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Children and Demand: Direct and Non-Direct Effects*

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Abstract. This paper examines the effects of children on demands. We employ a French family expenditure survey that has a number of unusual features to explore the source of the correlation between children and demands. The first sample we use is of older households which has information on completed fertility. The second sample is of younger households for whom we have family background variables. We find that children are not exogenous for some goods. These findings cast doubt on the usual practice of identifying "direct" children effects with the coefficients on the children variables in demand equations.

Keywords: children, demand, fixed effects, state dependence, intra-household effects

JEL Classification: D12, J13

Children affect almost all facets of household economic behavior in the sense that children variables are usually "significant" whenever they are included on the right-hand side of any regression (see Martin Browning, 1992). For example, one of the most robust facts in empirical economics is that food budget shares are higher for households that have children than for those that do not. Almost universally it is inferred from this that children are "food intensive"; that is, children's preferences[1] give a higher weight to food than do adult preferences. While this is plausible, it need not be the only effect at work in this empirical fact. A child represents an additional household member, and apart from the intensity aspect, its arrival may cause a

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reallocation of the budget because expenditure per capita decreases, because of the existence of economies of scale in the consumption of certain goods, or because of substitutions on the parts of the adults. All of these causes can be thought of as "direct" effects since they operate only when children are present. These are not, however, the only possible sources of a correlation between children and economic behavior. It is quite plausible that there are "non-direct" effects induced by heterogeneity, by state dependence or by intra-household effects.

In section 2 we present an extended example with heterogeneity in the preferences for goods (but not necessarily in the "taste" for children). We show that simply allowing that children are a choice variable (and that standard life-cycle allocation conditions hold) leads to pervasive bias in *all* parameters in a demand equation. That correlated heterogeneity leads to endogeneity bias is widely accepted; the point of our theoretical analysis below is that there is the same problem even if heterogeneity in tastes over goods is uncorrelated with heterogeneity in the propensity to have children (or the latter is absent). The second indirect effect, due to state dependence, would arise if the presence of young children changes parent's behavior and these changes have long run reverberations. For example, the data suggests strongly that people reduce restaurant expenditures when they first have children. Even when children have grown old enough to look after themselves, it may be that the "habit" of eating out is much attenuated and parents do not revert to their pre-children behavior (see Reuben Gronau, 1988). Finally, suppose that the birth of a child to a couple changes the division of any assets and rights to future income flows in the event of separation. This may lead to shifts in the current balance of power within the household and to a consequent change in expenditure patterns over and above those due to the direct and non-direct effects already mentioned. If there are such effects then we can no longer identify direct effects from simply including children variables on the right-hand side of the equation of interest.

Whatever the motivation for undertaking an analysis of demand, account should be taken of the fact that households differ in composition. There are at least three reasons why we should condition on household composition. First, we may be interested in the effects of household composition on demands. Second, even if we are not directly interested in the effects of children they are probably correlated with other right-hand side variables (e.g., total expenditure in a demand equation), so that leaving them out would lead to bias in the estimated coefficients of these other variables. Finally, children usually improve the fit considerably and consequently reduce the standard errors on other variables, which might be regarded as a sufficient reason to include them even if they are uncorrelated with other explanatory variables.

Often we are interested in measuring the effect of children on behavior. To be sure, there are those who reject this and who aver that we should treat children just as we do any other "good" and not include them as right-hand side variables (Browning (1992) characterizes this as the "purist" view). For them, since children might adjust to a policy change, the appropriate thing to do would be to estimate reduced forms. For most investigators, however, there is interest in conditional responses.[2] Indeed,

for women who have completed their fertility, conditional responses are all there is; for those who have started but not finished, they do provide partial information, and finally they give bounds for those who have not yet started. Given that we may have an interest in conditional responses, the problems of estimating them consistently and interpreting them arise. If there are only direct effects then there is only the latter problem. If, however, there are non-direct effects then both problems are potentially present. As an example, consider the construction of adult equivalence (ae) scales. If there is state dependence then the comparison group (i.e., the "no-children" group) is ambiguous since the demand behavior of those who have had children but whose children have left home will be different from those who will never have children. Similarly, the validity of adult equivalence scale models usually depend on there being only certain direct effects, to the exclusion of any other effect. We shall return to these issues below.

In this paper, we explore the nature of the correlation between children and demands to throw some light on these issues. We shall not be discussing whether children are the result of a choice, but rather whether conditioning demand on children with no instrumenting introduces a bias in the estimated demand parameters. As far as we are aware, neither the presence of non-direct effects nor exogeneity of children in demand equations have ever been tested for, presumably because of data availability constraints.[3] Cross-section data, which could allow for IV method tests, usually lack good instruments for children variables. Long panels with detailed information on consumption, which could control for fixed effects or state dependence, simply do not exist. Quasi-panels (time series of cross sections), when they can be constructed, could possibly be used to test for exogeneity of children by cohort means estimation. The ideal (non-experimental) data for addressing these questions would include information on planned and completed fertility of a panel of households, as well as on detailed expenditures.

Below we present tests using a far from ideal, but rather unusual data base: the French family expenditure survey "Enquête sur les Budgets des Familles 1984–85" (EBF). Two unusual features of this survey allow us to conduct two distinct sets of tests. First, households in which the head is over 55 years old are over-sampled and are asked a series of questions concerning their children who are not currently living at home. Since the children are not living at home the direct effects of children are absent[4] so that any correlation between demands and having had children can be taken as evidence of non-direct effects. Second, for all respondents, background questions concerning characteristics of the respondent's parents are asked. We use these to instrument for children and conduct conventional exogeneity tests. Of course, the validity of these tests rests on the exclusion restrictions we make and these are open to question (although we do test for the over-identifying restrictions).

In section 2, we present a more detailed discussion of the various direct and non-direct effects that can induce a correlation between children and demand. In section 3, we present the data. In section 4, we present tests for non-direct effects using the over-55's sample. We find evidence of the presence of such effects. In section 5, we run a conventional exogeneity test on a sample of households with

children present. These tests turn out to be rather inconclusive but there is weak evidence that exogeneity is rejected. Section 6 analyzes substantive effects and Section 7 concludes, mainly on the need for data on fertility and for further testing.

1. The correlation between children and demand

In this section, we discuss the various effects that can lead to a partial correlation between children and demand in cross-section data. Although there are many discussions of these effects scattered throughout the literature (see, e.g., Arthur Lewbel, 1985; Richard Dickens, Vanessa Fry, and Panos Pashardes, 1993; Browning, 1992; Angus Deaton and Christina Paxson, 1998) we have not been able to find any comprehensive and complete discussion. The analysis of household consumption behavior relates expenditures on non-durables at date t to a set of contemporaneous explanatory variables X_{ht} such as total expenditure on non-durables, region of residence, age and socio-professional category of the household members, and to a "children" variable, Z_{ht}, which we take to be a scalar, for convenience. To fix ideas, we can write a linear form for the budget share for a non-durable good w_{ht}, for household h at date t:[5]

$$w_{ht} = X'_{ht}\beta + \rho Z_{ht} + e_{ht}. \tag{1.1}$$

The question that we are interested in is: what are the structural mechanisms that lead to the presence of children on the right-hand side? We choose to distinguish between what we call *direct* and *non-direct* effects; the distinction being that direct effects are contemporaneous and hence are absent before the children are born and after they have left home.

1.1. *Direct effects*

Direct effects refer to effects that occur only when children are present in the household. There are four distinct direct effects: Engel effects, intensity effects, economies of scale effects and substitution effects. The effect resulting from all four will be termed the total direct effect, or more simply the direct effect. Depending on the purpose for the estimation, one is usually interested in measuring either the total direct effect, or a partial direct effect.

1.1.1. Engel effect. The first direct effect we discuss is suggested by the two empirical regularities associated with the name Engel, both based on comparing the food budget shares of different types of households. The first of these regularities is seen when comparing the food budget shares of a richer and a poorer household for a given household size, and the second when comparing the food budget share of a household with no children with that of a household with children, for a given level of total expenditure. In both cases, the food budget share of the second type of

household is higher. This suggests that for some goods more people is equivalent to less per capita income, leading to an increase in the budget share of that good if it is a necessity (and conversely for luxuries). It is this that the Engel effect captures. A pure Engel effect is obtained when children act only as a deflator for total expenditure.

1.1.2. Intensity effect. A modification of the budget structure following a variation in household composition can also occur because of different preferences of additional members. A child, for example, is usually supposed to have preferences that differ from that of adults. Thus the addition of a new household member, for a given level of total expenditure, can be expected to lead to increases in budget shares of all goods for which this new member is relatively more "intensive" than former household members. If children's preferences were similar to adult preferences, this effect would not be present as the additional household member would have a budget structure identical to that of existing household members. A pure intensity effect obtains with constant total expenditure per head. An obvious example of intensity effect is the demand for children's clothing which is zero if there are no children in the household and positive if there are. We could never model intensity effects as Engel effects since no matter how we change income we could never induce a household without children to begin buying children's clothing.

1.1.3. Public goods and economies of scale effect. The existence of economies of scale in the consumption of public goods is also a source of variation in the budget structure with the number of household members (see Deaton and Paxson, 1998). There are two off-setting influences here. First, more members makes a public good "cheaper" for each member so that demand might increase. On the other hand, the nature of a public good is that consumption by one person does not exclude consumption by another one, so that demand for a public good might stay constant as the number of household members increases. More surprisingly, it can also be shown in a simple setting that the demand for public goods can decrease as the number of household members increases. The reason for this is not due to a property of the public goods, but rather to the fact that as there are more people sharing private goods, household demand for private goods may increase, even if demand per capita decreases. The net effect of these influences on the budget share of public goods can be either positive or negative.[6]

1.1.4. Substitution effects. A fourth source of modification in the budget structure linked with the presence of children is substitution effects. It can be for instance that, because of the presence of the children, the adults eat more at home and less in restaurants. In this case, the increase in the budget share of food at home results from an increase in the adults' consumption in response to the change in relative price of various activities, as eating out becomes more expensive since it must now include the cost of baby-sitting. There are several ways to represent such effects. If children are considered to be a good, then their implicit price is their marginal utility, which decreases as their number increases. This causes substitutions away from other

goods which have become relatively more expensive. Alternatively, one can consider that children affect the effective relative prices of goods, as in the Barten model, typically making food at home (for the parents) cheaper and food in restaurants more expensive because the latter includes the cost of the baby-sitter. Finally, the restaurant/home example can be thought of in the context of household production, where the change in demand can be viewed as a change in the inputs to household production. It is not clear whether one wants to consider costs of changes in budget structure due to substitution effects to be part of the cost of children.

To finish this sub-section, we note that the fact that there are multiple direct effects can lead to problems of interpretation. Consider the use of demand estimates to evaluate the effects of tax changes on the welfare of individual households. For this purpose, only the intensity effect is important. A number of countries have differential tax rates on goods (often, e.g., there is no tax on children's clothing). This implicitly assumes that the only direct effect is the intensity effect. If this is not the case then this measure might under- or over-compensate households with children. Another use of demand estimates is the construction of equivalence scales. One of the most used methods to do this is the iso-prop (or Engel) method but this is only valid if there are only Engel direct effects for the good used to make comparisons (see Alexander Henderson, 1948; Angus Deaton and John Muellbauer, 1986).

1.2. Non-direct effects

We use the term "non-direct" to denote effects that may give rise to differences in demand behavior either before children are born or after they have left home, as well as when they are present (so that an alternative name would be "non-contemporaneous"). Using the same notation as previously, we can write:

$$w_{ht} = X'_{ht}\beta + \rho Z_h + e_{ht}, \tag{1.2}$$

where now the children variable does not have a time subscript so that t may refer to the period after the children have left home (or before they are born for households planning to have children). We will first present three sources of non-direct effects, and then discuss their impact on estimated demand parameters; since we believe that the heterogeneity is the likeliest indirect effect we devote a good deal of discussion to it. One other alternative is that differences in household composition are proxying for some excluded determinant of demand, for example the influence of family background. If this excluded variable is fixed for the household then this is, of course, indistinguishable from the case of correlated heterogeneity. In our empirical work we include the variables usually included in demand equations. If children are (partially) proxying for omitted variables then this implies that demand equations that do not include these variables will be misspecified.

1.2.1. Heterogeneity. That households differ in their tastes is widely accepted. Equally widely accepted is that children and demands are both objects of choice. The crucial point is how any heterogeneity is correlated across fertility and consumption behavior. To illustrate our concerns as regards the endogeneity of children we take a very simplified model of lifetime allocation—since we shall not attempt the estimation of any structural model below this simple model suffices for our purposes. We assume the household lives for two periods ($t = 1, 2$) and that children are present only in period 1 (i.e., children leave home at the end of period 1). For the moment we do not discuss how the number of children, z, is decided. There are two goods in each period with c_i and f_i denoting the consumption of good i in periods 1 and 2, respectively. To bring out the essential points without excessive complications we assume that there is no uncertainty and no time discounting. Let lifetime utility be given by:

$$V = \alpha \ln\left(c_1 - \frac{(\delta z + \gamma)}{(1-\alpha)}\right) + (1-\alpha)\ln(c_2) + \lambda z + \alpha \ln\left(f_1 - \frac{\gamma}{(1-\alpha)}\right) + (1-\alpha)\ln(f_2). \tag{1.3}$$

The parameter λ governs the "pure" taste for children. The parameters γ and δ capture the adult and child "subsistence needs" for good 1, respectively. With this functional form the "economic" costs of children are given by δz. If we take $\lambda > 0$ and $\delta > 0$ then we have that households prefer more children but they come at a cost to adult consumption. We assume that all discounted prices are set to unity so that the budget constraint is given by:

$$c_1 + c_2 + f_1 + f_2 = W, \tag{1.4}$$

where W is lifetime wealth. The within period (Marshallian) demands for good 1 in periods 1 and 2 are given by:

$$\hat{c}_1 = \alpha x_1 + \delta z + \gamma, \tag{1.5}$$

$$\hat{f}_1 = \alpha x_2 + \gamma, \tag{1.6}$$

where x_t is total expenditure in period t. These demands are, of course, independent of the "pure" taste for children (λ).

Suppose now that we had data on demands and demographics so that we could think of estimating the parameters in the demand equations above. We typically observe that households with the same right-hand side variables have different demands. Usually, we would allow for this by positing that there is measurement error and heterogeneity. Since the former raises issues of the endogeneity of total expenditure that are well understood and that are not our focus we assume that total expenditure is measured exactly so that variations in demand arise only from heterogeneity. In this case, the simplest form to assume is that the adult needs for

good 1, γ varies across households so that we have a simple additive error in equations (1.5) and (1.6). We now address two questions which are of particular relevance for the two samples we use in our empirical work below. First, when will the right-hand side variables be exogenous in the regressions for the sample of households of child-rearing age? Second, when will a variable for "past children" enter "significantly" the regressions for older households who do not have any children at home?

If children are simply "exogenously given" at level $\bar{z}$ then children and total expenditures ($x_1 = c_1 + c_2$ and $x_2 = f_1 + f_2$) are given by:

$$\begin{aligned} z &= \bar{z}, \\ \hat{x}_1 &= \frac{1}{2}\left(W + \frac{\delta \bar{z}}{(1-\alpha)}\right), \\ \hat{x}_2 &= \frac{1}{2}\left(W - \frac{\delta \bar{z}}{(1-\alpha)}\right). \end{aligned} \tag{1.7}$$

If we make the conventional assumption that the heterogeneity in preferences (γ) is uncorrelated with lifetime wealth and the "given" level of children then we can consistently estimate the parameters in equation (1.5) by OLS—that is, children and total expenditures are exogenous. Equally, if we add past children to the regression for the older households (equation (1.6)) they will not be "significant".

If, on the other hand, children are chosen according to the utility function above (and can be chosen in continuous amounts) then we have:

$$\begin{aligned} \hat{z} &= \max\left(0, \frac{(1-\alpha)\lambda W - 2(\delta + \lambda\gamma)}{\delta\lambda}\right), \\ \hat{x}_1 &= \frac{W}{2} + 1_{(\hat{z}>0)} * \left(\frac{W}{2} - \frac{\delta + \lambda\gamma}{\lambda(1-\alpha)}\right), \\ \hat{x}_2 &= \frac{W}{2} - 1_{(\hat{z}>0)} * \left(\frac{W}{2} - \frac{\delta + \lambda\gamma}{\lambda(1-\alpha)}\right). \end{aligned} \tag{1.8}$$

Now both children *and* total expenditure in each period depend on the heterogeneity terms in equations (1.5) and (1.6) so that OLS will yield inconsistent estimates of both parameters. This result holds even though the heterogeneity in the "pure" tastes for children (that is, the parameter λ) is independent of the tastes over goods (that is, the parameter γ). The mechanism at play is the following. If households differ in their preferences over goods (as captured by γ), they may make different life cycle choices. Households whose taste for goods is relatively large will tend to have less children, hence they will have lower total expenditure in the first period.[7] This creates a correlation between total expenditure and the error term in the demand equations for both periods 1 and 2. It also creates a correlation between children and the error term in the demand equation for period 1.[8] Thus this example illustrates that simply allowing that there is some heterogeneity in the tastes for goods and that children are a choice variable and that households adapt their expenditures to the life-cycle

pattern of children is sufficient to jeopardize identification of *all* parameters for a sample of households of child-rearing age. Thus the biases that arise from the endogeneity of children will be pervasive. Moreover, past children will appear to be "significant" in a sample of older households who no longer have children at home. This motivates our empirical investigation on the older sub-sample in our data.

1.2.2. State dependence. In the current context, state dependence refers to behavior changing with the birth of children and these changes possibly persisting when the children are older or even after they have left home. Thus the argument assumes habit formation with children leading to changes in behavior which themselves lead to changes in later behavior. For example, for the restaurant example given in the introduction, it can be that the parent's consumption of restaurant meals changes when they have children, and after the children leave, either the parents will get back into the habit of going to the restaurant immediately (there is no state dependence), or they will get back into it after a while, or even never. As another example,[9] suppose that people who have children choose to live in larger houses and then stay in the same residence after the children have left. If preferences over non-durables are not separable from the size of house then if we do not control for the latter, it will appear as though children have an indirect effect. In both these cases, there is state dependence. Of course, this argument is corrosive of all conventional demand studies which maintain intertemporal separability for non-durables so that a finding of state dependence would have far-reaching consequences for modelling demands.

1.2.3. Intra-household effect. The intra-household effect is another type of effect altogether. Indeed, it supposes that the unitary setting (in which there is a household utility function) is rejected, and that each adult household member is characterized by her/his own preferences (for detailed studies, see for instance Martin Browning et al., 1994 or Martin Browning and Pierre-André Chiappori, 1998). In a collective setting, regardless of the preference structure, the addition of a household member can lead to a modification in the power structure, and induce a modification in the budget structure. The direction of the change cannot be predicted *a priori*, for it depends on both the relative powers and the preferences of household members.

To conclude this sub-section, we note that even though the cross-section we use below has much richer retrospective information than is usual for expenditure surveys we do not believe it is possible to discriminate plausibly between different indirect effects. Distinguishing between heterogeneity and state dependence would require either panel data or a sample including young households that have different (and observable) fertility plans. Identifying intra-household effects would be feasible within a structural framework, and has been the object of other work (Browning et al., 1994); it is not the object of this paper. In our data analysis we shall only test for the presence of non-direct effects; the identification of which is driving our findings must be left to later work when better data are available.

2. The data and samples

The data comes from the French family expenditure survey "Enquete sur les Budgets des Familles" (EBF), conducted by the French National Institute of Statistics, INSEE, in 1984–85. The sample size is 11,977 households and it is representative of the French population. Besides the usual expenditure survey information, this survey has one feature which allowed us to use it for our purpose. Indeed, one of its aims was to study the living standards of the over 55 years old population, so that the survey contains a specific module with not only the usual questions about the current composition of the household, but also questions relating to children not living at home.[10] Furthermore, the survey also contains, for all households surveyed, a module about the background of the adults in the survey; this information is the source of our instruments for children in our demand equations.

We use two different samples for the tests. The first sample—the "over 55" sample—consists of 1089 married couples in which the husband is over 55, there is no one else living in the household (this excludes older households that still have children living at home) and in which both members are out of the labor force (either retired or non-participating). This latter selection condition is partly imposed by the survey design which asks particular questions that we use only of households in which the husband is retired. The great majority of these households also have the wife out of the labor force (either retired or "not in market work") and we choose to select out the households in which the wife is still in the labor force. We could, of course, include households in which the wife is in paid work and allow that some of our parameters depend on this. Viewed in this light the sampling decision we have made can be seen as a decision to allow that all of the parameters depend on the wife's labor supply and we do not have a large enough set of "wife employed" households to reliably estimate effects for this group. At worst this involves some loss of efficiency. The more important issue is that we take the labor supply of women married to retired men to be exogenous to the household demand decisions, once we condition on completed fertility and all of our other right-hand side variables. Modelling this theoretically is clearly beyond the scope of this study and the data requirements for testing for this would be formidable. Thus we choose to assume that the wife's labor supply is exogenous to demand behavior (once we condition on past fertility) and that any indirect children effects that we isolate are appropriate for households in which the husband is retired and the wife is out of the labor force. We would not extrapolate these effects to other households.

The second sample consists of households in which the head is between 20 and 55 years old, so that they are susceptible to having dependent children; we refer to this sample as the "Under 55" sample. More precisely, it contains 1491 households, headed by a married couple, with or without dependent children but with no other relative sharing the same dwelling, and in which both members work full time for a wage.[11] We take the labor supply decision to be exogenous for demand behavior, conditional on the instruments we describe below (which include family background

variables) and thus treat this as an exogenous sample selection. This exogeneity assumption is clearly one of convenience and cannot be justified in any rigorous way by appeal to a joint model of demand, fertility and labor supply. Lacking such a model, we cannot even present any credible analysis of the effect of ignoring endogeneity. Once again we emphasize that any effects found will not necessarily be the same as for other groups, such as households in which the wife is not employed full-time.

We limit the study to the budget shares of four non-durable goods: food at home (excluding alcoholic beverages), restaurants, services, and recreation. All together, these four budget shares represent, on average, 81 and 68 percent of non-durable expenditure for the "over 55" and "under 55" samples respectively (see Table 1 which also includes details of the goods we do not model). Services includes telephone, water, gas, electricity, heating, cleaning, hairdressing, cleaning products etc. Recreation expenditure records all non-durable expenditures on books, newspapers and magazines, tapes, cinema tickets, vacation expenditures etc. Some summary statistics for the budget shares are given in Table 1 and summary statistics for other variables are given in Table 2. For the goods we are studying, and for the other goods that form total expenditure on non-durables, the recording method is either a two week diary or a two months retrospective question, so that there might be some lumpiness in the data. To correct for this, we treat total expenditure as endogenous, and instrument it with functions of household total income. Since in the "over 55" sample both members are out of the labor force, income is constituted by retirement pensions, income from assets and health and social benefits. The survey records monetary transfers between households, so that we have been able to control that there are no reported income flows between the parents and the children's households in our sample.

Table 1. Structure of non-durable expenditure in both samples.

	Under 55		Over 55	
Budget share	Mean	% Zero	Mean	% Zero
Food at home	31.0	0	43.5	0
Restaurant	9.6	11.3	2.9	58.6
Services	16.1	0	24.1	0
Recreation	11.6	0.5	10.8	3.7
Alcohol	2.6	28.7	4.3	19.7
Tobacco	1.6	45.8	0.6	75.1
Men's clothing	4.0	25.7	2.5	55.9
Women's clothing	5.2	17.0	3.5	42.7
Various clothing	0.9	69.2	0.7	77.9
Children's clothing	2.3	47.2	0.1	98.6
Transportation	15.0	3.5	7.1	31.3

Table 2. Means and ranges of variation of the variables.

	Under 55 sample		Over 55 sample	
	Mean	Range	Mean	Range
Husband's age	35.74	20–54	69.67	55–94
Wife's age	33.86	17–63	66.58	46–89
Husband's education	10.57	6–15	8.43	6–15
Wife's education	10.50	6–15	7.74	6–15
Home owner	0.56	0–1	0.72	0–1
State dwelling	0.15	0–1	0.09	0–1
Total expenditure on non-durables	8.52	1.97–31.04	5.57	0.90–26.46
Log(total expenditure)	2.05	0.68–3.43	1.60	− 0.11–3.27
Paris	0.19	0–1	0.1	0–1
Rural	0.23	0–1	0.30	0–1
Town	0.59	0–1	0.60	0–1
Paris suburb	0.16	0–1	0.08	0–1
Summer	0.20	0–1	0.20	0–1
Fall	0.31	0–1	0.28	0–1
Winter	0.19	0–1	0.22	0–1
Spring	0.30	0–1	0.30	0–1
Year 2	0.55	0–1	0.57	0–1
Father of spouse white collar	0.13	0–1	0.07	0–1
Father of spouse blue collar	0.81	0–1	0.82	0–1
Father of spouse other or unknown	0.06	0–1	0.11	0–1
Mother of spouse white collar	0.04	0–1	0.01	0–1
Mother of spouse blue collar	0.37	0–1	0.41	0–1
Mother of spouse other	0.57	0–1	0.51	0–1
Mother of spouse unknown	0.02	0–1	0.06	0–1
Foreign origin of husband	0.03	0–1	0.04	0–1
Foreign origin of wife	0.03	0–1	0.03	0–1
Husband worker	0.52	0–1		
Husband employe	0.30	0–1		
Husband other (farmer, self-employed or other)	0.18	0–1		
Wife worker	0.36	0–1		
Wife employe	0.50	0–1		
Wife other (farmer, self-employed or other)	0.14	0–1		
Disability dummy			0.36	0–1
Number of children	1.2	0–5		
Number of young children	0.37	0–3		
Number of old children	0.83	0–5		
Rare visits			0.28	0–1
Weekly visits			0.28	0–1
Daily visits			0.31	0–1
Nb of observations	1491		1087	

Total expenditure is in 10,000 francs.

One concern we have is that the expenditure patterns of some households in the "over 55" sample might reflect impaired health status. We can only partially control for this by making use of a set of survey questions designed to measure the degree of disability. Households are asked whether either member has difficulty going up or down a flight of stairs alone, and whether they can go outside of their home alone. We used these two questions as indicators of disability which might alter expenditure patterns.

Another major concern with the "over 55" sample, is the possible effects on expenditures induced by the closeness between the households under study and their off-spring. For a large fraction of households in the sample, it is the case that the children's households are very close to the parents' household, and frequent visits are reported. For each of the children alive, we know the distance in kilometers between them and the parents' household, as well as an indicator, on a scale of one to six, of the frequency of visits by the child or anyone from his or her household. The frequency of visits is recorded as occurring almost every day (for 35 percent of households reporting children), more than once a week (32 percent), more than once a month (16 percent) or less than once a month (16 percent). Having discarded a few households for which the distance and the frequency of visits showed a discrepancy that could not be corrected,[12] we condition demands on an indicator of the frequency of visits. What we are trying to capture with these variables is something like variations in "actual" family size. However, these variables do not represent children's effect on expenditures, since they refer to visits by an unknown number of adult children and their spouses and offspring. In our tests, we will therefore distinguish between four groups of households: those who have not had children, those who have had children who visit less than once a week, those whose children visit more than once a week but less than daily and those who have children who visit almost every day.[13]

For food at home and restaurant, and for most of the expenditures on services and recreation, the recording period is 2 weeks. Amongst the "rare visits" group, some but not all households will have received visits from their children during the survey period. Hence, observed behavior of those will be closer to the behavior of those who have a higher frequency of visits by their children. The effect of this problem of measurement is therefore to attenuate the observed difference between our comparison household groups.

Finally, we address the problem that arises from the fact that households are not asked about completed fertility, but about the number of children alive. Some households are therefore miscategorized as childless. We could present the formal analysis of the effect of this one sided measurement error, but it is simpler to consider an extreme case. Suppose all households in the survey had children, and that reporting no children indicated that the children have not survived. Then, even if there is a non-direct effect, the coefficient on the children indicator in demand will be zero. This shows that the measurement error biases the coefficient towards zero, and that if it were possible to correct for this measurement error, it would actually strengthen our results.

3. Testing for non-direct effects

Our aim is to see whether there is a difference between expenditure patterns of "over 55" households depending upon whether they have had children or not. To do so, we concentrate on whether an older couple ever had children and ignore the numbers of these; thus in the regressions, children are represented by a simple dummy variable.[14] We examine four household groups: group 1 are the childless households (133 households, or 12.21 percent of the sample), in group 2 are 308 households (28.28 percent) whose children visit less than once a week (as a short hand the "rare visit" group), in group 3 are 308 households whose children visit more than once a week (the "weekly visit" group), and in group 4 are 340 households (31.22 percent) whose children visit almost every day (the "daily visit" group). What we are really interested in is the comparison between behavior of childless households and households that have children who do not visit. The closest we can get to this is to compare food budget shares of groups 1 and 2, that is, childless households, and households whose children visit rarely.

We present regression results including the usual controls and indicators for "past children" in Table 3. Because of the number of zeros for restaurants, we use a Tobit for that good. To choose the set of conditioning variables and the specification, we proceed in the following manner: based upon previous demand studies both on the same data base (Veronique Nichèle and Jean-Marc Robin, 1993) and on other data bases (James Banks, Richard Blundell, and Arthur Lewbel, 1997), we select a set of variables that we feel ought certainly to be included in the budget share equations. These are: the age and age squared of the husband, region of residence, season of the interview (the survey spanned almost a year, and stretched over 2 calendar years, 1984 and 1985) and the log and log squared of total expenditure. The regressions also include the age and age squared of the wife, a disability indicator and a dummy for owning a home. These variables are not usually found on the right-hand side of a demand equation. As for age, it is usually enough to condition on the husband's age. It appeared that this is not the case here. We attribute this as being due to the peculiarity of our sample, in which average ages are 70 for husbands and 67 for wives, and where it can be thought that the age of each household member plays a role in shaping the expenditure. As for the dummy indicating home ownership, standard two-stage budgeting hypothesis tell us that it has nothing to do in a demand system. Furthermore, it could be argued that it is endogenous. However, removing it from the equations does not qualitatively change the results, including it does improve the fit, and it is also the only way we have to control for the income in kind accruing to home owners.

To instrument total expenditure, the set of instruments used is (log) household income and its square, interactions between the income terms and both spouses' age variables, a dummy indicating living in state owned housing and a year dummy indicating the survey was taken in 1985 for some households; in all we have 10 over-identifying restrictions. Tests for the over-identifying restrictions[15] and for the exogeneity of total expenditure[16] are also presented in Table 3.

Table 3. IVE results for the over 55's.

	Food at home	Restaurant	Services	Recreation
Rare visits	−3.43 (1.33)	0.30 (1.64)	3.58 (1.27)	−0.95 (1.22)
Weekly visits	−1.45 (1.31)	0.46 (1.64)	2.80 (1.12)	−2.61 (1.20)
Daily visits	0.48 (1.31)	−0.85 (1.70)	2.49 (1.12)	−2.81 (1.18)
Log(total expenditure)	−15.35 (11.72)	−17.65 (19.58)	7.70 (12.70)	19.46 (10.88)
Log(total expenditure) square	−2.64 (3.37)	8.35 (5.73)	−2.38 (3.70)	−2.58 (3.25)
Husband's age	0.02 (0.09)	−0.85 (1.16)	−0.02 (1.00)	−0.88 (0.90)
Husband's age square	−0.00 (0.01)	0.01 (0.01)	0.00 (0.01)	0.01 (0.01)
Wife's age	0.86 (0.95)	−0.23 (1.46)	−2.01 (0.91)	1.35 (0.60)
Wife's age square	−0.01 (0.01)	0.00 (0.01)	0.02 (0.01)	−0.01 (0.00)
Rural	−2.10 (2.05)	−3.47 (2.63)	3.64 (2.22)	−1.85 (1.61)
Town	−2.68 (1.91)	−1.55 (2.51)	3.21 (2.14)	0.10 (1.54)
Paris suburb	1.05 (2.24)	−5.03 (2.81)	1.59 (2.33)	1.61 (1.80)
Home owner	−0.25 (0.90)	−1.61 (1.10)	4.12 (0.82)	−2.23 (0.70)
Winter	0.16 (1.07)	−0.32 (1.31)	0.37 (1.01)	0.01 (0.87)
Spring	−0.04 (0.97)	0.04 (1.21)	2.46 (0.92)	−0.68 (0.80)
Summer	2.00 (1.08)	2.35 (1.37)	−0.17 (1.00)	−1.84 (0.84)
Disability	−1.16 (0.86)	0.11 (1.00)	2.58 (0.80)	−1.39 (0.64)
Constant	57.01 (35.21)	42.93 (65.61)	69.26 (32.74)	−26.54 (26.73)
OIR test: $\chi^2(10)$	2.82		13.15	6.93
P under H_0	(98.54)		(21.51)	(73.18)
Exog. of tot. exp. $F(2,1067)$[a]	6.62	1.18	1.12	6.17
P under H_0	(0.14)	(55.42)	(32.82)	(0.22)
Test for omitted vars.				
$F(3,1066)$[b]	1.00	3.31	0.82	1.00
P under H_0	(39.14)	(34.67)	(48.25)	(39.28)
Nb. of obs.		1087		

Robust standard errors between brackets. Coefficients, standard errors and probabilities have been multiplied by 100. Restaurant estimated using Tobit. Significance at 5%: ratio of estimated coefficient to estimated standard error greater than 1.96, at 10%: 1.645.

[a]For restaurants, read: $\chi^2(2)$ instead of $F(2,1067)$.

[b]For restaurants, read: $\chi^2(3)$ instead of $F(3,1066)$.

As can be seen from Table 3, for the usual controls the results are unexceptional. The budget share of food at home, for instance, is decreasing in total expenditure. However, not much seems to significantly affect the budget structure of over-55's. It should be noted though that disability negatively affects the share of recreation expenditures and positively that of services. Turning to the parameter estimates of most interest to us, we see that there are significant differences in "food at home" and "services" between those who have no children and those who have children who rarely visit. The fact that the two effects are equal in magnitude but opposite in sign suggests that there is no effect on the excluded composite commodity category (alcohol, tobacco, clothing etc.). Since we are considering non-durable expenditure, and we are controlling for the interactions between the parent's and the children's

household,[17] this result suggests an indirect effect. It indicates that having had children affects the allocation of total expenditure on non-durables between goods, even when the children are not there and do not participate in the household's daily life.In other words, although our sample sizes are small (and the likely biases are toward finding no effect) it does appear that there are statistically significant non-direct effects; the "rare visit" group has a significantly lower budget share for food and a significantly higher budget share for services. For both these goods, the expenditure structure of the childless group is closer to that of the groups with children who visit often than it is to the expenditure structure of the group with children who rarely visit. In the case of food, it can be seen that those who have regular visits have a higher food budget share than those whose children do not visit regularly. This is consistent with the usual contemporaneous Engel direct effect.

4. Testing for exogeneity of children

The analysis in this section is based exclusively on the "under 55" sample, which we use to implement a conventional exogeneity test for children in demand. The children variables we include are a dummy for having children present, the number of young children (aged less than 6) and the number of older children (aged 7–18). Thus there are five potentially endogenous variables on the right-hand side of our budget share equations: log total expenditure and its square, and the three children variables. We use the same set of instruments as for the "over 55" 's (where we were instrumenting for total expenditure and its square), to which we add: dummies indicating a foreign origin of either adult member of the household (note that there is not very much variability in this variable, since 92 percent of the sample is French by origin); the profession of the wife (as manual worker, employee or other); the professions of the mother and the father of the wife; and the wife's age and age squared (but note these were included in the control variables for the over-55's analysis). What we are trying to capture with these instruments is cultural differences which are likely to lead to differences in fertility choices. Since the sample is composed of couples in which both members work full time, differences in jobs do not reflect differences in hours worked. Ideally, our choice of instruments would come from a well-specified model of lifetime dynamics. Although there are such models (e.g., Joseph Hotz and Robert Miller, 1988), we take a more reduced form approach and we satisfy ourselves with the facts that the variables we use as instruments do have some explanatory power in the reduced form; they are not rejected by the over-identifying tests and our instruments are similar to those used in previous studies looking at the exogeneity of children in the labor supply context. For example, Glenn Cain and Martin Dooley (1976) use religion and rural, residential and industrial structure; T. Paul Schultz (1978) uses the wife's origin and age and the schooling of both parents; and Thomas Mroz (1987) uses polynomials in the wife's age and education. In all we have 19 excluded variables so that we have 14 over-identifying restrictions per demand equation. In Table 4, we present estimates for the auxiliary equations for the

Table 4. Auxiliary equations for under 55's.

	Children dummy	Number of young children	Number of old children
Husband's age	79.03 (66.07)	51.33 (96.03)	−96.72 (130.28)
Husband's age square	−0.86 (0.85)	−0.34 (1.24)	1.37 (1.68)
Rural	11.92 (6.15)	−0.21 (8.94)	21.57 (12.13)
Town	11.72 (5.79)	−4.32 (8.41)	21.89 (11.41)
Paris suburb	8.54 (6.15)	−8.65 (8.94)	18.06 (12.13)
Home owner	8.27 (2.53)	11.27 (3.68)	5.53 (4.99)
Winter	0.66 (4.56)	−9.36 (6.63)	8.79 (8.99)
Spring	1.95 (4.63)	−9.12 (6.73)	9.35 (9.14)
Summer	−2.56 (3.31)	−10.70 (4.82)	8.26 (6.53)
Husband worker	6.99 (3.64)	5.97 (5.30)	2.59 (7.19)
Husband employe	1.32 (3.51)	2.71 (5.09)	−8.39 (6.91)
Log(income)	1259.97 (911.96)	769.32 (1325.49)	−277.08 (1798.25)
Log(income) square	−231.21 (165.66)	−62.90 (240.78)	−125.92 (326.66)
Wife's age	12.33 (1.40)	−1.87 (2.03)	26.94 (2.76)
Wife's age square	−0.16 (0.02)	−0.00 (0.03)	−0.34 (0.04)
Year 2	−4.42 (3.88)	2.72 (5.64)	−11.50 (7.66)
State owned dwelling	10.22 (3.25)	14.14 (4.72)	12.00 (6.40)
Husband's age × log(income)	−52.09 (47.65)	−17.97 (69.26)	36.61 (93.96)
Husband's age square × log(income)	0.55 (0.61)	0.02 (0.89)	−0.60 (1.20)
Husband's age × log(income) square	9.75 (8.55)	−0.13 (12.43)	2.47 (16.87)
Husband's age square × log(income) square	−0.10 (0.11)	0.03 (0.16)	0.00 (0.21)
Husband foreign	5.56 (7.84)	1.35 (11.39)	8.57 (15.46)
Wife foreign	7.21 (9.03)	10.77 (13.12)	3.38 (17.80)
Wife worker	10.12 (3.75)	3.23 (5.45)	15.89 (7.39)
Wife employee	3.79 (3.39)	−0.49 (4.93)	4.51 (6.69)
Mother of spouse white collar	13.45 (8.50)	0.97 (12.35)	24.19 (16.75)
Mother of spouse blue collar	9.11 (6.91)	−6.77 (10.04)	27.58 (13.62)
Mother of spouse other	6.81 (6.88)	−8.11 (10.01)	20.07 (13.58)
Father of spouse white collar	−4.54 (5.01)	1.05 (7.29)	−11.73 (9.88)
Father of spouse blue collar	−2.11 (4.13)	4.48 (6.01)	−10.17 (8.15)
Constant	−2058.59 (1249.93)	−1525.64 (1816.71)	805.42 (2464.67)

Standard errors between brackets. Coefficients and standard errors have been multiplied by 100. Significance at 5%: ratio of estimated coefficient to estimated standard error greater than 1.96, at 10%: 1.645.

children variables. The $F(19,1460)$ statistics for the excluded variables in these three equations take values of 6.31, 4.50 and 9.64, respectively; thus we do not seem to have any "weak instrument" problem.

The *2SLS* estimates for the four budget share equations are presented in Table 5. The results for the non-children variables are unexceptional. The budget share of food is decreasing in total expenditure and increasing in children; Parisians spend a significantly higher proportion of total expenditure on restaurants, etc. In Table 6, we present the test statistics for the over-identifying restriction tests and the

Table 5. IVE results for the under 55's.

	Food at home	Restaurant	Services	Recreation
Children dummy	18.11 (7.26)	−5.15 (6.87)	7.96 (5.39)	−6.36 (5.66)
Number of young children	−4.70 (3.96)	3.25 (3.63)	−6.36 (3.04)	−0.25 (2.93)
Number of old children	−4.50 (3.23)	0.47 (2.95)	−2.58 (2.37)	1.71 (2.37)
Log(total expenditure)	−39.50 (15.62)	17.16 (12.56)	−18.94 (11.16)	13.05 (11.34)
Log(total expenditure) square	5.65 (3.68)	−1.49 (3.03)	4.54 (2.62)	−1.85 (2.72)
Husband's age	−0.24 (0.70)	0.45 (0.62)	−0.38 (0.51)	0.53 (0.55)
Husband's age square	0.01 (0.01)	−0.01 (0.01)	0.00 (0.01)	−0.01 (0.01)
Rural	−1.40 (1.84)	−6.21 (1.59)	3.19 (1.19)	−0.97 (1.24)
Town	−0.55 (1.76)	−5.28 (1.52)	1.75 (1.10)	0.65 (1.18)
Paris suburb	−0.98 (1.88)	−4.53 (1.58)	1.21 (1.20)	1.92 (1.30)
Home owner	0.22 (0.72)	0.34 (0.58)	3.16 (0.53)	−0.49 (0.52)
Winter	−1.29 (0.85)	0.48 (0.76)	1.57 (0.65)	−1.39 (0.59)
Spring	0.07 (0.72)	−0.67 (0.64)	2.16 (0.55)	−0.19 (0.56)
Summer	0.51 (0.90)	−2.09 (0.73)	1.21 (0.63)	−0.69 (0.62)
Husband worker	1.82 (1.04)	1.65 (0.95)	0.51 (0.81)	−1.93 (0.79)
Husband employee	0.44 (0.95)	3.64 (0.88)	−0.02 (0.75)	−1.25 (0.77)
Constant	76.75 (19.19)	−19.63 (15.94)	36.14 (14.04)	−9.18 (13.96)
OIR test $\chi^2(14)$	12.3		9.58	15.97
P under H_0	(58.25)		(79.21)	(32.25)
Test for omitted vars. $F(3,1471)$[a]	1.32	6.09	0.50	1.58
P under H_0	(26.49)	(10.72)	(68.15)	(19.27)

Robust standard errors between brackets. Coefficients and standard errors have been multiplied by 100. Restaurant estimated using Tobit.

[a]For restaurants, read: $\chi^2(3)$. Significance at 5%: ratio of estimated coefficient to estimated standard error greater than 1.96, at 10%: 1.645.

Table 6. Exogeneity tests.

	Food at home	Restaurant	Services	Recreation
Expenditure and children $F(5,1469)$[a]	3.61 (0.30)	26.73 (0.01)	4.88 (0.02)	0.98 (43.07)
Expenditure $F(2,1469)$[b]	2.99 (5.05)	13.78 (0.10)	5.64 (0.36)	0.33 (71.79)
Children $F(3,1469)$[c]	2.02 (10.96)	6.79 (7.90)	3.46 (1.58)	1.09 (35.08)
Children for expenditure instrumented $F(3,1471)$[d]	1.97 (11.70)	6.79 (7.88)	3.59 (1.33)	0.77 (50.95)

For restaurant, read: [a]$\chi^2(5)$ instead of $F(5,1469)$, [b]$\chi^2(2)$ instead of $F(2,1469)$, [c]$\chi^2(3)$ instead of $F(3,1469)$, [d]$\chi^2(3)$ instead of $F(3,1471)$.

(augmented residual) exogeneity tests. The tests for the over-identifying restrictions do not reject. In the case of the children variables, exogeneity is rejected in the services equation (at 5 percent) and in the restaurant equation (at 10 percent). Our conclusion is that there is somewhat weak evidence that the exogeneity of children is rejected but our sample size is too small to allow us to regard this rejection as decisive.

5. Substantive effects

To illustrate the implications of our results, we present calculations of the direct and non-direct effects of children obtained from the estimates on both the "under 55" and "over 55" samples, as well as expenditure elasticities based on the under-55 sample for several different specifications.

If there are no cohort effects in preferences and if the distribution of heterogeneity is the same in both samples, then it is valid to compare the scale of children effects obtained in the two samples of "over 55" and "under 55" households. Furthermore, if the direct and non-direct effects of children are additive, then the direct effect of children can be obtained as the difference between the total effect, estimated on the "under 55" and the non-direct effect, estimated on the "over 55". For the over-55 sample, the IV parameter estimates given in Table 3 indicate that having had children (who do not visit) lowers the food budget share by 3.4 percentage points. It follows that the direct effect is 3.4 percent higher than the parameter estimate in a "children present" regression. Looking at the food Engel curve for the "under 55" sample (Table 5), we see that for a household with one child less than 6 years old, the estimated effect is 13.4 percent so that the direct effect is actually +16.8 percent. Thus conventional estimates of the direct effect of children on food of children are likely to be under-estimates, although the actual bias is relatively small. By the same calculation, we see that the estimated total effect of one child less than 6 years old is +1.6 percent for services, so that the direct effect is actually −1.98 percent. For restaurants and recreation, none of the relevant estimated coefficient is significantly different from zero.

Turning to the estimated elasticities, in the first column of Table 7, the median expenditure elasticity is calculated when the budget share is regressed on the set of variables containing total expenditure but no children variables. The second column also contains results from OLS, but when children variables are included in the regression. Finally, the last three columns report results of 2SLS. In the first of these, we instrument total expenditure and its square but do not condition on children. The next column reports elasticities obtained when we condition on children and

Table 7. Expenditure elasticities.

	No IVE		2SLS		
Children included	No	Yes	No	Yes	Yes
Variables instrumented	—	—	Exp.	Exp.	Exp. and Ch.
Food at home	0.62	0.58	0.49	0.40	0.47
Services	0.74	0.74	0.91	0.91	0.98
Recreation	1.42	1.49	1.47	1.60	1.53
Restaurant					
Overall elasticity	0.44	0.43	0.93	1.00	0.95
For positive observations	0.54	0.52	1.19	1.29	1.22

instrument total expenditure and its square; this is the most usual specification. In the last specification, we instrument both the children and the expenditure variables. For restaurant expenditures, because of zero expenditure for a non-negligible fraction of the sample, the notion of elasticity depends on what one is interested in. One can be interested in knowing what is the overall elasticity, or what is the relative change in the restaurant expenditure of those whose expenditure on restaurant is positive, or the change in the fraction of the population who does have positive expenditure on restaurant. We present below the results obtained for the first two modes of calculation, for the five specifications reported for the other goods.

The first thing to note is that conditioning on children or not does not make much difference for the expenditure elasticities if no account is taken of the possible endogeneity of total expenditure. When, as is usual practice, expenditure is instrumented, there is some difference, but here again, the treatment of the children variables does not have a large effect on estimated median income elasticities. Thus ignoring the endogeneity of children leads to a relatively small bias in the income elasticity.

6. Conclusion

There are well established correlations between children and demands, such as higher food budget shares when children are present. Opinions differ as to the source of these correlations, as well as to the appropriate statistical treatment of children in demand, but the literature is short of tests on this, presumably for lack of suitable data. In the first part of this paper, we presented an extended discussion of direct and indirect effects and how these impact on some child related issues. We then presented an example that showed that if there is heterogeneity in the tastes for goods then children variables and total expenditure in a conventional Engel curve analysis are potentially endogenous. This example is worrying since the assumptions that give rise to the endogeneity are quite widely accepted.Thus the question becomes: given that some of the coefficients in an Engel curve analysis will be biased if children are a choice variable, how bad is the bias?

In the empirical sections we presented two separate tests. The first of these examines the expenditure patterns of older households with no children currently present to see whether there are significant differences between those who have had children and those who have not. Despite our small sample size we find significant non-direct effects. As discussed above, three possible sources of these non-direct effects are heterogeneity, state dependence or intra-household effects. Given the available data, it does not seem feasible to us to convincingly test between these three sources. The second of our tests is a conventional exogeneity test on households with dependent children. The results are inconclusive. Although there is some weak evidence of endogeneity we need better instruments for fertility or larger sample sizes to produce a decisive test along these conventional lines.

Our finding that there is evidence of statistically significant non-direct effects is consistent with our theoretical priors. As discussed above, however, the more important point is whether the bias induced by the endogeneity of children is large. The analysis presented in the last section suggests that the bias is, in fact, quite small. Given this, we conclude that although allowance for the endogeneity of children in demands should be made if the data allows it, ignoring this concern will not lead to substantial inferential errors.

Appendix A.1. Selection of the samples

The data base we start with contains 11,977 households. From this, we extract two samples according to whether the household head is older than 55 or not, and to a number of other criteria, detailed below.

The "under 55" sample is composed of couples, with or without dependant children, in which the head of household is less than 55 years old, and in which both head and spouse work full time for a wage. We have had to remove 975 households in which either member declares a number of hours that is not compatible with a very loose definition of full time, namely more than 120.6 h per month (this corresponds to 29 h per week). We also remove 427 households whose income is much too low to have been accurately recorded. The cut-off we choose is 40,000 francs per year per individual (approximately U.S. $8000). For an individual working full time (39 h per week, that is, 1800 h per year) this corresponds to a wage of 22 francs per hour, much lower than the minimum wage. We then also remove 102 households in which the children are in the labor force, 8 households in which either spouse is self-employed, and 19 households that are clearly inconsistent. The sample we obtain contains 1492 households.

The "over 55" sample is composed of couples with no children living at home, in which both member are out of the labor force. This selection criterion yields a sample of 1323 households. We then remove 217 households in which household income is too low to have been accurately declared. We finally remove 17 households that are clearly inconsistent, and end up with a sample of 1089 households.

Notes

1. Strictly, the preferences that are imposed on them by their parents.
2. Here, we use "conditional on children responses" simply to mean that we put children on the right-hand side of some behavioral equation, whether or not we allow for the endogeneity of children.
3. Although some investigators test for the exogeneity of children in labor supply (see Browning, 1992 for references).
4. This does not allow for the effects induced by children visiting their parents. As will be seen, we can control for this in our analysis.
5. We omit time subscripts on the parameters of the equation. In a dynamic setting, these can be made to depend upon prices.

6. An increase in the budget share of public goods when household size increases can be obtained for example with linear preferences, and a decrease with Leontief preferences.
7. For a general utility function a sufficient condition for this is concavity.
8. Note that this would be the case even if the pure taste for children λ were constant across households.
9. We thank a referee for this suggestion.
10. This causes a problem since households are not asked about completed fertility, but about the number of children alive. We discuss the effect of this particular type of measurement error below.
11. For both a detailed description of the variables used and an explanation of why we start with a data base of 11,977 households and end up with samples of this size, refer to the appendix.
12. There is some arbitrariness in the distance we decided to be too large for a given frequency of visit. For daily visits, we dropped one household for which the distance is 260 km. For weekly visits, we allowed for distances less than 120 km, and for monthly visit, for 500 km.
13. It could be that the frequency of visits of children is endogenous, for instance if it depends on the taste for adult goods. We have shown in the heterogeneity example in section 2 the conditions under which OLS will yield inconsistent estimates of the effect of past children on demand. We do not believe that it is possible to correct for the potential bias on the children coefficient with the data at hand. Therefore, we assume that visits by children are exogenous.
14. It would be interesting to go further and generate evidence on the effects of the size of the family. We do not believe that we can make such demands on the data we have.
15. The test statistic is the nR^2 for the regression of the residuals of the main equation, where the endogenous variable has been replaced by its predicted value, on the instruments, see Russell Davidson and James MacKinnon (1993).
16. The null is that total expenditure is exogenous in demand. Hence, under the null, neither residual (total expenditure and total expenditure squared) should be significant in the demand equation.
17. Transfers in-kind, which might escape recording, are more likely to be occurring between households that visit than between households that do not visit. Therefore, the possibility of under-recording of transfers in-kind should not alter the comparisons made between the no children group and the rare visit group.

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