' expenditure and its composition. If economists have any serious claim to being scientists, it should be clearly visible here. The best minds in the profession have worked on the theory of consumption and on its empirical implementation, and there has always been more data available than could possibly be examined. I hope to show that there have been some stunning successes, where elegant models have yielded far from obvious predictions that have been well vindicated by the evidence. But there is much that remains to be done, and much that needs to be put right. Many of the standard presumptions of economics remain just that, assumptions unsupported by evidence, and while modern price theory is logically consistent and theoretically well developed, it is far from having that solid body of empirical support and proven usefulness that characterizes similar central theories in the natural sciences.

1. A simple theoretical framework

Almost all discussions of consumer behavior begin with a theory of individual behavior. I follow neoclassical tradition by supposing that such behavior can be described by the maximization of a utility function subject to suitable constraints. The axioms that justify utility maximization are mild, see any microeconomic text such as Varian (1978) or Deaton and Muellbauer (1980b), so that utility maximization should be seen as no more than a convenient framework that rules out the grossest kind of behavioral inconsistencies. The assumptions that have real force are those that detail the constraints facing individuals or else put specific structure on utility functions. Perhaps the most general specification of preferences that could be considered is one that is written

$$u_t = E_t(f(q_1, q_2, \dots, q_t, \dots, q_T)) \quad (1)$$

where u_t is utility at time t , E_t is the expectation operator for expectations formed at time t , q_1 to q_T are vectors of consumption in periods 1 through T , and $f(\)$ is a quasi-concave function that is non-decreasing in each of its arguments. Several things about this formulation are worth brief discussion. The function $f(\)$ yields the utility that would be obtained from the consumption vectors under certainty, and it represents the utility from a *life-time* of consumption; the indices 1 through T therefore represent age with 1 the date of birth and T that of death. The expectation operator is required because choice is made subject to uncertainty, not about the choices themselves, which are under the consumer's control, but about the consequences of current choices for future opportunities. It is not possible to travel backward through time, so that choices once made can-

not be undone, and yet the cost of current consumption in terms of future consumption foregone is uncertain, as is the amount of resources that may become available at future dates. The consumer must therefore travel through life, filling in the slots in (1) from left to right as best as he or she can, and at time (or age) t , everything to the left will be fixed and unchangeable, whether now seen to be optimal or not, while everything ahead of t is subject to the random buffeting of unexpected changes in interest rates, prices, and incomes. The solution to this sort of maximization problem has been elegantly characterized by Epstein (1975); here I shall work with something that is more restrictive but more useful, and note in Section 3 below some phenomena that are better handled by the more general model.

Intertemporal utility functions are frequently assumed to be *intertemporally additive*, so that the preference rankings between consumption bundles in any two periods or ages are taken to be independent of consumption levels in any third period. If so, the utility function (1) takes the more mathematically convenient form

$$u_t = E_t \sum_{r=0}^T v_r(q_r). \quad (2)$$

Note that by writing utility in the form (2), and since the expectation operator is additive over states of the world, preferences are being assumed to be *simultaneously* additive over both states and periods, an assumption that can be formally defended, see Gorman (1982) and Browning, Deaton, and Irish (1985), and which has the consequence that risk aversion and intertemporal substitutability become two aspects of the same phenomenon. Individuals that dislike risk and will pay to avoid it will also attempt to

smooth their consumption over time and will require large incentives to alter their preferred consumption and saving profiles. Note also that the additive structure of (2) means that, unlike the case of (1), previous decisions are irrelevant for current ones. For decision making at time t , bygones are bygones, and conditional on asset and income positions, future choices are unaffected by what has happened in the past. There can therefore be no attempt to make up for lost opportunities, nor can such phenomena as habit formation be easily modeled.

Because utility in (2) is intertemporally separable, maximization of life-time utility implies that, within each period, the period subutility function $v_t(\cdot)$ must be maximized subject to whatever total it is optimal to spend in that period. The period by period allocation of consumption expenditure to individual commodities need not, therefore, be planned in advance, but can be left to be determined when that period or age is reached, and period t allocation will follow according to the rule

$$\text{maximize } v_t(q_t) \text{ subject to } p_t \cdot q_t = x_t, \quad (3)$$

where p_t is the price vector corresponding to q_t , and x_t is the total amount to be spent in t . Problem (3) is one of standard (static) utility maximization, though note that x_t is not given to the consumer, but is determined by the wider intertemporal choice problem. Nevertheless, not the least advantage of the intertemporally additive formulation is its implication that the composition of expenditure follows the standard utility maximization rule. It allows separate attention to be given to demand analysis on the one hand, i.e. to the problem (3), and to the consumption

function on the other hand, this being understood to be the intertemporal allocation of resources, i.e. the determination of x_t .

Write the maximized value of utility from the period t problem as $\psi_t(x_t, p_t)$, where $\psi(\)$ is a standard indirect utility function. The original intertemporal utility function then takes the form

$$u_t = E_t \sum_{r=0}^{T-t} \psi_r(x_{t+r}, p_{t+r}). \quad (4)$$

The constraints under which this function is maximized are most conveniently analyzed through the conditions governing the evolution of wealth from period to period. If A_t is the (ex-dividend) value of assets at the start of period t , N_{it} is the nominal holdings of asset i with price P_{it} , d_{it} is the dividend on i paid immediately before the beginning of t , and y_t is income in period t , then

$$A_{t+1} = \sum_i N_{it} (P_{it+1} + d_{it+1}) \quad (5)$$

$$\sum_i N_{it} P_{it} = A_t + y_t - x_t \quad (6)$$

Conditions (5) and (6) determine how wealth evolves from period to period, and the picture is completed by requiring that the consumer's terminal assets be positive, i.e.

$$A_{T+1} \geq 0 \quad (7)$$

To solve this problem, the technique of backward recursion is used. This rests on the observation that it is impossible to know what to do in period t without taking into account the problem in period $(t+1)$, nor that in $(t+1)$

without thinking about $(t+2)$, and so on. However, in period T there is no future, so that looking ahead from date t , we can write subutility in period T in terms of that period's price and inherited assets, and we write this as v_T , i.e.

$$v_T = v_T(A_T) = \psi_T(A_T + y_T, p_T). \quad (8)$$

Given this, the consumer can look ahead from period t to period $(T-1)$ and foresee that the problem then will be to choose the composition of assets N so as to maximize v_{T-1} , where

$$v_{T-1}(A_{T-1}) = \max_N [\psi_{T-1}(A_{T-1} + y_{T-1} - N \cdot P_{T-1}) + E_{T-1}\{v_T(N \cdot (P_T + d_T))\}]. \quad (9)$$

At the next stage, assets in $(T-2)$ will be allocated so as to trade off the benefits of consumption in $(T-2)$ versus the benefits of A_{T-1} in v_{T-1} in (9) above and again yielding a maximized value v_{T-2} . As we follow this back through time, the consumer finally reaches the current period t , where he or she faces an only slightly complicated version of the usual "today tomorrow" trade-off; the asset vector N must be chosen to solve the problem,

$$u_t = \max_N [\psi_t(A_t + y_t - N \cdot P_t) + E_t\{v_{t+1}(N \cdot (P_{t+1} + d_{t+1}))\}]. \quad (10)$$

From this sequence of problems, several important results readily follow. Firstly, consider the derivatives of each of the functions $v_r(A_r)$ which represent the marginal value of an extra unit of currency for the remaining segment of life time utility from r through to T . By the envelope theorem, see for example Dixit (1976) for a good exposition, it is legitimate to differentiate through the maximization problem, so that we have at once

$$v_r'(A_r) = \partial \psi_r / \partial x_r = \lambda_r, \text{ say,} \quad (11)$$

so that λ_r is the marginal utility of money in period r . Secondly, the maximization of (10) with respect to portfolio choice gives the relationship, for each asset i ,

$$P_{it} \partial \psi_t / \partial x_t = E_t \{ (P_{it+1} + d_{it+1}) \partial \psi_{t+1} / \partial x_{t+1} \} \quad (12)$$

which, defining the asset return R_{it+1} as $(P_{it+1} + d_{it+1})/P_{it}$, and using (11) can be rewritten in the simple form

$$\lambda_t = E_t (\lambda_{t+1} R_{it+1}). \quad (13)$$

This equation, in current parlance often referred to as the "Euler equation," can be used to derive many of the implications of the theory of consumption. Note first that it is little more than the standard result that the marginal rate of substitution between today's and tomorrow's consumption should be equal to the relative price. However, the equation is set in a multiperiod framework, not a two-period one, and it explicitly recognizes the uncertainty in both asset returns and in the value of money in subsequent periods. The equation also holds for all i , i.e. for all assets, so that the result also has implications for asset pricing as well as for consumption and saving, and for this reason the model is often referred to as the consumption-asset pricing model. I shall return to these implications below.

The theory as presented above is the modern equivalent of the life-cycle theory of consumption that dates back to Irving Fisher (1930), and Frank Ramsey (1928), and that had its modern genesis in the papers by Modigliani and Brumberg (1954) and (1954, published 1979). Modigliani and Brumberg's

treatment differs from the above only in not explicitly modelling uncertainty, and by including only a single asset. The modern version appears first in Breeden (1979) and in Hall (1978), see also Grossman and Shiller (1981).

2. Predictions and evidence

One of the most important implications of the theory above, and of equation (13) in particular, is that the evolution of consumption over the life-cycle is independent of the pattern of income over the life-cycle. The asset evolution equations (5) and (6) allow consumers to borrow and lend at will, so that the only ultimate constraint on their consumption is one of life-time solvency. In consequence, consumption patterns are free to follow tastes, the evolution of family structure, or the different needs that come with ageing, provided that in the end, total life-time expenditure lies within total life-time resources, whether from inherited wealth, or from labor income. It is often assumed that tastes are such that consumers prefer to have a relatively smooth consumption stream, and this can be illustrated from a special case of equation (13). Assume that the within period utility function is homothetic so that $\psi(x,p)$ is $\phi(x/a(p))$ for some linearly homogeneous function $a(p)$, and that $\phi(\cdot)$ has the isoelastic form with elasticity $(1-\sigma)$. Life time utility takes the form

$$u_t = \sum_{r=0}^T (1+\delta)^{-r} (x_{t+r}/a(p_{t+r}))^{1-\sigma}. \quad (14)$$

where δ is the rate of pure time preference, and $\sigma \geq 0$ is the coefficient of relative risk aversion and the reciprocal of the intertemporal elasticity of

substitution. Equation (14) can be used to evaluate (13), and gives immediately

$$E[\{(1+r_{t+1})/(1+\delta)\}(c_t/c_{t+1})^\sigma] = 1 \quad (15)$$

where r_{t+1} is the real after tax rate of interest from t to $t+1$ on any asset, and c_t is real consumption, $x_t/a(p_t)$. Equation (15) shows that, if expectations are fulfilled, consumption will grow over the life cycle if the real rate of interest is greater than the rate of pure time preference, and vice versa, while with $r_t = \delta$, consumption is constant with age. These results are of course an artefact of the specific assumptions about utility, and for any real household, consumption can be expected to vary predictably with age according to patterns of family formation, growth, and ageing; Modigliani and Ando (1957) have suggested that consumption per "equivalent adult" might be constant over the life-cycle. But whatever the shape of preferences, there need be no relationship between the profiles of consumption and of income; income can be saved until it is needed, or borrowed against if it is not yet available.

Independent of the life-time pattern of consumption is its level, which under the life-cycle model is determined by the level of total life time resources, so that individuals with the same tastes but with higher incomes or higher inherited assets will have higher levels of consumption throughout their lives. If the future were entirely predictable, the consumption plan at any point in time could be decided with reference to the level of total wealth, this being the value of financial assets and the discounted present value of current and future incomes. In this sense, the life-cycle model is a permanent income theory of consumption, where permanent income is the

annuity value of life-time wealth, though the life-time interpretation is only one of the many that are offered in Friedman's (1957) original statement. Whether life cycle or not, linking consumption to *future* incomes has important consequences. First, consumption will respond only to "surprises" or "shocks" in income; changes in income that have been foreseen are already discounted in previous behavior and should not induce any changes in plans. Of course, this does *not* mean that consumption will not change along with changes in income; a change may have been planned in any case, and some proportion of any actual change may well have been unforeseen. However, if a substantial fraction of the regular changes in income over the business cycle are foreseen by consumers, or if unanticipated fluctuations in income are regarded as only temporary with limited consequences for total life-time resources, then consumption will not respond very much to cyclical fluctuations in income. Aggregate consumption is indeed much smoother than is aggregate income, and this has been traditionally accepted as an important piece of confirmatory evidence. I shall take up the matter again below when I deal with the recent econometric evidence.

The distinction between measured income and permanent income is also important for the interpretation of cross-sectional evidence. Since measured income can be regarded as an error-ridden proxy for permanent income, the regression of consumption on measured income will be biased downward (rotated clockwise) compared with the true regression of consumption on permanent income. Cross-sectional regressions, or time-series regressions of simple Keynesian consumption functions will therefore tend to understate the long-run marginal propensity to consume. Well before the work on life-cycle models, Kuznets (1946) showed that the long-run saving

ratio in the United States had been roughly constant in spite of repeated cross-sectional analyses showing that the saving ratio rose with income, and the life-cycle theory could also readily account for these findings. It is interesting to note that the constancy of the saving ratio is far from being well established as an empirical fact; the evidence for other countries with long-run data is very mixed, and even the United States saving ratio is clearly influenced in the long-run by technical change, migration patterns, and demographic shifts, see Kuznets (1962) and Deaton (1975). Life-cycle and permanent income theories also predict that households with atypically high income will tend to save a great deal of it, a prediction which explained the apparently anomalous finding that black households tend to save more than white households at the same level of measured income; since blacks typically have lower household income than whites, those with the same measured income can be expected to have a higher transitory component.

The Modigliani and Brumberg life-cycle story was also important because it offered a story of capital accumulation in society as a whole that relied on the way in which people made preparation for their own futures, particularly for their future retirement. In a stationary life-cycle economy, in which there is neither economic nor population growth, aggregate saving is zero, and the old, as they dissave, pass on the ownership of the capital stock to the next generation who are, in turn, saving for their own retirement. With either population or income growth, the aggregate scale of saving by the young would be greater than that of dissaving by the old, so that, to a first approximation, the aggregate saving ratio, while in the long-run independent of the level of national income, would depend on the sum of its population and per capita real income growth rates. Modigliani

(1986), in his Nobel address, has given an account of how very simple stylized models of saving and retirement yield quite accurate predictions of the saving ratio and of the ratio of wealth to national income, and the predictions about the growth effects have been repeatedly borne out in international comparisons of saving rates, see Modigliani (1970), Houthakker (1961, 1965), Leff (1969), and Surrey (1974). Perhaps the only problem with these interpretations is that there is little evidence that the old actually dissave, except by running down state social security or pension schemes, see for example Mirer (1979). Partly, this may be a rational response to uncertainty about the date of death and about possible medical expenses near the end of life, Davies (1980), partly there may be statistical problems of measurement, Shorrocks (1975), and partly consumers may wish to leave bequests. However, most countries' tax systems penalize donors who do not pass on assets prior to death, so the reason for the size of actual bequests remains something of a mystery. Bernheim, Schleiffer and Summers (1985) have gone so far as to suggest that parents retain their wealth until death in order to control their heirs and to solicit attention from them. They claim empirical support for a positive relationship between visits by children to their parents and parents' bequeathable assets; visits are apparently especially frequent to rich, sick, parents, but not at all frequent to poor sick parents. Related to the dispute about the reason for bequests is a parallel dispute on their importance in the transmission of the capital stock, see the original contribution by Kotlikoff and Summers (1981) and Modigliani's reply, summarized in his (1986) Nobel lecture.

The life-cycle and permanent income models also provided the econometric specifications for a generation of macroeconometric models. Ando and

Modigliani (1963) suggested a simple form for the aggregate consumption function in which real aggregate consumption was a linear function of expected real labor income, YL , and of the real value of financial assets, i.e.

$$c_t = \alpha E_t(YL) + \delta W_t \quad (16)$$

In practical econometric work, the expectation was typically replaced by a linear function of current and past values of labor income, a procedure that can be formally justified by modeling labor income as a linear ARIMA process, a topic to which I shall return below. Wealth or a subset of wealth was included as data allowed, although sometimes the return to wealth was included with labor income which could then be replaced by total income, so that, with smoothing, (16) becomes a permanent (total) income model of consumption. A favorite variant, suggested in Friedman (1957), was to model permanent income as an infinite moving average of current income with geometrically declining weights,

$$y^p_t = (1-\lambda)\sum \lambda^r y_{t-r}, \quad (17)$$

so that if current consumption is proportional to permanent income, substitution yields

$$c_t = k c_{t-1} + k(1-\lambda)y_t \quad (18)$$

a formulation that is also easy to defend if consumers "partially adjust" to changes in current income. Models like (18), possibly with additional lags, and with the occasional appearance of more or less "exotic" regressors, such as wealth, interest rates, inflation rates, money supply, as well as various

dummy variables for "problem" observations, were the standard fare of macro-econometric models in their heyday, from the early sixties for about a decade and a half. They fit the data well, they accounted for the smoothness of consumption relative to income, and they accorded at least roughly with the general features of the life-cycle and permanent income formulations which provided them with pedigree and general theoretical legitimacy. Dozens of papers could be cited within this tradition; those by Stone (1964), (1966), Evans (1967), Davidson, Hendry, Srba, and Yeo (1978) will perhaps stand as good examples.

3. Recent econometric experience

In the mid-1970's, the general state of complacency of macroeconomic modelling was rapidly eroded, largely by the apparent inability of the standard models to explain, let alone to predict, the co-existence of unemployment and inflation. The relationship between consumption and income did not escape some of the blame, although the main focus of attack was elsewhere. Standard consumption functions that had worked well into the early seventies seriously under predicted aggregate saving during the period of (at least relatively) rapid inflation that characterized most Western economies in the middle of the decade. The implementation of the theory of the consumption function was also singled out for discussion in Lucas' famous (1976) essay that became known as the Lucas "critique." As Lucas forcefully argued, if consumption is determined by the discounted present value of *expected* future incomes, the response of consumption to a change in income is not well-defined until we know how expectations of income are formed. Each observed realization will cause a re-evaluation of future

prospects in accordance with formulae that depend on the nature of the stochastic process governing income. If the nature of the stochastic process is changed, for example by a fundamental change in the tax code, then the way in which information is processed will change, and new information about incomes will have different implications for future expectations and for future consumption. This insight is of great importance, although its implications for econometric modelling were initially taken much too negatively; if the rules keep changing, econometric models will be inherently unstable (as evidenced by their performance in the mid-seventies) and we should give up trying to find stable relationships. Instead, as events have shown, the introduction of rational expectations has given a whole new lease of life to the study of consumption, with developments as positive as anything that has happened since the life-cycle and permanent income models were the "new" theories in the mid-fifties. Lucas' critique suggested at least two lines for research. First, could the failure of consumption functions, or indeed of macroeconometric models in general, really be traced to a change in the way expectations were formed? If so, it ought to be possible to detect changes in the stochastic process generating real income. Second, and more generally, if expectations are important, there ought to be high returns to the simultaneous modelling of consumption and income, so that knowledge of the structure of the latter can be used either to estimate the consumption function, or to test for the validity of the expectations mechanism. My own reading of the evidence is that the Lucas critique is not capable of explaining the failure of the empirical consumption function, but that the under prediction of saving resulted from ignorance of the fact that saving appears to respond positively to inflation, or at least to unanti-

cipated inflation. There is overwhelming evidence from a large number of countries, see in particular Koskela and Viren (1982a,b), that saving increased with inflation in the 1970's, even when we allow for real income and its various lags. Such a finding is also consistent with the life-cycle theory since unanticipated inflation imparts a negative shock to real assets, so that risk-averse, low intertemporal elasticity consumers will save to replace the lost assets so as to avoid the chance of low consumption later. It is also possible to explain the relationship through the confusion between relative and absolute price changes that is engendered by unanticipated inflation in an environment in which goods are bought sequentially, see Deaton (1977), but it would be hard to devise a test that would separate this from the life-cycle explanation. But if inflation was indeed the cause of the failure of the empirical consumption functions, then it is a standard enough story. An important variable was omitted from the analysis, it had not been very variable in the past so that its omission was hard to detect, and economists had not been imaginative enough to perceive its importance in advance. The Lucas' critique is only one of the many problems that can beset an econometric equation, and it does not seem to have been the fatal one in this case.

The second research direction, the joint examination of income and consumption has proved more productive. The first important step was taken by Hall (1978), who pointed out that equation (15) implies that, as an approximation, consumption should follow a random walk with drift. To see why, assume that the real interest rate r is constant and known, and write (15) in the form

$$c_{t+1}^{-\sigma} = ((1+\delta)/(1+r))c_t^{-\sigma} + \epsilon_{t+1} \quad (19)$$

where the expectation at t of ϵ_{t+1} is zero. Equation (19) is exact, but a convenient expression can be reached by factoring c_t out of the right hand side, taking logarithms, and approximating. This gives

$$\ln c_{t+1} = \ln c_t + g + v_{t+1} \quad (20)$$

where g is positive or negative as r is greater than or less than δ , and the "innovation" v_{t+1} , like ϵ_{t+1} , has expectation zero at time t . Equation (20) shows that, in the absence of "news," consumption will grow or decline at a steady rate g , so that nothing that is known by the consumer at time t or earlier should have any value for predicting the deviation of the rate of change of consumption from its constant mean. The result is often referred to as the "random walk" property of consumption, though the theory does not predict that v_{t+1} has constant variance, so that, strictly speaking, the stochastic process is not a random walk.

For someone used to thinking about the consumption function as the relationship between consumption and income, equation (20) is notable for the apparent absence of any reference to income. But of course income can appear through the stochastic term v_{t+1} if current income contains new information about its own value or about future values of income, and this will generally be the case. The random walk model does not predict that consumption should not respond to current income. It does however predict that, conditional on lagged consumption, past income or changes in income should not be correlated with the current change in consumption, and a considerable amount of effort has recently gone into testing this proposition. In Hall's (1978) original paper, to the surprise of the author

and of much of the profession, the model worked well for an aggregate of U.S. consumption of non-durables and services. The level of consumption certainly depends on its own lagged value, but the addition of one or more lagged values of income or of further lagged values of consumption did not significantly add to the explanatory power of the model. Hall examined the role of a number of other lagged variables, and discovered that lagged stock-market prices had predictive power for the change in consumption, so that he concluded by formally rejecting the model. However, the overwhelming impression was favorable, at least relative to expectations.

Hall's test procedures are attractive because they do not depend on the properties of the income process, and focus only on consumption and its lags. But robustness comes at the price of power, and later work has devoted considerable attention to the joint properties of consumption and real income. Perhaps the natural route to modeling is to find a representation of real income as a stochastic process, typically as some sort of ARIMA. Once this is known, changes in income can be decomposed into anticipated and unanticipated components using the standard forecasting formulae from statistical time series analysis, so that it becomes possible to test whether consumption responds to one but not to the other. The random walk model seemed not to survive these tests so well. Papers by Flavin (1981) and by Hayashi (1982) showed that, for U.S. data, consumption is sensitive to *anticipated* changes in income, something that should not be the case in a thoroughgoing life-cycle model in which consumers are efficiently looking into the future. The phenomenon became known as the "excess sensitivity" result, and was typically ascribed to the existence of a substantial number of consumers who wish to borrow against future income but are unable to do

so. Such liquidity constrained consumers can be expected to consume all their available income, so that their consumption will increase one for one with *all* income changes, whether anticipated or not. However, it is not clear that the excess sensitivity finding is itself robust. First, it is becoming increasingly recognized that there are problems of econometric testing in the time-series models are more severe than had been generally supposed. The time series of both consumption and income are non-stationary, and it sometimes seems as if hypothesis testing in models involving non-stationary variables is a like building on shifting sands; see Mankiw and Shapiro (1985,1986) and Durlauf and Phillips (1986) for some of the problems. Second, there are a large number of variables other than income which can affect consumption, so that, according to (20), surprises in wealth and in inflation should affect consumption, as should the level of real interest rates. Adding even a few of these variables reduces degrees of freedom and diminishes the probability of being able to reject the basic model. Both Bean (1985) and Blinder and Deaton (1985) find that time-series models of consumption with several variables are more easily reconciled with the theory than are the simple two variable models. Not all of this should be ascribed to lack of degrees of freedom; for example Blinder and Deaton consistently find that unanticipated changes in wealth affect consumption and that anticipated changes do not. Third, even in a bivariate income-consumption model, Campbell (1985) has found that the model is largely consistent with the time series evidence. Campbell recognizes the possibility of time-series feedback from lagged consumption to income, and models saving and the change in income as a bivariate vector-autoregressive system in which each series is regressed on lagged values of both. The

structure of this representation then turns out to be very close to what it would have to be if the life-cycle rational expectations model were correct. The conflict between Campbell's results and the excess sensitivity findings are presumably accounted for by the feedback from saving to changes in labor income, since his model is otherwise compatible with the earlier ones.

Similarly mixed findings are also being uncovered from longitudinal panels that follow individual households over time. In contrast to the situation with labor supply, there are few panel data in the U.S. that cover household consumption, and most work has used the data on expenditure on food that is contained in the Michigan Panel Study of Income Dynamics (PSID). In an elegant paper, Hall and Mishkin (1982) found results that were in accord with the excess sensitivity results; there is a strong negative correlation in their data between changes in consumption and changes in lagged income that is inconsistent with the view that only surprises in income should matter. However, since in their data changes in income are negatively correlated over time, a negative correlation between the lagged income change and the change in consumption can be interpreted as a positive correlation between consumption changes and changes in actual income, as predicted by the model of liquidity constraints. Hall and Mishkin conclude that their results would be consistent with a model in which about one fifth of consumers were unable to borrow as much as they wished. Once again, these results were supported by other similar evidence, see in particular Zeldes (1985) and Bernanke (1985), also using the PSID, Runkle (1983), using data from the Denver Income Maintenance Experiment, and Hayashi (1985) using panel data from Japan. However, one potential problem with the use of panels is the importance of errors of measurement in such

data. There is a considerable body of evidence that PSID income changes are subject to very substantial reporting errors, see in particular Altonji (1986), Duncan and Hill (1985), and Abowd and Card (1985). Altonji and Siow (1985) have recently estimated a model similar to Hall and Mishkin's using the PSID but with allowance for measurement error, and they find little conflict with the view that consumption responds only to news. However, it is unclear, at least to this reader, whether the acceptance of the model represents low power once errors of measurement are allowed for, or whether such errors really offer a plausible explanation for Hall and Mishkin's findings.

A more formal line of research has attempted to estimate the Euler condition (15) directly, thus avoiding the approximations made by Hall and by others. Rewrite (15) once more, this time as

$$(1+r_{t+1})(c_{t+1})^{-\sigma} - (1+\delta)(c_t)^{-\sigma} = \epsilon_{t+1} \quad (21)$$

where, as before ϵ_{t+1} is orthogonal to any variable known in period t or earlier. Hansen and Singleton (1982) proposed that the parameters in (21) be estimated by a generalized methods of moments scheme. Suppose that we have two variables or instruments z_{1t} and z_{2t} , each known at time t , so that we have $E_t(z_{it}\epsilon_{t+1})=0$ for $i=1,2$. We can then estimate the two unknown parameters, σ and δ , by equating sample and theoretical moments, and solving the two equations, $i=1,2$

$$T^{-1}\sum[z_{it}\{(1+r_{t+1})(c_{t+1})^{-\sigma} - (1+\delta)(c_t)^{-\sigma}\}] = 0. \quad (22)$$

If, as is typically the case, we have more than two z -variables, then it will not generally be possible to choose the two parameters so that (22) is

exactly zero. Instead, the vector can be made as small as possible, or more specifically, the parameters can be estimated by minimizing a quadratic form that can be thought of as a weighted sum of squares of the left-hand side of (22), see Hansen and Singleton for details. If the model were true, this minimized value ought to be small, so that with more instruments than parameters, the generalized method of moments procedure yields a test-statistic that is diagnostic for model adequacy.

Test procedures based directly on the Euler conditions have several notable advantages. As was the case for Hall's procedures, few assumptions have to be made about the structure of the income process, and the model satisfies the best professional standards of seeking a direct confrontation between theory and data with as few approximations and supplementary assumptions as possible. The model can also be readily extended to test the implications of the consumption asset pricing model by repeating the tests using the returns on a range of alternative assets, see (13) above. Hansen and Singleton's study, as well as several others, find that the test statistics are much too large to be consistent with the theory and so reject the intertemporal model implied by the Euler conditions. Given the apparent superiority of the tests, these results have been accorded a great deal of weight in the literature. However, while I believe that Hansen and Singleton's work represents a very important methodological advance, I think that there are good reasons for not treating their results as a definitive rejection of life-cycle theory. The high level of technique that is embodied in deriving the Euler equation, not to mention the complexity of generalized methods of moments estimation, should not blind us to the very simple, even simple-minded, economic story that underlies these models. Fundamentally,

the Euler equation says that the marginal rate of substitution between today's and tomorrow's consumption should be equal to the rate of return on assets between today and tomorrow, so that estimation of the Euler equation, unlike the Hall or excess-sensitivity tests, focuses very directly on the relationship between real interest rates and changes in real consumption, and the model will not fit the data if there is no close association between the two. And it only takes a very cursory inspection of U.S. time-series data to see that there is no such association. Real consumption grew in all but one year between 1954 and 1984, while real after tax interest rates were as often negative as positive, so that consistency with the theory would require that the pure rate of time preference be negative. Nor is there any association between the rate of growth of consumption and the level of real after tax interest rates, see Deaton (1986b) for some data. But this in no way reflects badly on the life-cycle theory. As was made perfectly clear in the original Modigliani and Brumberg papers, and it is the essence of the life-cycle model, aggregate consumption cannot be expected to behave like individual consumption. Imagine a stationary economy with neither population nor real income growth, in which there is an excess of real interest rates over the rate of pure time preference, and in which all consumers have identical additive life-time preferences with isoelastic subutility functions. In such an economy, each individual has a consumption path that is growing over time, but aggregate consumption is constant, a result that is achieved by old-people dying and being replaced by young people who have much lower consumption levels relative to their incomes. Unless we believe that there is some automatic and immediate relationship between real interest rates, time preference and growth, as would obtain for example along a

"golden age" growth path, or unless we believe that consumers have infinite lives, then there is no reason at all to suppose that aggregate consumption should look at all like the life-cycle path of a representative consumer. Representative agent models are frequently useful, and it is not very constructive to dismiss macroeconomics because it requires implausible aggregation assumptions. However, the life-cycle model provides a well-worked out account of individual and aggregate saving, an account that is consistent with a good deal of other evidence and theory, and it *does not* predict that aggregate consumption should be consistent with the intertemporal optimization conditions for a single individual. The general question of the effects of interest rates on consumption is something that has remained in dispute for a long time, and in spite of repeated attempts to isolate the effect, careful studies have tended to be unable to do so, or at least to find effects that are at all robust, or that can be replicated on even slightly different data sets or data periods. Economic theories or policy prescriptions that rely on intertemporal substitution of consumption in response to changes in real interest rates are not well-buttressed by any solid body of empirical evidence.

Another useful approach to testing the life-cycle model is to consider the stylized facts of the income and consumption processes, and to see whether consumption behaves in the way that is to be expected given the stochastic process of income. Most people who have studied the time series for quarterly real disposable income in the U.S. agree that, like GDP, the series can be parsimoniously described by a model that is linear in its first two lags, i.e. an autoregression of the form

$$y_t = \alpha_1 + \alpha_2 y_{t-1} + \alpha_3 y_{t-2} + u_t \quad (23)$$

where u_t is the income innovation, that part of current income that cannot be anticipated from previous observation of the series. Of course, real income is not a stationary series, but has a strong upward trend, and there is considerable disagreement about the nature of this trend, what is the economic story behind it, and how it should be modeled. One possibility is that real income contains a *deterministic* time trend, so that there is some sort of equilibrium growth path that cannot be altered by shocks to the economy. Shocks certainly exist, but they cause only short term temporary deviations from the path and have little or no long term significance. In this view, equation (23) applies to the *deviations* of income from trend, not to income itself; equivalently, (23) can be modified by including a linear or quadratic time trend. The alternative view is that there is no deterministic trend, but that the *rate of change* of income is a stationary stochastic series with constant mean. In practice, this can look very like the previous model, but there is the vital conceptual difference that in the second, non-deterministic model, there is nothing that will ever bring income back to any deterministic path. In consequence, shocks to current income have *permanent* and long-lasting effects. The version of (23) that corresponds to this view can be written

$$((y_t - y_{t-1}) - \gamma) = \rho((y_{t-1} - y_{t-2}) - \gamma) + u_t \quad (24)$$

which can readily be seen to be a special case of (23), though note that it is the case where the time series possesses a unit root, or is stationary in first differences. For (24) to be a valid specialization of (23), the quadratic equation with the α 's of (23) as coefficients must have a unit

root, hence the term. Equation (24) appears to fit the data well and the parameter ρ turns out to be around 0.4, so that (24) says that if the increase in real income in one quarter is greater than its long term mean, then the next quarter's increase is also likely to be above the mean, though by less. While the long-term mean of the rate of change of income is constant and equal to γ , good fortune (positive u 's) and bad fortune (negative u 's) never has to be paid for (or made up), since shocks are immediately consolidated into the income level, and growth goes on in the same way as before, but from the new base. As Campbell and Mankiw (1986) have emphasized, the unit root model exhibits shock persistence, while the deterministic trend model does not; they suggest that shock persistence is what we should expect if supply shocks predominate over demand shocks, with the reverse in standard Keynesian models where shocks are typically attributed to fluctuations in aggregate demand.

It turns out that it is almost impossible to tell these two processes apart on U.S. time series data. Processes with unit roots are inherently difficult to tell apart from processes that are stationary around deterministic trends, and the tests that are available, Dickey and Fuller (1981), Phillips and Perron (1986), certainly cannot reject the hypothesis that (24) is a valid specialization of (23). Nor would the tests convince a believer in the deterministic model that income does not have a deterministic trend, even though it will readily be recognized that the deviations from trend are themselves close to non-stationarity. Since both processes are special cases of (23) with the inclusion of a time trend, and since each assumes parameter values that are very close to one another, one might think (and hope) that the two models would have very similar implications. But it is

easy to see this is not true. If permanent income is taken as the annuity value of discounted future incomes, then (24) implies that any innovation u_t to current income, because it will persist for ever, and because it can be expected to be followed by another infinitely persistent innovation of the same sign, will change permanent income by more than the amount of the innovation. Equation (25) below gives the formula for the change in permanent income, if the real interest rate is r , and if real income follows (24), see Flavin (1981) or Deaton (1986b),

$$\Delta y^P_t = \frac{(1+r)^2}{r+1-\rho} u_t \quad (25)$$

so that the change in permanent income is between one and a half and twice as large as the innovation in current income. By contrast, fitting the deterministic model yields a much smaller effect, with the change in permanent income about one fifth of the shock in measured income. Since consumption should change by about the same amount as does permanent income, the life-cycle model, together with the unit root formulation, yields the uncomfortable prediction that consumption should be more variable than income over the business-cycle, not less. If the unit root model is correct, then the life-cycle and permanent income models can be rejected because they predict what they were designed to predict, that consumption is smooth relative to real income! The deterministic model gives no such problems, but as yet we have no way of being sure that it is correct, unless, of course we assume from the start that the life-cycle story is true.

There is insufficient space in this essay to follow these issues further, or to discuss in detail the evidence for and against the two formulations of

the stochastic process governing real income; the interested reader can refer to Deaton (1986b) and to the evidence on persistence in GDP presented by Campbell and Mankiw (1986) and by Cochrane (1986). There are a number of possible solutions to these puzzles, and a great deal of empirical work remains to be done, though I suspect that the time-series data on income are insufficiently long to allow the isolation of the very long-run properties on which the permanent income theory rests, see in particular the interesting paper by Watson (1986).

4. Variations on the basic theme

There exist many interesting developments of the basic life-cycle model, and I have space to discuss only a few. I have already mentioned the role of liquidity constraints, and many people would take it as transparent that many consumers do not have access to unlimited credit, or else face borrowing rates that are higher than the rates at which they can lend. Of course, many consumers may be able to smooth their consumption without recourse to borrowing, and the borrowing needs of many others may be met by the typically rather good markets in home mortgages. For consumers who nevertheless wish to borrow but cannot, their spending will be closely tied to their actual income. For some of the theoretical and empirical literature on this point see Flemming (1973), Dolde and Tobin (1971), and Hayashi (1985). The theoretical consequences of uncertainty about the date of death have been worked out by Yaari (1965), and as argued above, play a possibly important part in the explanation of the saving behavior of the elderly.

Another line of research is the possible relaxation of the assumption that preferences are intertemporally additive. Allowing all periods (or

ages) to interact with all other periods in an unrestricted way, as in equation (1), would be much too general to be useful, and the search has been for simple models that break the restriction in natural and straightforward way. One useful analogy is with the theory of durable good purchases, where utility depends on the stock of assets possessed, the stock in turn being the integral of past purchases less depreciation. Purchases in one period therefore have consequences for utility in subsequent periods, something that will be taken into account by a forward looking consumer. In the case of durable goods, the assumption of perfect capital markets effectively converts durable into non-durable goods, with the price of a unit of stock for one period being the implicit rental or user cost, the latter being defined as the sum of interest cost, depreciation, and expected capital loss, see for example Diewert (1974) or Deaton and Muellbauer (1980b, Chapter 13). However, various authors, Houthakker and Taylor (1970) perhaps being the first, have extended the durable model to encompass "psychic" stocks which, like physical stocks, are augmented by purchases and diminished by depreciation, but unlike physical stocks, can either increase or decrease utility. The latter case covers habit formation; consumption of an addictive good generates pleasure now, but engenders a hungry habit that is pleasureless but costly in the future. The model has been given an elegant formulation in two papers by Spinnewyn (1979a,b). As an example, see also Muellbauer (1986), take the utility function

$$u = \sum (1+\delta)^{-t} \theta(c - \alpha c_{t-1}), \quad \alpha > 0 \quad (26)$$

where α is a measure of habit formation. Spinnewyn maximizes this function with respect not to c_t , but with respect to the "net" quantities $z_t = c_t -$

αc_{t-1} , and shows how to rewrite the budget constraint so as to define corresponding prices of the z 's that reflect not only market prices of the goods, but also the costs of consumption now in terms of pleasure foregone later. Under certainty, and looking ahead from time t , the full shadow price of an additional unit of consumption now is

$$p_z = \sum_{k=0}^{T-t} (\alpha/(1+r))^k p_{t+k} \quad (27)$$

because the habits that are built up now have to be paid for later. Note that this sort of formulation also predicts that it is $c_t - \alpha c_{t-1}$, not c_t , that is proportional to permanent income, so that consumption itself will adjust only sluggishly to changes in permanent income with habits causing a drag. Other formulations of non-separable preferences can be found in the papers by Kydland and Prescott (1982), and by Eichenbaum, Hansen, and Singleton (1984), both of which are concerned to reconcile fluctuations in the aggregate economy with the behavior of a single representative agent.

Many of the models discussed so far assume that the consumption function actually exists, hence taking for granted the essentially Keynesian assumption that income is given to the consumer, and is not chosen together with consumption. A considerable body of work has grown up in the last ten years that is concerned with the simultaneous choice of labor supply and consumption in a life-cycle setting. Heckman (1971) and Ghez and Becker (1975) are among the pioneers of this approach. Unlike the price of goods, the price of leisure tends to show a systematic pattern over the life-cycle, so that, if consumers are free to choose their hours, and if they can freely borrow and lend so as to transfer resources between periods, it will pay them to work hardest during those periods in their life-cycles when the

rewards for doing so are highest, and to take their life-time leisure when wage rates are low and leisure is cheap. There is superficial evidence in favor of this story, and Ghez and Becker, followed by Smith (1977) and Browning, Deaton, and Irish (1985), all find that workers tend to work longest hours in middle age when wage rates are high and the lowest number of hours at the beginning and end of the economically active life, when wage rates are relatively low. Consumption also tends to peak in middle age, and this can be brought into the story by assuming that consumption and leisure are complements, so that the lack of leisure in middle age is partially compensated by high levels of expenditure. This elegant fable has also been made much of in equilibrium theories of the business cycle, which accounts "unemployment" as a voluntary vacation taken when the real wage is low and leisure is on sale, see in particular Lucas and Rapping (1969) and Lucas (1981).

There now exists a growing volume of literature that shows just how much violence to the facts is done by this story. All the evidence quoted above looks across different individuals at different points in their life-cycles, while the theory says that the same individual will change his or her hours of work along with changes in the real wage over the life-cycle. Time-series and panel data from the U.S. and time-series of cross-sections from the United Kingdom suggest that this is simply not the case, see for example Mankiw, Rothenberg, and Summers (1985), Ashenfelter and Ham (1979), Ashenfelter (1984), and Browning, Deaton, and Irish (1985), and even MaCurdy's (1981) more positive study provides only very weak evidence, see in particular Altonji (1986). The joint consumption and labor supply story fares even less well than the labor supply model alone, and there is clear evi-

dence that the way in which consumption and hours fluctuate over the cycle (sometimes together and sometimes in opposite directions) is not consistent with the way in which they move together over the life-cycle. The attempt to provide a unified theory of business and life-cycles has been an interesting and important one, but it cannot be said to have been successful.

I have been somewhat cavalier in my treatment of aggregation issues, choosing to emphasize them when I believe them to be important, for example in the fitting of Euler conditions, and ignoring them when it has been convenient to do so. Attempts to do better than this have not been notably successful. Formal conditions that allow aggregation in consumption function models are typically too restrictive to be useful, so that, in theory, changes in the distribution of income should have detectable effects on aggregate consumption. However, attempts such as that by Blinder (1975) to link the distribution of income to consumption have not been notably successful, perhaps because the income distribution is not variable, or because it changes smoothly enough over time to preserve a stable relationship between average income and average consumption. There is also an issue of aggregation over goods in order to define real consumption at all, even at the level of the individual agent. In the derivation in section 1 above, I made the convenient assumption that within period preferences were homothetic, so that an index number of real consumption could be formed. But homotheticity, although very convenient for studying the consumption function, is very inconvenient for studying the allocation of expenditure among goods since it implies that the within-period total expenditure elasticities of each good are all equal to unity. Fortunately, there are aggregation results of Gorman's (1959), see also Deaton and Muellbauer (1980b, Chapter

5) for an exposition, that allow us to have the best of both worlds, at least if we remain with intertemporally additive preferences. If the single period indirect utility function $\psi(x,p)$ takes the form known as the "generalized Gorman polar form"

$$\psi(x,p) = F(x/a(p)) + b(p) \quad (28)$$

where $a(p)$ and $b(p)$ are linearly homogeneous functions of prices and $F(\cdot)$ is monotone increasing, then the real expenditure index $x/a(p)$ can serve as an indicator of real consumption just as in the homothetic case. This happens because when the consumer chooses the allocation of life-time expenditure over periods so as to maximize the intertemporal sum of terms like (28), the $b(p)$ terms are irrelevant. However, the intra-period demand functions that correspond to (28) do not display unitary elasticities unless the $b(p)$ is identically equal to zero, and quite general functional forms are permitted. There is therefore no real conflict between the analysis of the consumption function on the one hand, and the analysis of demand on the other. It is to the latter that I now turn.

4. Theoretical and empirical demand functions

Demand functions are the relationships between the purchase of individual goods, income or total expenditure, prices, and a variety of other factors depending on the context. Economists have attempted to make empirical links between demand and price since Gregory King's famous demand curve for wheat, see Davenant (1699), and since the middle of the nineteenth century, there has been a great development in the theory of consumer behavior. Much practical work continues in the tradition of King, paying little attention to

formal theory, concerning itself instead with finding empirical regularities. For a firm studying the demand for its product, or for anyone interested in establishing a single price elasticity, this probably remains the best approach; the major developments in econometric technique and empirical formulation have not been much concerned with, or relevant to, these very practical questions. The pragmatic approach (the term comes from Goldberger's famous but unpublished (1967) study), probably reached its peak with the publication of Richard Stone's great monograph, Stone (1954a), and much is still to be learned by a careful study of Stone's procedures for measuring income and price elasticities. However, in this essay, I shall follow the literature, and follow its more methodological approach.

The theory outlined in Section 1 above suggests that the demand functions of an individual consumer can be derived by maximizing a utility function $u(q)$ subject to a budget constraint $p \cdot q = x$, where x is total expenditure. In the analysis here, x is chosen at some previous level of decision making, but traditionally it is treated as if it were a datum by the consumer. The utility maximization yields a vector q that is some function $g(x, p)$, say, of total expenditure and prices. These demand functions cannot simply be any functions, but must have certain properties as a result of their origins in utility maximization. Obviously, the total value of the demands should be equal to total outlay x , the "adding-up" property, and it must be true that proportional changes in x and in p do not have any effect on quantities demanded, the "homogeneity" or "absence of money illusion" property. Somewhat less obvious are the famous symmetry and negativity properties. These apply to the Slutsky (1915) matrix, S , the typical element of which is defined as

$$s_{ij} = \partial q_i / \partial p_j + q_j \partial q_i / \partial x. \quad (29)$$

As any intermediate text shows, see for example Deaton and Muellbauer (1980b, Chapter 2), the Slutsky matrix must be symmetric and negative semi-definite. The symmetry property is not readily turned into simple intuition; negativity implies that the diagonal elements of the matrix are non-positive, a proposition often referred to as "the law of demand." The four properties, adding-up, homogeneity, symmetry, and negativity, essentially exhaust the implications of utility maximization, so that any empirical demand functions that satisfy them can be regarded as having been generated by utility maximization, or by rational choice, with "rational" defined, following Gorman (1981), as "having smooth strictly quasi-concave preferences, and being greedy."

Stone (1954b) was the first to attempt to use this theory directly to confront the data. He started from a (general) linear expenditure system of the form

$$p_i q_i = \sum_j a_{ij} p_j + b_i x \quad (30)$$

where a_{ij} and b_i are unknown parameters. Stone showed that, in general, the system (30) does not satisfy the four requirements, but will do so if, and only if, the parameters are restricted so that the model can be written in the form

$$p_i q_i = p_i \gamma_i + \beta_i (x - p \cdot \gamma) \quad (31)$$

with the β -parameters summing to unity. In this form the model is known as the linear expenditure system. As Samuelson (1947-8) and Geary (1949-50) had earlier shown, the utility function corresponding to (31) has the form

$$u = \sum \beta_i \ln(q_i - \gamma_i). \quad (32)$$

sometimes referred to (somewhat inappropriately) as the Stone-Geary utility function. It can be thought of as a sum of Bernoulli utility functions of the quantity of each good above the minimal γ 's.

Stone's achievement lay not in deriving the demand functions, but in thinking to estimate them. The demand functions (30), even if fitted to the data by least-squares, require non-linear optimization, and Stone invented a simple and not very efficient scheme, but one that allowed him to obtain parameter estimates and a good fit to inter-war British data for a six commodity disaggregation of expenditures. This was a major breakthrough, not only in demand analysis, but also in applied econometrics in general. Indeed, much of demand analysis for a decade or so after Stone's paper consisted of applying better algorithms and faster computers to the fitting of Stone's model to different data sets.

The linear expenditure system offers a demand model for a system of, say, n goods, and requires only $2n-1$ parameters, a degree of parsimony that was very important in allowing the model to be estimated on very short time series data. However, such economy brings its own price, and the linear expenditure system is very restrictive in the sort of behavior that it can allow. In particular, and pathological cases apart, the model cannot allow inferior goods (goods the demand for which falls as total outlay increases), nor can it allow goods to be complements rather than substitutes. (As defined by Hicks (1936) goods i and j are complements if the (i,j) th term in the Slutsky matrix is negative, so that the utility compensated cross-price response of i to an increase in the price of j is positive.) Normal (non-

inferior) goods that are substitutes for one another may be the most important case, but they do not encompass everything that we might want to study. The linear expenditure system also implies that the marginal propensity to consume each good is the same no matter what is the total to be spent, and many cross-section studies of household budgets have suggested that this is not in fact the case.

Unfortunately, it is quite difficult to write down utility functions that will lead to more general demand functions than those of the linear expenditure system, nor is there any obvious way of generalizing Stone's procedure of writing down functions and making them consistent with the theory. Progress was only really made once applied demand analysis started using "dual" formulations of preferences to specify demands. In the demand context, duality refers to a switch of variables, from quantities to prices, so that utility becomes a function, not directly of quantities consumed, but indirectly of prices and total expenditure. This indirect utility formulation is given by the function $\psi(x,p)$, already used above, and this is simply the maximum attainable utility from total outlay x at prices p . Since $\psi(x,p)=u$, and the function is monotone increasing in x , it can be inverted to give $x=c(u,p)$, known as the "cost function," since it gives the minimum necessary cost that is required to reach the utility level u . By a theorem usually attributed to Shephard (1953) and to Uzawa (1964), these two functions contain a complete representation of preferences; provided preferences are convex, and provided the functions satisfy homogeneity and convexity (or concavity) conditions, preferences can be reconstructed from knowledge of either of the two functions. It is also very easy to move from either cost

or indirect utility functions to the demand functions. For the indirect utility function, we have Roy's identity, Roy (1943),

$$q = - \nabla_p \psi(x, p) / \psi_x(x, p) = g(x, p) \quad (33)$$

which immediately yields demand functions from preferences in a form that are suitable for estimation, while for the cost function, we have Shephard's Lemma (1953),

$$q = \nabla_p c(u, p) = \nabla_p c(\psi(x, p), p) = g(x, p) \quad (34)$$

where, as in (33), the operator ∇ denotes a vector of partial derivatives.

Demand analysis now had a high road to specification. Think of some quasi-convex decreasing function of the ratios of price to total outlay and call it an indirect utility function, or think of some function of utility and prices that is increasing in its arguments and linearly homogeneous and concave in prices and call it a cost function. Either way, and with only simple differentiation, new (and sometimes) interesting demand functions will be generated. Alternatively, and even more importantly, it is possible to use theory to aid and check out empirical knowledge. If it is known that the marginal propensity to spend on food is a declining function of total expenditure, or if it is thought likely that some goods do not depend very directly on the prices of other goods, it is relatively straightforward to find out what preferences (if any) will yield the result. It becomes possible, not just to generate demand functions serendipitously, but to generate good and useful ones deliberately.

There are many examples that could be cited from the literature. One of the most widely used is the translog model which was first proposed in 1970

by Jorgenson and Lau, see Christensen, Jorgenson, and Lau (1973) for a convenient reference. To derive the translog, write the indirect utility function in terms of the ratios of prices to outlay, $r=p/x$, and approximate the indirect utility function as a second order polynomial in the logarithms of r . Application of Roy's identity yields demand functions in which the budget share of each good is the ratio of two functions, each of which is linear in the logarithms of the price to outlay ratios. Estimation of these rational functions, like estimation of the linear expenditure system, requires the use of non-linear maximization techniques. A related model, the "almost ideal demand system" has been proposed by Deaton and Muellbauer (1980a), and I use this to illustrate some of the issues that arise with the current generation of demand models. The AIDS is specified by the logarithm of its cost function which takes the form

$$\ln c(u, p) = \alpha_0 + \sum \alpha_k \ln p_k + 0.5 \sum \sum \gamma_{km} \ln p_k \ln p_m + u \exp(\sum \beta_k \ln p_k) \quad (35)$$

so that, applying Shephard's lemma and rearranging, we have demand functions

$$p_i q_i / x = w_i = \alpha_i + \beta_i \ln(x/P) + \sum \gamma_{ij} \ln p_j \quad (36)$$

where P is a linearly homogeneous price index, the form of which can readily be inferred from (35). The parameters of the model must satisfy certain restrictions if (35) is to be a proper (log) cost function, and (36) a proper system of demand functions. The matrix of γ -parameters can be taken to be symmetric in (35), but must be so in (36), and its rows and columns must add to zero for the homogeneity and adding-up properties to be satisfied. The β -parameters can be positive or negative, with positive values indicating luxury goods, and negative values necessities. The main advant-

age of the AIDS model in time-series applications is that the price index P can typically be approximated by some known price index selected before estimation, so that the demand system is linear in its parameters. In consequence, it can be estimated by ordinary least squares on an equation by equation basis, at least if the symmetry of the γ -matrix is ignored. The homogeneity restrictions can be tested equation by equation using a t - or F -test, and while imposing or testing symmetry requires an iterative procedure, estimation can be done by straightforward iterated restricted generalized least-squares, see Barten (1969) or Deaton (1974a) for further discussion.

The results of estimating the AIDS model are sufficiently similar to those from other models and other studies, see e.g. Barten (1969), Deaton (1974a), Christensen, Jorgenson, and Lau (1973), and many others, that perhaps they can be taken as representative. What typically seems to happen is that the homogeneity restrictions appear *not* to be satisfied, so that in the application of AIDS to British data, Deaton and Muellbauer found, for example, that the F -test for transport had a value of 172 compared with the 5% critical value of 4.8. Results on symmetry from AIDS and other systems are more mixed, and it now seems clear that testing symmetry is not usually possible given the amount of data typically available in time series, or put more positively, that there is no convincing evidence against symmetry. The difficulty is that symmetry involves a set of restrictions *across* different equations, so that unlike homogeneity, which involves tests *within* each equation, exact, small sample tests are not available. Researchers have therefore fallen back on asymptotically valid tests, and it turns out that these work very badly for the usual sort of samples, especially when there

are more than a very small number of goods in the demand system. The papers by Laitinen (1978) and Meisner (1979) first established the problem, see also Evans and Savin (1982) and Bera, Byron, and Jarque (1981) for further evidence.

The AIDS model, like the translog and several others, e.g. Diewert's (1973) "generalized Leontief" system, fall into the class of "flexible functional forms." This criterion of flexibility, first proposed by Diewert (1971), is an important guarantee that the model is sufficiently richly parametrized so as to allow estimation of what are thought to be the main parameters of interest, typically the total expenditure elasticities, and the matrix of own and cross-price elasticities. A "second order" flexible functional form is one that has sufficient parameters, so configured, that it is possible to set the value of the function, and of its first and second partial derivatives to any arbitrary set of (theoretically permissible) values. By applying Roy's identity or Shephard's lemma, it is clear that a cost or indirect utility function that is a second order flexible functional form will yield demand functions that are first-order flexible, so that is possible for estimation to yield any set of price and expenditure elasticities that are consistent with utility theory. For empirical work, such a guarantee is important, because it ensures that the elasticities are being *measured*, not assumed. Contrast, for example, the linear expenditure system (31) with the AIDS model (36). Both could be fitted to the same set of data, and the parameter estimates of each could be used to generate a complete set of expenditure and price elasticities. But the linear expenditure system is not a flexible functional form, and so its estimated elasticities are not independent of one another, as is apparent from the fact that there

are $2n-1$ parameters compared with the total number of potentially independent elasticities, which is $(n-1)(1+n/2)$. (There are $n-1$ independent demand equations, each of which has an expenditure elasticity, and n price elasticities; however, one price elasticity per equation is lost to homogeneity, and symmetry imposes a further $(n-1)(n-2)/2$ constraints.) The linear expenditure system does not therefore *measure* all the price and income elasticities, but determines them by a mixture of measurement and assumption, the main assumption being that of additive preferences, see Deaton (1974b) for further details. The AIDS, by contrast, has exactly the right number of parameters to allow for intercepts and a full set of elasticities, so that when it (or the translog, or the generalized Leontief) is estimated, so is the full set of elasticities.

Being able to do this is a great step forward in methodology, but just as the linear expenditure system probably asks too little of modern data, (although not of the data available to Stone and the early pioneers of the systems approach), the second-order flexible functional forms probably ask too much, or equivalently, put too little structure on the problem. The consequences show up in large standard errors, a high frequency of apparently chance correlations, and a lack of robustness to functional form changes within the class of flexible functional forms, in other words, in all the standards symptoms of over-parametrization. These problems are particularly acute for the measurement of price elasticities, because in most time series data, commodity prices tend to move together with relatively little variation in relative prices. And although the focus of most research on demand analysis over the last thirty years has been on the estimation and testing of price responses, there is certainly no consensus on what numbers,

if any, are correct. Estimates obtained from the linear expenditure system are not credible because they are forced to satisfy an implausibly restrictive structure, while those from flexible functional forms are not credible because the data are not informative enough to supplement the lack of prior structure. Some intermediate forms are clearly required.

One of the attractions of flexible functional forms is their ability to approximate quite general forms for preferences. However, the models so far considered offer only local approximations, and there is no guarantee that they have satisfactory *global* properties. Partly this is the standard problem that a fitted model will be forced to give a reasonable account of the data over the sample used for estimation, but may predict very badly elsewhere. But there are other deeper issues. Taking the AIDS as an example, estimation of (36) subject to symmetry and homogeneity will produce a system of estimated demand functions that will satisfy adding-up, homogeneity and symmetry for *all* values of x and p . However, there are two other important properties that are not assured. First, there is no guarantee that the predicted budget shares will necessarily lie between zero and one, so that there may be regions of price space in which the estimated model yields nonsensical predictions. Second, there is no way that the AIDS can be guaranteed to have a negative semi-definite Slutsky matrix for all prices, at least not without restricting parameters to the point where the model ceases to be a flexible functional form. The parameters could be chosen so as to satisfy negativity for some particular combination of prices and outlay, but there will be no guarantee that the law of demand will be satisfied elsewhere. In the translog model, it is possible to impose a restriction that guarantees negativity everywhere, but the model with the

restriction has the property that all estimated own price elasticities must be less than minus one, independently of whether this is in fact true, and it almost certainly is not, see Diewert and Wales (1986). A demand system is described as "regular" if it has a negative definite Slutsky matrix and predicts positive demands, and several empirical studies, see e.g. Wales (1977) for one of the first, found that estimated flexible functional forms were not regular over disturbingly large regions of even the parameter space used to estimate them. Caves and Christensen (1980), and later Barnett and Lee (1985) and Barnett, Lee, and Wolfe (1985), investigated the same problem theoretically by taking a known utility function, choosing the parameters of flexible functional forms to match its level and derivatives at a point, and then mapping out the regions of price space in which the systems remained regular. The results, at least for the translog and the generalized Leontief model, were not good.

These regularity issues may seem of limited importance in practice, but this is far from being the case. One of the major reasons for being interested in complete empirical demand systems is to be able to examine the consequences of price changes, particularly of price changes that follow changes in government policy. The United States relies relatively little on indirect taxation as a source of public finance, but such is not the case in most of Europe, and the vast majority of developing countries maintain complex systems of price wedges, particularly for foods and for agricultural production. The effects of such systems cannot be predicted without good information on how demands respond to price changes, nor can reforms be intelligently discussed. However, estimated demand systems that are not regular are not a great deal of help. All of the theory of welfare econ-

omics, of consumer surplus, of optimal taxation and of tax reform, *assumes* that demand behavior is generated by utility maximization at the individual level, and implementation without regularity risks internal contradiction. For example, if compensated demand functions slope upwards, the government can generate a dead-weight gain by imposing a distortionary tax. Of course, it may not be the empirical work that is wrong, but the theory that we use to try to model behavior. If so, the estimated demand functions are still not useful, since we now have no idea what to do with them. But I doubt that the evidence goes so far; it is not that behavior itself is irregular, but that we have not yet found a good way modelling strategy that contains a reasonable amount of prior information to supplement the paucity of data, and at the same time can deliver global regularity if it is warranted by the evidence.

A number of interesting experiments are currently under way that involve new modelling techniques. One possibility is that the Taylor series expansions that motivate most flexible functional forms are themselves inadequate to the task. In particular, Taylor approximations lose their ability to approximate if they are also asked to possess other properties of the functions that they are approximating. For example, we might want to test whether or not preferences are additively separable, as in the linear expenditure system. One strategy would be to write down some second-order approximation to preferences, estimate the resulting demand model, and then test whether or not the conditions imposed on the demands by additivity are satisfied. But this will not work in general, because there may be no additive system of demand equations that has the precise functional form demanded by the approximation. The same phenomenon is well illustrated by

Stone's derivation of the linear expenditure system itself. The original general linear expenditure equations (29) can clearly be justified as a Taylor approximation to any set of homogeneous demand functions, and yet the imposition of only *symmetry* generates the demand system (30) which comes from the *additive* utility function (31). Additivity is not imposed, but linear expenditure systems are only symmetric if they are additive. Similarly many flexible functional forms are only globally regular if they are homothetic, see for example, Blackorby, Primont, and Russell (1977). Several recent studies have proposed alternative ways of making functional approximations. Gallant (1982) has proposed using Fourier series approximations while Barnett (1983) has suggested that Laurent series can be used to generate demand models with good properties. Gallant's models are even more heavily parametrized than standard flexible functional forms, and there must be some question as to the suitability of trigonometrical functions for demand functions. Barnett's "miniflex Laurent" model does not use the full flexibility of the Laurent series, but appears to have quite good approximation and regularity properties in practice, see Barnett and Lee (1985) and Barnett, Lee, and Wolfe (1985); even so, its estimation is complex, and many of the parameters have to be estimated subject to inequality constraints.

A second line of current research has abandoned the standard approach of econometric analysis, taking instead a completely non-parametric approach. Since many of the difficulties discussed above arise from choice of functional form, it is useful to ask how far it is possible to go without assuming any functional form at all. We know from standard revealed preference theory that two observed vectors of prices and quantities can be

inconsistent with utility maximization; if bundle one is chosen when bundle two is available, so that bundle one is revealed preferred to bundle two, then no subsequent choice should reveal bundle two to be preferred to bundle one. Before embarking on the exercise of fitting some specific utility function to any finite collection of price and quantity pairs, one might then ask whether the collection is conceivably consistent with any set of preferences. If it is, then contradictions between an estimated system and the theory must be a matter of inappropriate functional form. The conditions for utility consistency of a finite set of data were originally derived by Afriat (1967), who proposed a condition called cyclical consistency. Much later Varian (1982) not only provided an accessible and clear account of Afriat's results, but also recast the cyclical consistency condition into a "generalized axiom of revealed preference (GARP)" that runs as follows. A bundle q^i is strictly directly revealed preferred to a bundle q if $p^i q^i > p^i q$, while q^i is revealed preferred to q , if there exists a sequence j, k, \dots, m such that $p^i q^i \geq p^i q^j$, $p^j q^j \geq p^j q^k, \dots, p^m q^m \geq p^m q$, so that q^i is directly or indirectly (weakly) revealed preferred to q . GARP is satisfied if for all q^i revealed preferred to q^j , it is not true that q^j is strictly directly revealed preferred to q^i , and given GARP the data can be rationalized by a continuous, strictly-concave, and non-satiated utility function. Differentiability can also be ensured by a slight strengthening of GARP, see Chiappori and Rochet (1987). GARP is readily tested for any given set of data by checking the pairwise inequalities and using a simple algorithm provided by Varian to map out the patterns of indirect revealed preference. Repeated applications of the method to time series data have nearly always confirmed the consistency of the data with the theory. In

retrospect, it is clear that violations of GARP cannot occur unless some budget lines intersect, so that if, over time, economic growth has resulted in the aggregate budget line moving steadily outward with little change in slopes, GARP is bound to be satisfied. (However, post-war U.S. data budget planes do occasionally intersect, and Bronars (1987) has recently shown that hypothetical demands generated by selecting random points on the actual budget lines would more often than not fail GARP.)

The contradictions between the parametric and non-parametric approaches can perhaps be resolved by thinking of the latter as a modelling technique that uses a very large number of parameters, so that the failure of the parametric models to fit theory to data can be thought of as failure to parametrize the models sufficiently richly. But I have already argued that these models already have too many parameters, and adding more would only exacerbate the already serious problems of measurement. For many purposes, the theory is only useful if it is capable of delivering a description of the data that is reasonably parsimonious. There is also something rather simple minded about non-parametric techniques that tends to be disguised by the sophisticated and elegant expositions that they have been given them by Varian and others. Consider a very simple theory that says variable x should move directly with variable y as, for example, in the Euler equation (15) above which says that, under certainty, consumption should grow from period t to $t+1$ if and only if the real interest rate from t to $t+1$ is greater than some fixed constant. A non-parametric test on a finite set of data would accept the theory if, in fact, x and y always did move together, and reject it if x and y ever moved in opposite directions. That such

testing procedures are widely employed in the press and by the uninformed public is no reason for treating them seriously in economics.

I have so far discussed the formulation and estimation of demand functions, meaning the relationships between quantities, outlay, and prices, and this has been the topic of most applied demand analysis over the last thirty years. However, there is an older tradition of demand analysis, in which the object of attention is household budget data, and this literature has recently been enjoying something of a revival. Since household budget data typically come from a cross-section of households over a short period of time, usually within a single year, prices are treated as common to all sample points, so that the focus of attention becomes the relationship between demand and outlay and the influence of household composition on the pattern of household expenditures. The oldest, and perhaps only law of economics, Engel's Law, that the share of food in the budget declines as total outlay increases, comes from Engel's (1857, published 1895) study of Belgian working class families, and early empirical studies of demand were almost inevitably based on household surveys, see Stigler (1954) for a masterly review. The modern study of Engel curves, the relationships between expenditure and total outlay, begins (and almost ended) with Prais and Houthakker (1955). Prais and Houthakker studied the shapes of Engel curves, the relationship between demand and household composition, and the variation in unit values across households, particularly in relation to the choice of quality, a topic that has subsequently been unjustly neglected. The functional forms for Engel curves that Prais and Houthakker examined became the staple menu for most subsequent studies, even though only one of their forms, the linear Engel curve, is capable of satisfying adding-up, and

the linear form typically performs very badly on the data. Since 1955 a number of other Engel curves have been proposed, notably the lognormal Engel curve of Aitchison and Brown (1957), and Leser's (1963) revival of the form suggested much earlier by Holbrook Working (1943). Working's form, which apparently escaped the attention of Prais and Houthakker, makes the budget share of each commodity a linear function of the logarithm of total outlay. This formulation is particularly useful, for not only is it capable of accounting for most of the curvature that is discovered in empirical Engel curves, but it is also consistent with utility theory, and corresponds to the case where the welfare elasticity of the cost of living is independent of income. Gorman (1981) has provided a general characterization theorem for Engel curves of the form

$$p_i q_i = \sum_k a_{ik}(p) \xi_k(x) \quad (37)$$

and has shown that the $\xi_k(\cdot)$ functions can be powers of x (polynomial Engel curves), or x multiplied by powers of $\log x$ (Engel curves relating budget shares to powers of the logarithm of outlay), or have trigonometric forms. This last form includes Fourier representations of Engel curves, while the first two allow Taylor or Laurent expansions for the expenditure/outlay and for the share/log-outlay forms. The Working Engel curve is the first member of Gorman's "share to log" class, and the theorem tells us that we may add quadratic or higher order terms to improve the fit. However, Gorman's paper contains a remarkable result; the matrix of the a -coefficients in (37) has rank at most equal to three. In consequence, the share to log and log-squared Engel curves are as general as any, as are the Engel curves of the quadratic expenditure system, see Howe, Pollak, and Wales (1979). Given

Gorman's results, and the empirical success of the Working form, it and its quadratic generalization deserve wide use in the analysis of budget studies. There is also accumulating evidence that such forms are indeed necessary. Thomas (1986), in a wide-ranging examination of household survey data from developing countries, has shown that Engel's Law itself does not appear to hold among the very poor, so that, in many cases, the share of the budget devoted to food at first rises with total outlay before falling in conformity with the Law.

Prais and Houthakker also proposed a much-used formulation for the effects of household composition on behavior. It can be written

$$p_i q_i / m_i(a) = f_i(x/m_0(a)) \quad (38)$$

where a is a vector of household demographic characteristics (perhaps a list of numbers of people in each age and sex category) and m_i and m_0 are scalar valued functions known as the "specific" and "general scales" respectively. In this literature, scales are devices that convert family structure into numbers of equivalent adults, so that a family of two adults and two children might be two equivalent adults for theater entertainment, three equivalent adults for food, and six equivalent adults for milk. The general scale is supposed to reflect the overall number of equivalent adults, so that the Prais and Houthakker model is a simple generalization of the idea that per capita demand should be a function of per capita outlay. Barten (1964), in a very important paper, took up the Prais-Houthakker idea of specific scales, but assumed that the arguments of the household utility function were the household consumption levels each deflated by the corresponding specific scale. The consequences of Barten's formulation are sim-

ilar to those of Prais and Houthakker, but embody the additional insight that changes in family composition affect the effective shadow prices of goods, so that demographic changes will exercise, not only income, but also substitution effects on the pattern of demand. The story is often summarized by the phrase, "if you have a wife and child, a penny bun costs three-pence," quoted in Gorman (1976), but the really far-reaching substitution effects of children are probably on time use and labor supply, particularly of women.

Since household surveys typically contain large samples of households, there is less need for theory to save degrees of freedom, and it is possible to estimate quite general functional forms that link expenditures to household composition patterns and then to interpret the results in terms of the various models. In addition, neither the Prais-Houthakker nor the Barten model seem to yield easily implemented functional forms, e.g. linear ones, nor it is clear that either model is even identified on a single cross-sectional household survey in which all prices are constant, see for example Muellbauer (1980) and Deaton (1986a). However, some empirical results for the two models can be found in Muellbauer (1977,1980) and in Pollak and Wales (1980, 1981) who also examine Gorman's (1976) extension of Barten's model in which additional people are supposed to bring with them fixed needs for particular commodities. The fixed needs model is close to the formulation proposed by Rothbarth (1943) for measuring the costs of children. Rothbarth pointed out that there are certain commodities, adult goods, that are not consumed by children, so that when children are added to a household, the only effects on the household's consumption of adult goods will be the income effects that reflect the fact that, with unchanged total resour-

ces, the household is now poorer. Deaton, Ruiz-Castillo, and Thomas (1985) have recently attempted to test Rothbarth's contention, and in their Spanish data it seems possible to identify a sensible group of adult goods, the expenditure on each of which changes with additional children in the same way as they change in response to changes in outlay.

Studies of the effects of family composition on household expenditure patterns have frequently been concerned, not only with estimating demands, but also with attempts to measure the "cost" of children. It would take me too far afield to do justice to this topic here. Readers interested in this controversial area should perhaps start with Rothbarth (1943), who in a few pages makes a very simple and quite convincing case, and look also at Nicholson (1975). Pollak and Wales (1979) weigh in on the opposite side, and claim that it is impossible to measure child costs from expenditure data. My own position is argued in Deaton and Muellbauer (1986); there are certainly grave problems to be overcome in moving from the analysis of household survey data to the measurement of the costs of children, and it is clear that identifying assumptions must be made that are more severe and more controversial than those required, for example, to go from demand functions to consumer surplus. But that does not mean that it is not possible for such assumptions to be proposed and to be sensibly discussed.

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