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A Critical Analysis of
Empirical Studies
of Transfers in Japan

by

David W. Campbell*

Working Paper No. 53

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of Bard College

*The Jerome Levy Economics Institute

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1. INTRODUCTION

This paper is the first comprehensive critical analysis of the empirical studies of transfers in Japan, an area of research which dates only from 1986.¹ The three articles I examine are: Hayashi's (1986) "Why is Japan's Saving Rate So Apparently High?", Hayashi, Ando, and Ferris's (December 1988) "Life Cycle and Bequest Savings", and Dekle's (1989) "The Unimportance of Intergenerational Transfers in Japan." The first article, which covers the 1969-74 period, is the most careful published attempt to date to quantify the amount of intergenerational transfers in Japan. The second is a preliminary attempt to estimate the flow of transfer wealth in 1984 in the household sector in Japan. The third study measures the amounts of intergenerational transfers received over the 1968-83 period by the 40-44, 45-49, 50-54, and 55-59 generations which head all worker households (two-or-more person nuclear, extended, and other families) in 1983.

In Section 2 I elucidate the Modigliani and Kotlikoff and Summers definitions of transfer wealth. This is a necessary introduction to Section 3 which looks at the definitions of transfer wealth actually used by the authors above. Sections 4 and 5 present analyses of their estimation of respectively accumulated wealth and life cycle saving.

2. DEFINITIONAL ISSUES

Recently there has been a lively controversy in the literature over exactly what is the appropriate definition of transfers (Cf., Kotlikoff (1988), Modigliani (1986, 1988) and Kotlikoff and Summers (1981, 1986)). In this section I clearly delineate and compare the contrasting views of Modigliani and Kotlikoff and Summers.

The Kotlikoff–Summers definition is particularly straightforward from a conceptual standpoint. K–S define all net gifts received by the individual as transfers. In addition all of the nominal return attributable to gifts is included in the transfer wealth of the individual. In the aggregate transfer transactions among living individuals cancel out; the stock of aggregate transfer wealth is then the amount of net transfers received from those dead by those still living, accumulated at the rates of total nominal return (nominal rent plus nominal capital gains of assets).

The Modigliani definition uses a more complicated conceptual framework. It draws a distinction between gifts received that are used to support current consumption and gifts received that are saved. The former is not counted as a transfer, the latter is. Transfer wealth for an individual is the sum of all net transfers received adjusted for inflation, i.e., the real value of transfers is maintained. In the aggregate, as in the K–S case, transfer transactions among living individuals cancel out; the stock of aggregate transfer wealth is then the total of net transfers from those dead to those still living, accumulated at the inflation rates (inflation rates here are broadly based measures of inflation such as the CPI).

These definitions are made mathematically precise in the equations that follow. Assuming that income is received and consumption occurs at the beginning of each

period, then under the K-S definition aggregate life-cycle wealth from the start of period t to the start of period $t+s$ is:

$$LCW_{t,t+s} = \sum_{i=t}^{t+s} (Y_i - C_i) \prod_{k=i}^{t+s-1} (1 + r_k) \quad (1)$$

Y_i is the aggregate after-tax labor income plus government transfers in period i of those still living at the start of period $t+s$, C_i is their aggregate consumption in period i , r_k is the rate of nominal return and $\prod_{k=t+s-1}^{t+s-1} (1 + r_k)$ and $\prod_{k=t+s}^{t+s-1} (1 + r_k)$ are defined respectively to be $1 + r_{t+s-1}$ and 1. The K-S definition of aggregate transfer wealth from t to $t+s$ is:

$$TW_{t,t+s} = \sum_{i=t}^{t+s} (T_{c,i} + T_{s,i}) \prod_{k=i}^{t+s-1} (1 + r_k). \quad (2)$$

where $T_{c,i}$ is net aggregate transfers used for consumption in period i received by those still living at the start of period $t+s$ from those who died during the interval. $T_{s,i}$ is defined identically except that these transfers were saved.

Note that $TW_{t,t+s}$ can also be computed alternatively by using the following formula.

$$TW_{t,t+s} = A_{t+s} - A_{t-1} \prod_{k=t-1}^{t+s-1} (1 + r_k) - LCW_{t,t+s}. \quad (3)$$

where A_{t+s} and A_{t-1} represent the aggregate wealth of the household sector in periods $t+s$ and $t-1$.

Using the same notation as above it is easy to write down the analogous Modigliani expressions.

$$TW_{t,t+s} = \sum_{i=t}^{t+s} T_{s,i} \prod_{k=i}^{t+s-1} (1 + \pi_k). \quad (4)$$

$$\begin{aligned}
LCW_{t,t+s} = & \sum_{i=t}^{t+s} (Y_i - C_i + T_{c_i}) \prod_{k=i}^{t+s-1} (1 + r_k) \\
& + \sum_{i=t}^{t+s} T_{s_i} \left(\prod_{k=i}^{t+s-1} (1 + r_k) - \prod_{k=i}^{t+s-1} (1 + \pi_k) \right). \quad (5)
\end{aligned}$$

By comparing (1) and (5) and (2) and (4) we see that the definitions are the same when both T_{c_i} and r_k minus π_k are zero for all periods. The stock of transfer wealth under the Modigliani definition can be larger or smaller than the stock under the K-S definition depending on the values of T_{c_i} , T_{s_i} , r_k , and π_k over the sample period. An increase in T_{c_i} for one or more periods will increase the stock of transfer wealth under the K-S definition but leaves the Modigliani stock unchanged. The same experiment for T_{s_i} leads to increases in both stocks; the increase in the K-S stock will be larger than the increase in the Modigliani stock when the relevant streams of accumulated nominal return (the $\prod(1 + r_k)$ terms involved) are all larger than the corresponding streams of accumulated inflation. This holds in general when r_k minus π_k is positive over the entire sample period. When the length of the sample period is increased it is impossible to infer anything a priori about the absolute magnitudes of the stocks or their relative magnitudes vis-a-vis the stock of total assets without making steady state assumptions. Note in particular that as s changes even the original sample period values of Y_i , C_i , T_{s_i} , and T_{c_i} change since the relevant population has changed.

Finally I note that there is no consensus in the literature that on theoretical grounds the K-S definition is preferable to the Modigliani definition or vice-versa (Cf., Kessler and Masson (1989)). Of course none of this would make any difference if as an applied matter the results came out largely the same under the two definitions. As Modigliani (1986, 1988) has shown there is little reason to believe this is the case however. Considering this, I will employ both definitions

throughout this paper.

Above I presented in general terms two definitions of aggregate transfer wealth and noted they represented different measures of the total of accumulated net transfers received by those still living from those who died during the sample period. Below I further refine these definitions by noting the conventions that have been adopted vis-a-vis transfers within the family.

Two conventions have been adopted on intrafamily transfers. First, in a very widely accepted practice, accumulated interspousal transfers are excluded from intergenerational transfers.² To be more precise under both the K-S and Modigliani definitions when a spouse dies all accumulated interspousal transfers (including bequests) from the decedent regardless of the date of receipt and regardless of how the transfers were used are considered part of the life-cycle wealth of the remaining spouse. These interspousal transfers are capitalized at the relevant rates of nominal return.

The second convention also widely accepted (Cf., footnote 2) is that consumption expenditures of minor children are classified as consumption expenditures of their parents rather than as transfers. When a parent dies bequests to minor children are of course considered transfers. The only difference between Modigliani and K-S in this regard is that the rates of capitalization of this part of transfer wealth are respectively the appropriate inflation rates and the corresponding rates of nominal return. If at the time of the death of the parent the children are adults, the above analysis holds for those transfers received while the children were minors; transfers received by the children from the decedent after reaching adulthood are treated in the standard fashion under the two definitions.

3. DEFINITIONS OF TRANSFER WEALTH

I turn first to the basic definition of transfer wealth (Hayashi (1986), p. 188, equation 8) that Hayashi used.³ I abstract here from his adjustments for capital gains/losses and for what he terms “underreporting.” In particular this means that the inflation rate for a year is equal to the weighted average across assets of the rates of **nominal** capital gains. I show below that the definition of transfer stock he uses is not one of the standard definitions and that compared to the Modigliani definition it may represent a substantial underestimation or overestimation of the stock of transfer wealth. Further, Hayashi presents no theoretical justification for the definition he uses.

Hayashi defines life-cycle saving to equal disposable income minus consumption. Life-cycle saving for a cohort over a period equals **all** nominal rent in that period plus after-tax labor income plus government transfers minus consumption plus net gifts received that are used for current consumption. Note that net gifts received that are saved are not counted as saving though the nominal rent from these gifts is. In the Hayashi calculation the stock of transfers for a cohort in a year is equal to year-end wealth minus the life-cycle saving for the year minus initial wealth revalued at year-end prices. In equations we can write:

$$W_1(\text{year - end wealth}) = W_0(1 + r_1 + \pi_1) + T_{s_1}(1 + \frac{r_1}{2} + \frac{\pi_1}{2}) + (Y_1 - C_1 + T_{c_1})(1 + \frac{r_1}{2} + \frac{\pi_1}{2}). \quad (6)$$

$$S_1(\text{life - cycle saving}) = W_0(r_1) + T_{s_1}(\frac{r_1}{2}) + (Y_1 - C_1 + T_{c_1})(1 + \frac{r_1}{2}). \quad (7)$$

$$\begin{aligned}
\text{Transfers} &= W_1 - W_0(1 + \pi_1) - S_1 \\
&= T_{s_1}\left(1 + \frac{\pi_1}{2}\right) + (Y_1 - C_1 + T_{c_1})\left(\frac{\pi_1}{2}\right). \quad (8)
\end{aligned}$$

$$K - S \text{ definition of transfers : } (T_{s_1} + T_{c_1})\left(1 + \frac{r_1}{2} + \frac{\pi_1}{2}\right). \quad (9)$$

$$\text{Modigliani definition : } T_{s_1}\left(1 + \frac{\pi_1}{2}\right)$$

where W_0 equals initial wealth, r_1 is nominal rent and π_1 is the weighted average across assets of the rates of nominal capital gains (and is equal to the overall inflation rate).

Hayashi's stock of transfer wealth (equation 8) is the amount of net gifts received that were saved plus the nominal capital gains attributable to these gifts plus the nominal capital gains accruing from $Y_1 - C_1 + T_{c_1}$. This definition is obviously different from the standard definitions listed in equation 9. Comparing the Hayashi definition to the Modigliani definition, we see that to the extent that nominal capital gains (losses) on $Y_1 - C_1 + T_{c_1}$ were high, the Hayashi measure will be greater than (or less than) the Modigliani measure. In the limiting case in which there was no inflation (no nominal capital gains), the two measures are equal. However, in the 1969–74 period inflation was very high (Cf., Hayashi, Table A1, column PCON), and no doubt the Hayashi figures are very substantially biased.

I look now at the definitions of transfer wealth for a cohort that Hayashi, Ando, and Ferris (December 1988) employed.⁴ The change in wealth equals all nominal rent and all nominal capital gains earned in the period plus transfers received that were saved plus transfers received that were consumed plus after-tax labor income

plus government transfers minus consumption. The authors' standard measure of saving equals all nominal rent earned in the period (including nominal rent on transfers received) plus after-tax labor income plus government transfers minus consumption. In equations we have:

$$\begin{aligned}
 W(a, t) - W(a - 1, t - 1) &= W(a - 1, t - 1)(r_1 + \pi_1) \\
 &\quad + (T_{s_1} + T_{c_1})\left(1 + \frac{r_1}{2} + \frac{\pi_1}{2}\right) \\
 &\quad + (Y_1 - C_1)\left(1 + \frac{r_1}{2} + \frac{\pi_1}{2}\right). \quad (10)
 \end{aligned}$$

$$\begin{aligned}
 S^* &= W(a - 1, t - 1)r_1 + (T_{s_1} + T_{c_1})\frac{r_1}{2} \\
 &\quad + (Y_1 - C_1)\left(1 + \frac{r_1}{2}\right). \quad (11)
 \end{aligned}$$

Subtracting equation 11 from equation 10:

$$\Delta W - S^* = (W(a - 1, t - 1) + \frac{T_{s_1} + T_{c_1}}{2} + \frac{Y_1 - C_1}{2})\pi_1 + T_{s_1} + T_{c_1}. \quad (12)$$

where r_1 represents the rate of nominal rent and π_1 is the rate of nominal capital gains on assets. *Hence the change in wealth minus saving equals nominal capital gains earned in the period plus transfers received.*

The alternate measure of saving, S^{**} , is:

$$\begin{aligned}
 S^{**} &= S^* + \text{expected nominal capital gains on land} \\
 &\quad \text{and stocks} - \text{capital losses due to inflation} \quad (13)
 \end{aligned}$$

where expected nominal capital gains on land and stocks equals eight percent of their year-end value and capital losses due to inflation equals the inflation rate

times year-end total wealth. Therefore:

$$\Delta W - S^{**} = \Delta W - S^* - (\text{expected nominal capital gains on land} \\ \text{and stocks} - \text{capital losses due to inflation}) \quad (14)$$

The Modigliani and Kotlikoff–Summers definitions of transfer wealth in this case are:

$$K - S : \quad (T_{s_1} + T_{c_1})\left(1 + \frac{r_1}{2} + \frac{\pi_1}{2}\right). \quad (15a)$$

$$M : \quad T_{s_1}\left(1 + \frac{\pi}{2}\right), \quad (15b)$$

where π is the conventional inflation rate.

Subtracting (15a) from (12) we have:

$$(\Delta W - S^*) - (T_{s_1} + T_{c_1})\left(1 + \frac{r_1}{2} + \frac{\pi_1}{2}\right) = \\ (W(a-1, t-1) + \frac{Y_1 - C_1}{2})\pi_1 - (T_{s_1} + T_{c_1})\frac{r_1}{2}. \quad (16)$$

Hence the authors' standard measure of transfers can be either greater than or less than the K–S measure. In particular to the extent that the rate of nominal gains on assets (π_1) is high, the authors' measure will overestimate the K–S measure. In recent years, though not in 1984, π_1 has been very high. Further to the extent that transfers in a year are small relative to initial wealth at the beginning of the year, the authors' measure again will overestimate the K–S measure.

The same exercise for the authors' second definition of transfers only entails subtracting off from the RHS of (16) expected real capital gains. These in 1984 were positive and hence the authors' second measure of transfers (14) is always

less than their first (12). As in the first case the second measure may be less than or greater than the Kotlikoff–Summers measure and the π_1 analysis above also holds here. Also as inflation increases the second measure again tends to overestimate the Kotlikoff–Summers measure.

Summing up, we see that the two definitions proposed by the authors differ from those conventionally used. And Hayashi *et al.* do not seriously argue why their definitions are to be preferred. In addition I note that their definitions are not intergenerational as usually defined since interspousal transfers are not subtracted off (Cf., Section 2).

Finally since Dekle (1989) does not precisely define life cycle saving, it is impossible to evaluate his definition of transfers, though it is clearly modeled on the Kotlikoff–Summers definition.

4. ESTIMATION OF ACCUMULATED WEALTH

I first examine Hayashi's (1986) estimation of wealth. In order to calculate the flow of transfers Hayashi estimated the initial and final per-household wealth of families that were nuclear in 1974. I show below that his intergenerational transfer figure is extremely sensitive to error in his wealth calculations. I then catalogue the reasons why it is likely that these errors are of substantial magnitude.

In Table 1 I have attempted to duplicate Hayashi's calculations for 1974 (figures for extended families of the 25 to 39 age bracket are not presented because Hayashi combined this category with older extended families). Hayashi does not detail his calculation method. We do know though that for each age group and for each type of family he estimated an imputed rent–food expenditure ratio. He

Table 1
Age Profile of Income, Imputed Rent, and
Food Expenditure by Family Type,
Worker Households, 1974

	Age in 1969				
	25-29	30-34	35-39	40-44	45-49
Panel A, Nuclear					
1.Number of households	2.2	2.1	1.8	1.5	.9
2.Total assets	2,480,000	3,048,000	3,770,000	4,732,000	5,237,000
3.Net financial assets	689,247	808,879	1,193,305	1,684,955	2,065,174
4.Tangible assets	1,790,753	2,239,121	2,576,695	3,047,045	3,171,826
5.Imputed rent	89,538	111,956	128,835	152,352	158,591
6.Food expenditure	532,860	588,600	624,600	622,344	567,504
7.Imp. rent/Food exp.	.168	.190	.206	.245	.279
Panel B, Extended					
1.Number of households	—	—	—	.4	.3
2.Total assets	—	—	—	6,750,000	7,018,000
3.Net financial assets	—	—	—	2,117,159	2,553,541
4.Tangible assets	—	—	—	4,632,841	4,464,459
5.Imputed rent	—	—	—	231,642	223,223
6.Food expenditure	—	—	—	667,068	632,988
7.Imp.rent/Food exp.	—	—	—	.347	.353

Number of households is in millions. Other figures are in yen.

Sources: Hayashi's Table 9; 1974 **National Survey**, v. 1, Table 21 and v. 7, Table 4.

then multiplied this ratio times yearly food expenditures for each group to get each group's yearly imputed rent. Next he assumed that nominal returns on tangible assets in 1974 was five percent.⁵ This permitted him to calculate the total of tangible assets for each group. Net financial assets are given directly in the **National Survey (National Survey of Family Income and Expenditure)** tables. The total of tangible and net financial assets is total assets. Since he did not list his imputed rent–food expenditure ratios, I generated the implicit figures by working backward from total assets.

As an illustration of how sensitive Hayashi's transfer calculation is to error in the wealth figures, let us suppose that he overestimated by five percent (.05 versus the true figure of .04762) the nominal return on real assets in 1974. Or equivalently we could suppose that the true imputed rent–food expenditure ratios in 1974 were five percent higher than those actually used. Then tangible assets in 1974 will of course be five percent higher, and total assets turn out to be 3.24 percent higher. Hence 1974 wealth of the groups he looked at would increase from 78 to 80.53 trillion yen and transfers would increase from 8 to 10.53 trillion yen, an increase of 32 percent.⁶

Are errors of the magnitude described above likely? The easiest way to look at this is to consider in turn the three distinct kinds of wealth computations he undertakes: one, the wealth in 1974 of nuclear families in the targeted age group, two, the initial wealth of families that were nuclear in 1969 and remained nuclear through 1974, and, three, the initial wealth of families that were extended in 1969 and became nuclear by 1974.

For the category 1 calculation likely sources of error are the nominal rate of return on tangible assets in 1974 and the 1974 imputed rent–food expenditure

ratios. Hayashi's estimate of the nominal return on real assets may or may not be correct: no such information is given in the **National Surveys**, and I did not try to corroborate Hayashi's figure. For the imputed rent–food expenditure ratios, the key problem is that ratios are not broken down by both age of the household and family type in the **Surveys**. However they are broken down separately by each category. Since there are ten age categories and three family type categories (nuclear, extended and other), this in effect gives us thirty unknowns and thirteen equations. The underidentification problem cannot be resolved. Indeed even if all cell values were zero except for nuclear and extended families of the 35–49 age bracket, this would still leave us with six unknowns and five equations. Therefore we can be confident that there is some imprecision in Hayashi's estimates of these ratios, though it is impossible to quantify how much.

For the category 2 calculation, in addition to the sources of error indicated above, there is also the problem that Hayashi assumes that the per–household wealth in 1969 of all nuclear families of the same age bracket is the same in spite of the fact that these families can be divided into two distinct groups: those who remained nuclear through 1974 and those who formed extended families by 1974 (Cf., see my related comments in Section 5). It is likely that the economic characteristics of these two groups are somewhat different, and hence Hayashi has certainly introduced a degree of bias in his calculation. Recall that even if this difference is not large in percentage terms it may profoundly affect the size of the stock of transfer wealth.⁷

The calculations for the category 3 group – those younger generation households that were extended in 1969 and became nuclear by 1974 – are the most problematic. Indeed I think it is fair to say that they are entirely unreliable.

Hayashi's procedure was to assume that the 1969 wealth held by the elderly in extended families headed by those in the 25 to 29 age bracket was equal to the difference between the wealth held by these extended families and the wealth held by 25–29 year-old nuclear families. Wealth of the elderly in older extended families was computed by discounting the wealth of the dependent elderly above by a productivity factor of five percent per one-year age difference. Finally the wealth of the dependent elderly in each age bracket was subtracted from the wealth of the corresponding extended families to arrive at the wealth of the younger generations in these families.

I think it is reasonable to believe that the wealth held by the young in the 25 to 29 age bracket in 1969 was quite uniform across family status (i.e., nuclear–nuclear, nuclear–extended, extended–extended, and extended–nuclear). Hence Hayashi's initial calculation was acceptable subject to the reservations detailed in my category 1 discussion above. However his imputation of the wealth of the older elderly is entirely an ad-hoc procedure. It depends on the assumption of drastic stationary conditions which simply were not satisfied in the post-war period in Japan. For a lucid but rather too abbreviated analysis of this issue see Ando (1985, Chapter V). In the absence of any justification of this procedure, it is impossible to have any confidence in his estimates of the wealth of the older elderly. Further even if these estimates were not open to question, Hayashi does not distinguish the behavior of the 35 to 49 younger generation who switched from extended to nuclear over the period from the behavior of those in this age category who remained extended over the period. Unlike the 25 to 29 case, we can expect substantial differentiation in 1969 wealth holdings for these groups. In brief I see few redeeming features in his treatment of category 3 wealth estimation.

Next I analyze the accumulated wealth calculations undertaken by Hayashi, Ando, and Ferris (December 1988). I restrict my comments to their cohort effect computations; the sum of the cohort effect and the cross-sectional growth rate equals the longitudinal growth rate. I start with the young nuclear group. The cohort effect here is defined as:

$$W(a - 1, t) - W(a - 1, t - 1). \quad (17)$$

Note that the first term represents the log of the average per-household wealth in t of those young nuclear households in the five-year age bracket $a-1$ in t and the second term is the log of the average per-household wealth in $t-1$ of those households that become young nuclear households in t and that are in the five-year age bracket $a-1$ in $t-1$.

The authors make a series of very strong assumptions that are not very precisely stated in order to attach a number to the cohort effect. These assumptions appear to be the following:

1. The economy is in a steady state.
2. Therefore the cohort effect for all young equals the productivity rate.
3. It might well be the case that for certain subsets of the young the cohort effect does not equal the productivity rate.
4. However the incomes of extended and nuclear young in 1984 were the same so we can conclude the cohort effects of the two groups were the same.
5. Hence we can use the productivity rate as the cohort effect for nuclear young.

Even accepting statements 1 to 4 above, 5 would follow only if the young in these age brackets were comprised solely of young nuclear and young extended. In fact,

a large number of households were dropped from the original sample. The authors are then implicitly assuming that the cohort effect of the omitted young equals the cohort effect of the other two groups.

In addition to this point, I note that:

1. The Japanese economy is not in a steady state now (and was not in 1984) though many economists seem to believe that Japan is converging to a steady state. In a transition period the behavior of economic agents is not going to be consistent with steady state behavior because their past (pre-transition state) behavior was not consistent with steady state behavior. One can imagine for instance the effects a halving of the real growth rate over the past fifteen years have had on saving and consumption of the middle aged in Japan.

2. It seems a heroic assumption to posit that the cohort effects of the nuclear young and the extended young in 1984 were the same simply because their income in that year was the same.

3. Even if one is willing to ignore the above reservations, there still remains the problem of attaching a number to the cohort effect. Presumably even if an economy can be approximated by a steady state, as a practical matter there is going to be some variation in productivity rates. To the extent that the numbers jump around a lot, picking an appropriate productivity rate would seem to be difficult. The authors are careful to acknowledge this point.

I turn now to the cohort effect calculation done by the authors for the elderly in extended families. Nearly the identical procedure was used to compute the cohort effect of the independent elderly; therefore the force of my comments below applies to that case as well.

The authors use an approximation to the following equation to calculate the

cohort effect:

$$W(a-1, t) - W(a-1, t-1) = \frac{-(\log PI(a, t) - \log PI(a-1, t))}{pa(a) - pa(a-1)} \quad (18)$$

where in this case $W(a-1, t)$ is the log of the average household wealth in t held by the elderly in extended families whose children in t are in the five-year age bracket $a-1$, $W(a-1, t-1)$ is the log of the average household wealth in $t-1$ of the elderly who are in extended families in t and whose children in t are in age bracket a , $\log PI(a, t)$ is the log of the permanent income of the elderly in the second group, and $\log PI(a-1, t)$ is the log of the permanent income of the elderly in the first group. As $pa(a) - pa(a-1)$ (the difference in the mean ages of the two groups) approaches 1 we have:

$$\frac{W(a-1, t)}{PI(a-1, t)} = \frac{W(a-1, t-1)}{PI(a-1, t-1)} \quad (19)$$

where here W and PI are not in logs. The LHS of (19) is the wealth-PI ratio in t of the first group of elderly, and the RHS is the wealth-PI ratio in $t-1$ of the second group of elderly.

This “elasticity of wealth with respect to permanent income” of course need not be one for any a let alone all a , as the authors observe in footnote 11. Further no empirical evidence is presented to substantiate their assumption. Therefore their analysis of the cohort effect for the elderly living in extended families should simply be dismissed as unsupported.

Even if one accepted (19), one problem remains – the unreliability of their estimates of permanent income which they define to be the sum of pension and business income. The authors are aware of this issue and in effect concede that these estimates are merely suggestive.⁸

I conclude that the methodology used to compute cohort effects is intrinsically flawed. Hence the estimates of the cohort effects are merely speculative. This also means of course that the stock-based longitudinal growth rate estimates are entirely unreliable.

Dekle's (1989) estimation of the wealth accumulated over the 1968–83 period by four cohorts is marred by several defects. First in his estimation of final wealth, he assigns to the target generations the entire wealth of the households that they head. This means, most notably, that the wealth of the elderly living in extended families headed by the target generations is improperly allotted to the younger generation. Turning to his estimates of initial wealth, he takes the initial wealth of the target generations to be the 1968 wealth of all worker households in which the heads are 25–29, 30–34, 35–39, and 40–44. There are two glaring errors here. First households are excluded that should be included and vice-versa. To take a concrete example, for his cohort 1 (households aged 25–29 in 1968 and 40–44 in 1983), he excludes the large number of 25–29 year-olds who were unmarried, living with their parents in 1968, and not the heads of their households but who in 1983 were living in nuclear households (for a detailed discussion of the changes over time in family composition of selected cohorts see Campbell (1991), Chapter 3, Section 2). Second, echoing a comment above, even for those households that were properly included, he assigns the entire household wealth to the target generations, setting equal to zero the wealth holdings of other adult generations in the household.

Abstracting from the above, it is apparent that Dekle's calculation of the value of residential land held by homeownership households headed by the target generations is in error. Dekle claims that for these households the value of resi-

dential land is the same across cohorts and is equal to the average value of newly acquired lots. The available evidence (see Campbell (1991), Appendix 1, fn. 13) indicates that for homeownership two or more person all households (i.e., worker and nonworker households comprised of extended, nuclear, and other families), the value of owned residential land increases with the age of the cohort, that these differentials are significant, and that they are driven almost exclusively by differences in the amounts of residential land owned by the cohorts. These conclusions most likely apply to worker households, the sample Dekle investigated. Hence Dekle's contention that for these households, the value of owned residential land is the same across cohorts is almost certainly wrong, a point that he concedes. Particularly given this, it seems one would be compelled to explain where the average value of newly acquired lots stands in the distribution across cohorts of the value of residential land held by homeownership households headed by the target generations. However Dekle is silent on this subject. It strongly appears then that Dekle's calculation of the value of residential land is gravely flawed. This suggests that his overall wealth calculations are suspect given the importance of residential land in household portfolios.

In summary then his failures in his estimation of wealth to distinguish cohorts from synthetic cohorts and the target generations from their households with the difficulties he encountered in computing the value of residential land held by homeownership households in all likelihood means that his estimates suffer from major biases.

5. ESTIMATION OF LIFE CYCLE SAVING

I discuss first the derivation of the unadjusted life cycle saving figures in Hayashi (1986). Hayashi makes three assumptions about saving behavior:

1. Everyone of the same age bracket in nuclear (as defined by Hayashi) households in 1969 saved the same amount in 1969. And all of the young of that age bracket in extended households in 1969 saved the same amount.

2. The per-household saving of the two groups in (1) was the same in 1969.

3. Further the per-household saving of all those in the indicated age bracket remained the same in real terms over the five year period.

The three assumptions taken together imply that all the young of the same age bracket – whether in extended families or not in 1969 and regardless of whether they changed their family status over the period – saved the same amount per household in real terms over the five year period.

Obviously the easiest way to check if this implication is correct is to compare the per-household saving of nuclear families in 1969 and 1974 by age bracket. These figures in 1974 yen are listed in Table 2 (computed directly from Hayashi's Table 9). From the table it is clear that Hayashi's assumptions, taken at face value, are not supported by the evidence. This does not mean of course that his estimate of life-cycle saving is necessarily wrong. It could be the case, for instance, that all the young of a certain age bracket saved the same amount in any year, but that this amount varied across year. If so, the 1969 saving figures could represent the five-year average, and hence his estimate of life-cycle saving would be unbiased.

Table 2

Per-household Saving in 1969 and 1974 by Age Bracket,
Nuclear Families, Worker Households

	Age in 1969		
	35-39	40-44	45-49
1969 Savings	307	347	357
1974 Savings	577	596	663
% Change	88	72	86

More realistically the actual saving behavior Hayashi was interested in probably cannot be characterized so fortuitously. Over the period the young of a certain age bracket under Hayashi's categorization can be divided into four groups:

- a) Those who were in nuclear families in 1969 and stayed in nuclear families over the five years.
- b) Those who were in nuclear families in 1969 and switched to extended by 1974.
- c) Those who were in extended families in 1969 and remained in extended families through 1974.
- d) Those who were in extended families in 1969 and formed nuclear families by 1974.

It is likely that the amount saved per household varied both across the four groups (a, b, c, and d) and across time. Hence Hayashi should have tried to isolate the saving behavior of groups a and d across time. This would have been no simple matter. In particular a key point would be trying to estimate the amount saved per household by those in group d in 1969. This is especially difficult since in the extended family category in 1969 there are not only type c and type d nuclear families but also as well the parents of both groups. And the **National Survey** data, in tabulated form at least, does not provide a comprehensive breakdown of income and consumption by family member.

In this situation it seems clear that the way to proceed would be to posit a baseline figure for the type d per-household saving amount in 1969 (and hence posit an implicit saving amount for their parents), and then run simulations around the baseline. This is an essential step since the saving behavior of the dependent elderly is so little understood. If that were done, reasonable saving profiles for

group d over the entire period could be established. And a related exercise could be done for group a.

With the above in mind it is impossible at this stage to state definitively the amount of bias that Hayashi has introduced into his calculations by his oversimplification of this problem. It seems logical to believe however that the amount of bias introduced by assumption 3 swamps the amount of bias attributable to his first two assumptions. In short his assumption of constant real saving over a period in which both saving rates and real income were increasing probably means that on the whole he significantly underestimated the life-cycle savings of the groups under investigation.

Turning to Hayashi, Ando, and Ferris (December 1988), if one abstracts from definitional problems (Cf., Section 3), their estimation of life cycle saving for nuclear and extended households seems basically robust. An important reason for this is that since the time horizon for their study was only one year, they had largely complete longitudinal saving data (but not longitudinal wealth data, see Section 4). In contrast, the Hayashi (1986) and Dekle (1989) studies with much longer time horizons (five and fifteen years respectively) did not have any longitudinal data available and hence had to estimate life cycle saving indirectly.

Dekle's (1989) estimation of the life cycle savings over the 1968-83 period of the target generations which head all worker households in 1983 suffers from largely the same drawbacks as his estimation of their accumulated wealth. Hence here I only note that he measured the life cycle savings of the synthetic cohorts instead of estimating the savings of the actual cohorts, and he attributed the entire life cycle saving of the households he looked at to the target generations, in effect assuming that the life cycle saving of other adult generations in these households

(largely the parents of the target generations) was zero. In regard to the second point, the shape of the age-wealth profile of the elderly in extended families who are not the heads of their households and the extent of their life cycle saving are issues of much controversy that remain unresolved in the literature (Cf., Campbell (1991), Chapter 2, Section 2.5).

NOTES

1. However Horioka (1990), an excellent survey on Japanese saving, does examine briefly this material.

2. See, for instance, Modigliani (1986, 1988), K-S (1981, 1986) and Dekle (1989) and the references they cite.

3. $T_{c,i}$ and $T_{s,i}$ are as defined in Section 2: $T_{c,i}$ represents net *non-capitalized* transfers in current prices that were received in year i and were used for consumption; $T_{s,i}$ is defined identically except that these transfers were saved.

4. Actually I compare the change in wealth with the two measures of saving they used. The authors compared the change in logs of wealth with their two measures of saving divided by end of period wealth.

5. An examination of Hayashi's Table 5B and Table 6 of volume 1 of the **1974 National Survey** verifies this.

6. A similar exercise was not done for initial wealth held by families that were nuclear in 1974 because both the nominal rate of return on tangible assets in 1969 and the deflator used by Hayashi could not with absolute certainty be verified. However it is obvious that since the initial stock is less than the final stock, the effect described in the text will be less dramatic. In addition to the extent that tangible assets in 1969 comprised a smaller percentage of total assets than in 1974, the effect again will be reduced. Nevertheless even in this case it is likely that the sensitivity of the transfer stock to errors in initial wealth is very substantial.

7. See footnote 6.

8. Hayashi et al., page 469 and pages 488-90; in particular note the last few lines of page 488.

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