Compensatory Inter Vivos Gifts

Hochguertel, S.; Ohlsson, H.

2007

TI 2007-074/3
Tinbergen Institute Discussion Paper

Compensatory Inter Vivos Gifts

Stefan Hochguertel[1]
Henry Ohlsson[2]

[1] *VU University Amsterdam, and Tinbergen Institute;*
[2] *Uppsala University.*

Tinbergen Institute
The Tinbergen Institute is the institute for economic research of the Erasmus Universiteit Rotterdam, Universiteit van Amsterdam, and Vrije Universiteit Amsterdam.

Tinbergen Institute Amsterdam
Roetersstraat 31
1018 WB Amsterdam
The Netherlands
Tel.: +31(0)20 551 3500
Fax: +31(0)20 551 3555

Tinbergen Institute Rotterdam
Burg. Oudlaan 50
3062 PA Rotterdam
The Netherlands
Tel.: +31(0)10 408 8900
Fax: +31(0)10 408 9031

Compensatory Inter Vivos Gifts

Stefan Hochguertel
VU University Amsterdam
Tinbergen Institute

and

Henry Ohlsson
Uppsala University

September 2007

Compensatory *inter vivos* gifts*

Stefan Hochguertel Henry Ohlsson

31 August 2007

Abstract

Parents' transfer motives are important for understanding, e.g., macroeconomics, income (re)distribution, savings, and public finance. Using data from six biennial waves of the Health and Retirement Study 1992–2002, we estimate grouped tobit-type latent variable models with multi-level error components. First, we find that *inter vivos* transfers from parents to children are gifts, and not temporary help to overcome liquidity constraints. Second, *inter vivos* gifts are compensatory in the sense that life-time poorer children will receive higher transfers than their life-time richer siblings. Third, *inter vivos* gifts do not, however, make up the entire difference in life-time incomes.

Keywords: *inter vivos* gifts, compensatory transfers, liquidity constraints, altruism, exchange

EconLit subject descriptors: D100, D640, D910

Correspondence:
Stefan Hochguertel, Department of Economics, VU University Amsterdam, De Boelelaan 1105, NL–1081 HV Amsterdam, The Netherlands, email <shochguertel@feweb.vu.nl>,
Henry Ohlsson, Department of Economics, Uppsala University, Box 513, SE–751 20 Uppsala, Sweden, email <henry.ohlsson@nek.uu.se>.

*We thank the Health and Retirement Study for providing the data. The many previous versions of the paper have been presented numerous times. Helpful comments from Dan Anderberg, Sören Blomquist, Martin Browning, Jim Davies, Lena Edlund, Tom Juster, André Masson, John Schmitt, Donald Storrie, seminar and conference participants, and from two anonymous referees and the editor are gratefully acknowledged. Stefan Hochguertel's participation was funded by the TMR Network on Savings and Pensions, while being at Uppsala University. Henry Ohlsson thanks the Bank of Sweden Tercentenary Foundation for support through grant 94–0094:01–03. Some of the work was done when Ohlsson enjoyed the hospitality of ERMES UMR CNRS 7017, Université Panthéon-Assas, Paris II.

1 Introduction

Parents intentionally, but also unintentionally, make transfers to their children in different ways. There are biological transfers of natural talents and abilities. Purchases of education and other human capital investments, for example providing access to social networks, are other ways of making transfers. Parents can also transfer financial and tangible property by bequests and *inter vivos* gifts.

The objective of this paper is to find out what determines parents' *inter vivos* transfers to their children. In the data, many parents do not transfer at all, and many children do not receive at all even when their siblings do. The observed pattern of transfers is related to characteristics of both the child and the parent. An important question is if parental transfers are compensatory, i.e., if parents transfer more to a child with less resources of her own than her brothers and sisters.

Understanding the determinants of parental property transfers is crucial for a wide range of economic issues. Some of these are the possible effects of fiscal policy, the equality of opportunity, the determinants of savings, and the optimal design of tax systems. In macroeconomics, for example, the Ricardian equivalence predictions rest on the assumption of dynastic, altruistic, behavior. Second, parental property transfers are also important when discussing the distribution of income and wealth. The extent to which wealth is carried over from one generation to the next affects how equal opportunities really are. Parental transfers may also decrease the efficiency of public redistribution by counteracting the intended effects of public transfers.

A third field for which parental property transfers are important is savings. Strong transfer motives will affect savings behavior. This concerns saved amounts but also the timing of savings over the life cycle. Finally, there are also public finance aspects of parental property transfers. Depending on the determinants of transfer behavior, taxes on *inter vivos* gifts, bequests, and inheritances may or may not create excess burdens.

The theoretical literature on parental transfers is characterized by different assumptions concerning parents' motives for making transfers. It is an empirical question to determine which of the motives are most important.

Empirical studies of intergenerational transfers find that *inter vivos* gifts tend to be compensatory. *Post mortem* bequests, on the other hand, are usually equally divided.[1] This difference between bequest and gift behavior is somewhat of a puzzle. Simple versions of altruistic models of intergenerational transfers, for example, only predict that total transfers will be compensatory. It is an open question whether both bequests and *inter vivos* gifts will be compensatory in the simple models.

In this paper we study data from the Health and Retirement Study (HRS). The HRS has been designed and conducted by the University of Michigan's Survey

[1] Pestieau (2003), Laferrère and Wolff (2006), and Arrondel and Masson (2006) include surveys of the empirical literature.

Research Center. It is a panel data set, focusing on health and retirement related issues of the U.S. pre-retirement population (cohorts born between 1931 and 1941). It was launched in 1992 and is repeated biennially. We use data from six waves 1992–2002.

The HRS and the related Asset and Health Dynamics among the Oldest Old (AHEAD) have been used in several previous empirical studies. McGarry and Schoeni (1995) and McGarry (1999), using the first wave of the HRS, find that gifts are compensatory in the sense that higher income of a child makes a gift less likely. McGarry and Schoeni (1995) and McGarry (2000), the latter using the first two waves of the HRS, find that gift amounts are compensatory. Dunn and Phillips (1997), McGarry and Schoeni (1997), and McGarry (1999), using the first wave of AHEAD, also find that gifts are compensatory in the sense that higher income of a child makes a gift less likely.

We want to emphasize three features of our analysis as compared to previous studies: *First*, it is essential to use data that capture variations in several dimensions. The predictions of the transfer theories are predictions of the within-family-variation in transfer behavior, not the between-family-variation. Moreover, theory distinguishes between transfers to compensate for differences in permanent economic resources (permanent income), *inter vivos* gifts, and transfers to ease temporary needs (temporary liquidity constraints). Data, therefore, need to have a family dimension, a sibling dimension, and a time dimension.

The HRS provide such data. Data (i) are on the recipient level (children), rather than data on the donor level (parents), (ii) are for all siblings in each family, and (iii) from several waves, and not only a single cross section. We are—to our knowledge—the first authors to estimate transfer models exploiting the multidimensional panel structure of the data. This gives us the possibility of being able to disentangle the hypothesis of compensatory *inter vivos* gifts from temporary help to children in order to ease liquidity constraints.[2] This is the economic contribution of our paper.

Second, theories about compensatory transfers are more about transfer amounts than transfer probabilities. Most previous papers have focused on transfer probabilities, while we focus on transfer amounts. *Third*, there are many observations where transfers are zero, no transfer is made from parent to child. It is, therefore, crucial to take into account that data are censored.

The main empirical innovation is that we use a limited dependent variable model with a nested grouped error structure. We specify the unobservables to fall into three categories: an idiosyncratic error term, varying over families, children, and time; a child-level specific error component, varying over children within a family and constant over time; a family-specific error component, varying over families and constant for all children in the family (and constant over time). This makes our model a multi-level error components model. The econometrics of lin-

[2] Access to several waves of panel data also allows us to estimate random effects income models for the children. We use these estimations to compute the permanent income of each child.

ear multi-level components with random effects is detailed in the contributions by Antweiler (2001) and Baltagi et al. (2001). Applications include Cardoso (2000).

We include in our specification averages of variables over children, which is inspired by the well-known Mundlak (1978) approach. This way, we hope to capture the within-variation at the family level. Mundlak has shown the equivalence of random effects and fixed effects in a linear model when the specification includes time-averages of regressors in a standard (one-way) panel. With this in mind, we can interpret our estimates as emulating fixed family effects.

Our model is a nonlinear extension of this structure and estimated by Maximum Likelihood. Applications of nonlinear models and estimation issues are discussed in some detail in Rabe-Hesketh et al. (2005). We allow the idiosyncratic error to be heteroskedastic. This has not only implications for standard errors but also repercussions for coefficient estimates in the latent variable model due to the nonlinearity of the model. We found that available software to estimate this model fails to produce estimates in any reasonable time span. We, therefore, rely on our own software which employs a simulated Maximum Likelihood estimator based on quasi-random Monte Carlo methods. Our paper is, therefore, one of the very few that presents estimates of a generalized tobit model with multi-level effects on a large panel data set.

The sensitivity analysis includes not only changes in specification and samples, but also trade off our parametric approach with the semiparametric one proposed by Honoré (1992) on a one-way model.

Our main findings are as follows:

1. We find that intergenerational transfers flowing from parents to children are *inter vivos* gifts, not temporary help to overcome liquidity constraints.[3] We identify this by using a regressor that proxies for the potential importance of liquidity constraints.

2. Gift amounts from parents are compensatory in the sense that life-time poorer children will receive higher transfers than their life-time richer siblings.

3. We do not find, however, that gifts make up the entire difference in life-time resources. One dollar less of life-time income compared to a sibling triggers about 2 cents of expected (unconstrained) *inter vivos* gifts (levels model) or an increase by 0.003 percent (semielasticity in a nonlinearly transformed model).

The paper is structured as follows: We present and discuss in Section 2 testable predictions from theoretical models of intergenerational transfers. Section 3 describes the data and provides some descriptive results. The estimation results are reported in Section 4. We also present some sensitivity analyses in this section. Section 5 concludes.

[3] Despite the questionnaire explicitly allowing for both types.

2 Theoretical framework

Gifts and temporary help are voluntary intergenerational transfers. Different motives for voluntary intergenerational transfers have been proposed in the theoretical literature. We will discuss altruistic, exchange, egoistic, and risk-sharing motives.[4]

Altruism. This is the Becker (1974) and Barro (1974) framework. Consider an altruistic parent who has several children. The parent cares about her own lifetime consumption and the children's lifetime consumption possibilities. The parent will try to equalize the consumption possibilities of the children.[5] Higher lifetime income for a child relative to the siblings reduces the lifetime transfers received. Higher lifetime resources for the parent leads to more transfers to all children. Similarly, higher lifetime income for a sibling also increases the lifetime transfer to a child.

What matters are the total resources of the other people in the family, not the distribution within the family. A child will only get more if family lifetime resources increase. The altruistic model generates an adding-up condition. If the parent (or a sibling) gains a dollar in permanent income while a child loses the same amount in permanent income, a one dollar gift will restore the initial optimal allocation of resources.[6]

Exchange. Bernheim et al. (1985) and Cox (1987) present versions of the exchange model. In this model, the parent values the attention of the children more than services otherwise purchased in anonymous markets. Suppose a parent obtains such attention in proportion to the amount she gives to each child. Higher income of the parent will tend to result in more gifts (more attention purchased from the children), but also more own consumption.

Since the opportunity cost of each child's time is increasing in his income, the implicit price the parent will have to pay for attention will tend to be increasing in the child's income. The probability that the parent makes any purchases at all will, therefore, be decreasing in child income.

Given that the parent makes purchases (transfers), the impact of the children's incomes on total spending is, however, ambiguous. Suppose that the price elasticity is low because there are no close substitutes to the services of a particular child. The amount will then be increasing in the child's income. If, on the other hand, the price elasticity is high, the amount decreases in the child's income.

Transactions costs—in the form of travel or travel time costs—suggest that children living closer to their parents need relatively lower compensation. Parent's poor health may mean higher demand for attention or higher compensation payments.

[4] See also the surveys by Laitner (1997), Masson and Pestieau (1997), Laferrère and Wolff (2006), and Arrondel and Masson (2006).

[5] The stronger the parent's altruism the more the parent wants to equalize.

[6] Cox (1987) is the first to calculate this derivative condition. Altonji et al. (1997) and Laitner and Ohlsson (2001) test the condition. McGarry (2000) stresses that the condition does not necessarily apply to current income.

Egoism. In another frequently used model (e.g. Blinder, 1974; Andreoni, 1989; Hurd, 1989), a parent derives utility from the amount it gives (joy of giving or warm glow) but not from the utility the child actually derives from the resulting transfer. This is sometimes called the egoistic model.

Compared to the altruistic model, there are no differences of the effects of the parent's income. The models differ in the implications of children's incomes. Transfer behavior according to the egoistic model is not affected by the incomes of the children.

Risk-sharing. Transfers within families are also discussed in the literature on risk sharing within families. Intra-family transfers may be the result of informal insurance arrangements within the family in situations when insurance markets are missing or when insurance markets are affected by adverse selection and moral hazard. Usually these transfers compensate for temporary rather than permanent income losses. Kimball (1988) and Coate and Ravallion (1993) discuss risk sharing in the absence of insurance markets. Kotlikoff and Spivak (1981) study how families provide substitutes to annuities from insurance markets.

Suppose households cannot insure because of imperfect markets for annuities. And suppose that there is no risk-sharing within the family. Instead households have to save for a long retirement. If they die young, their unused resources become accidental bequests. If they live a long time, they may die with little or no estate. The accidental model of Davies (1981) is a version of the life–cycle model. Friedman and Warshawsky (1990) report rather ambivalent support for the model.

Parents can make transfers during their lifetime—*inter vivos* transfers. An alternative is to bequeath, thus making the transfer *post mortem*. Why *inter vivos* transfers and not bequests?

The existence of liquidity constraints may make parents choose *inter vivos* transfers rather than bequests (Bernheim et al., 1985). It is difficult for children to borrow against future inheritances because of imperfect markets and asymmetric information. Parents may, on the other hand, choose to postpone transfers as long as possible for strategic reasons (Cremer and Pestieau, 1996). The motivation for this is to provide incentives to study and work for the children. The existence and design of gift, estate, and inheritance taxes may affect the choice between gifts and bequests by creating incentives for tax avoidance, see Nordblom and Ohlsson (2006).

Simple versions of altruistic models of intergenerational transfers predict that total transfers will be compensatory. It is an open question whether both bequests and *inter vivos* gifts will be compensatory in these models. The empirical findings are that *inter vivos* gifts tend to be compensatory while bequests usually are equally divided among heirs. Can this be given a theoretical basis?

Lundholm and Ohlsson (2000) assume that gifts are private information while bequests are public information and that parents care about their reputation after death. Given these assumptions altruistic parents will choose compensatory gifts and equal bequests. Bernheim and Severinov (2003) also discuss theoretical models that generate results consistent with the empirical evidence.

3 Data and descriptives

3.1 Data

We use data from the Health and Retirement Study (HRS), which follows the 1992 pre-retirement cohort (born during 1931–1941) through time into retirement and beyond. The sampled population covers U.S. residents excluding institutionalized persons households. The core sample aims to be representative, although there is deliberate oversampling of Blacks, Hispanics, and Florida residents. Not every household has children, and we shall focus on the ones that do. We use the first six waves (1992–2002) of final release biennial surveys.

Within a household there are two main respondent types: the household financial respondent and the family respondent, the latter usually being the female member in a couple. Apart from family structure and transfers, the questionnaire covers the demographic background, health status, housing, employment, last job and job history, retirement plans, assets and liabilities, income, and information on children.

The family respondent provides information on children and transfers. Child demographics extend to sex, age, education etc. of all children of the family. *Inter vivos* money transfers relate to flows from parents to their children during the preceding years.

We have expended large efforts at checking, and where necessary, imputing information from adjacent years for child level background variables. Since household composition may change over time, both at the parental and the child level, we make sure that we keep constant the source of information on each child over time. More information on how we tailored the data to our needs is available in Appendix A.

We start with all children observed in the families of all HRS households in any of the six waves 1992–2002. Children are defined as being children of the family respondent and/or the spouse, excluding grandchildren, spouses of children, or other unrelated household members. We exclude additional mentions of children that were mentioned only after the first waves, unless they enter the household via family restructuring (or birth). For instance, in some cases new children are introduced into an existing household simply by a new spouse with children of his or her own entering.

There are 108,635 observations in total. The data structure allows identification of child-level variables through more than one respondent in the case that an original 1992-household split and both split-off households report on a common child. These duplicate interviews is what we call 'family stories' and we identify the person from whom a particular 'family story' originates in a given sample wave.[7] We then select one 'family story' for each child, which ensures that the reporting person stays constant over time. This leaves 102,827 observations. We measure as parent-level variables the characteristics of the reporting person (or the

[7] See Appendix A for details.

Table 1: Number of families and children

wave	1992	1994	1996	1998	2000	2002	any
families	4,499	4,456	4,721	4,828	4,886	4,843	5,210
children	15,795	15,958	17,161	17,760	18,089	18,064	20,033

household that this person lives in when variables are measured at the household level). Table 1 provides a count of families with children in the data where interview information was available.

The HRS data set has a number of important strengths.[8] There is relatively little attrition at the parent level. At the child level there is close to no attrition and, therefore, no selection because parents provide the information on all children. Couples in the sampled generation display relatively high marital stability, and the number of children in each family is fixed in many cases. In addition, parents will have accumulated relatively much wealth, and are at a point in their life cycle where they need to decide how to spend it.

For the present study, the information on *inter vivos* transfers is of crucial importance. To illustrate, in the first wave, respondents are asked the following question (verbatim quote):

> (Not counting any shared housing or shared food,) Have you [and your (husband/partner)] given (your child/any of your children) financial assistance totaling $500 or more in the past 12 months?
>
> [DEFINITION: By financial assistance we mean giving money, helping pay bills, or covering specific types of costs such as those for medical care or insurance, schooling, down payment for a home, rent, etc. The financial assistance can be considered support, a gift or a loan.]

If the answer is affirmative, the respondent is then asked to give the total amounts transferred, per child. In those cases where parents are unable to give dollar amounts, static and unfolding bracketing techniques have been employed over the years to elicit the magnitudes.

There are two changes in wording in these questions over the years. In the second wave, the reporting threshold is lowered from $500 to $100, but restored to $500 again in the third wave. In regression analyses, survey wave dummies will

[8] The Panel Study of Income Dynamics (PSID) is often used in the *inter vivos* transfer literature, for example by Altonji et al. (1992, 1997). PSID, however, only has a single cross section of transfer information, making it impossible to distinguish the risk-sharing hypothesis from implications of other transfer models. HRS families have more children than the PSID families. The National Longitudinal Surveys (NLS) have also used to study *inter vivos* transfers, see, for example, Light and McGarry (2003). Both PSID and NLS may also suffer from (non-random) attrition at the child level, since children households are separately interviewed and then matched to parent interviews (Fitzgerald et al., 1998). The NLS, in addition, will not be representative for parent–children matches due to selection on birth cohort sampling for both generations (Rosenzweig and Wolpin, 1993).

Table 2: Transfer incidence, made and received

transfers by number of waves, percent:	none	1	2	3	4	5	6	total
family level, made	24.60	17.89	15.82	14.26	12.40	8.98	6.04	100
child level, received	56.46	17.77	10.31	6.91	4.80	2.57	1.18	100

Note. Weighted statistics.

capture much of the difference in definitions. Forcing a $500 dollar threshold on to wave-2 information also turned out to be immaterial for our results.

From the third wave onwards, parents are requested to mention all transfers made over the last two years. In particular the latter will have an effect on the measured amount of transfers made, as shown below in the summary statistics. In regression analyses, we shall divide all transfer amounts by 2 for those interviews where a 2-year horizon applied. We convert all monetary values (income, wealth, and transfers) to 1991 dollars.

For descriptive statistics we impute missing values for transfer amounts, conditional on parents reporting that amounts were given, and conditional on available bracket information. We avoid using imputed values on transfer amounts in most of our empirical analyses in Section 4 where possible. We use imputed values for income and wealth throughout. Our measure of permanent income is likewise unobserved in the data (see Appendix C for details).

3.2 Descriptives

The data set has two dimensions that are of particular importance for our study: variation within a family between children, and variation for a given child over time. Table 2 displays transfer incidence reflecting these two dimensions. A quarter of all families never made any transfers during the entire observation period. More than half of all children never received any transfers. We count 18 percent of families making a transfer once and 6 percent making transfers each wave. 18 percent of children are reported to have received a transfer in one wave during our observation period, 1 percent is reported to have been receiving between every pair of waves. The pattern is consistent with families targeting transfers at particular children in a particular year.

Table 3 shows that 39 percent of the families gave in 1992 while 17 percent of the children received gifts. The corresponding numbers for 1996 are of very similar magnitude, while the incidences are higher in the 1994 wave. The amounts given and received are, however, slightly lower in 1994 compared to 1992. Due to the change in wording of the questionnaire, we find substantially larger amounts reported in the 1996 wave. Over all years, there is an increasing trend in transfer amounts as the parental generation ages. It appears that giving becomes more selective, as children become less likely to be recipients.

Table 4 cross-tabulates the number of children reported on in the family against

Table 3: Transfers made and received, per wave

wave			1992	1994	1996	1998	2000	2002
incidence:								
	family level	mean	0.39	0.52	0.44	0.38	0.37	0.34
		median	0	1	0	0	0	0
	child level	mean	0.17	0.24	0.20	0.17	0.16	0.14
		median	0	0	0	0	0	0
amounts:								
	family level,	mean	2,008	2,030	3,439	2,431	2,534	2,737
	unconditional	median	0	141	0	0	0	0
	conditional	mean	5,185	3,939	7,897	6,480	6,874	8,101
		median	2,500	1,414	3,352	2,970	2,453	2,692
	child level,	mean	585	579	967	679	703	749
	unconditional	median	0	0	0	0	0	0
	conditional	mean	3,424	2,409	4,714	3,915	4,360	5,401
		median	1,700	943	1,787	1,697	1,635	1,615

Note. Weighted statistics. Amounts are in 1991 dollars.
Conditional: statistics obtained on sample with only positive transfer amounts.

the number and fraction of parents who have given financial assistance, per wave. For families with two or more children, this fraction is decreasing in the number of children. Conditional on giving anything at all, a fraction of parents with more than one child gives the same amount to all children. Equal sharing is decreasing in the number of children. Around a fifth of the parents with two children give equally whereas only about 3 percent of the parents with more than four children give the same amounts. It is perhaps interesting to note that equal sharing appears to increase over time for 2- and 3- child families (while the parental generation ages). Allowing for some intrafamily variation does not substantially change the picture.

Table 5 suggests that not only are richer parents more likely to give at all, but also that higher net worth increases the likelihood of equal giving.

4 Estimation results

4.1 Estimation

Let us first introduce some notation: x is a vector of regressors, y is the observed outcome of interest (transfers from parents to child). The data vary in three dimensions: there are families (or, households), indexed h, at the outermost level. Within a given family, there are potentially many children, indexed k. For each child,

Table 4: Fractions of families giving and giving equally.

number of children		number of families					
		1992	1994	1996	1998	2000	2002
1	all	441	401	430	434	428	424
	% giving	33.7	51.7	42.3	34.3	33.5	27.4
	% giving equally	—	—	—	—	—	—
	—, ±20%[a]	—	—	—	—	—	—
2	all	1,190	1,145	1,188	1,192	1,195	1,167
	% giving	41.0	54.0	47.5	40.6	41.5	38.8
	% giving equally	11.3	14.9	19.8	19.5	19.1	24.5
	—, ±20%[a]	16.7	20.8	24.4	25.0	23.4	27.2
3	all	1,020	1,002	1,041	1,077	1,082	1,076
	% giving	39.9	51.5	45.0	39.6	37.7	37.7
	% giving equally	3.5	3.1	8.2	10.7	13.0	12.1
	—, ±20%[a]	4.7	3.7	9.5	11.7	13.5	12.6
4	all	768	786	824	830	855	832
	% giving	40.7	52.3	44.4	37.4	36.5	31.9
	% giving equally	1.7	3.0	5.7	7.1	9.1	6.5
	—, ±20%[a]	1.7	3.0	6.2	7.1	9.1	7.2
4+	all	1,080	1,122	1,238	1,295	1,326	1,344
	% giving	35.3	48.1	37.7	33.5	32.6	28.7
	% giving equally	0.6	0.9	3.0	4.3	5.2	2.9
	—, ±20%[a]	0.6	0.9	3.3	4.3	5.2	2.9

[a] Means: including allowing an absolute deviation of 20% from the intrafamily mean

Table 5: Parents' net worth (1991 dollars).

		1992	1994	1996	1998	2000	2002
total	number	4,499	4,456	4,721	4,828	4,886	4,843
	mean	221,763	240,869	249,037	272,994	315,808	304,046
giving	number	1,662	2,228	1,969	1,739	1,730	1,550
	mean	294,565	277,911	330,683	393,649	423,953	450,676
equal giving	number	210	326	351	313	321	274
	mean	344,122	340,822	469,669	633,360	588,364	834,085

Note. Weighted statistics. Net worth imputations from RANDHRS distribution.

Table 6: Interval tobit

observed data regime	interval	y^ℓ	y^u	f
uncensored	$[a,a]$	a	.	$\varphi(\tilde{y}^\ell)$
left-censored data	$(-\infty,b]$	b	.	$F(\tilde{y}^\ell)$
right-censored data	$[a,\infty)$	a	.	$F(-\tilde{y}^\ell)$
interval data	$[a,b]$	a	b	$F(\tilde{y}^u)-F(\tilde{y}^\ell)$

we have repeated observations over time, indexed t. Observation time is equally spaced (two years between sample waves).

Our model can be cast in terms of a latent variable formulation. This takes into account the many zero-transfer cases. Let $y^\star$ be the latent variable of notional transfers from parents to children and y the observed value which has a distribution censored from below, say. The model can then be written as

$$y^\star_{hkt} = x'_{hkt}\beta + \mathrm{error}_{hkt}. \tag{1}$$

We consider tobit type models as opposed to selection models as the latter are not semiparametrically identified, owing to the lack of exclusion restrictions implied by theory. There is also nothing in the existing data that our intuition suggests would be a suitable candidate to separate the intensive from the extensive transfer margin.

Many parents are not able or not willing to give exact amounts on transfers per children. In that case, the HRS questionnaire elicits amount information in pre-specified intervals $[a,b]$. We can generalize the model to an interval-tobit model, and specify the observed endogenous variable accordingly as a twin, y^ℓ and y^u. Denote the the standardized counterparts of y^ℓ and y^u by $\tilde{y}^\ell$ and $\tilde{y}^u$ and the likelihood contribution for an observation by f. The latter depends on the observational regime as outlined in Table 6.

Here, $\varphi(\cdot)$ denotes the (symmetric) density and F the corresponding *cdf*. If either $a \to -\infty$ or $b \to +\infty$, brackets are open and the standard likelihood contribution of the tobit obtains. The standard tobit would also apply when we relied on imputed values for the continuous but partially observed observations.

The error in (1) has three components,

$$\mathrm{error}_{hkt} = \varepsilon_{hkt} + \alpha_{hk} + \eta_h \tag{2}$$

where ε_{hkt} is an idiosyncratic error term, α_{hk} is a child-level effect, and η_h is a family-level effect. This is the nested structure considered by, for instance, Antweiler (2001) and Baltagi et al. (2001) for the linear model. Nesting means that α_{hk} are never shared between units h, but lie entirely within.

Incorporating (2) into the structure of Table 6 and postulating a parametric distribution for each of the components allows the model to be estimated by Maximum Likelihood. In the current application, we consider normal distributions for all three levels for reasons of feasibility. The resulting log likelihood function, expressed in terms of joint densities of data and random effects, to be maximized is then

$$\ln \mathscr{L}_h = \ln \left[\int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \prod_{k=1}^{K_h} \prod_{t=1}^{T_{hk}} f_{kt}(y^{\ell}_{hkt}, y^{u}_{hkt}, x_{hkt}, \eta_h, \alpha_{hk}; \theta) \mathrm{d}\alpha_{hk} \mathrm{d}\eta_h \right] \tag{3}$$

where K_h is the number of children in family h, and T_{hk} is the number of reports available on child k by family h. The sample is unbalanced at both levels. θ includes β and distributional parameters.

The model as it stands can be classified under the heading of generalized linear latent model, which has recently become popular in many areas of applied research. We specify the variance of the idiosyncratic error as a function of observables, so as to take into account heteroskedasticity. In particular, we postulate

$$\sigma_{hkt} = c \cdot \exp(x'_{hkt}\gamma) \tag{4}$$

which allows the idiosyncratic errors to be from a different distribution in a quite flexible way. As is well known (see Arabmazar and Schmidt, 1982), slope parameters β in limited dependent variable models may not be consistently estimated when heteroskedasticity is not taken into account.

To integrate out both levels of effects η_h and α_{hk}, we approximate the integral inside the likelihood function by a sum over draws from the estimated effects distribution (see Hajivassiliou and Ruud, 1994, for details of simulation methods within the maximum likelihood context). We have

$$\ln \mathscr{L}_h = \ln \left[\frac{1}{R} \sum_{r=1}^{R} \frac{1}{S} \sum_{s=1}^{S} \prod_{k=1}^{K_h} \prod_{t=1}^{T_{hk}} f_{kt}(y^{\ell}_{hkt}, y^{u}_{hkt}, x_{hkt}, \eta^{r}_{h}, \alpha^{s}_{hk}; \theta) \right]. \tag{5}$$

The standard simulation ML methodology relies on (pseudo-)random draws, which has the disadvantage that possibly many Monte Carlo draws are needed in order to accurately approximate the integral in question. In particular, with large data sets such as ours, and the additional complication of a nested error structure, this consideration becomes an issue. Recent statistical literature has turned to considering quasi-random draws. These are deterministic choices of support points for a distribution, giving the advantage of much better coverage over the entire domain of the distribution. See for instance Train (2003) for a discussion. This methodology has the big advantage that much fewer ‘draws’ are needed than standard (pseudo-)random Monte Carlo methods. We rely on Halton sequences.

Since quasi-random numbers are deterministic, results establishing the asymptotic equivalence between the simulated ML and the ML estimator (Hajivassiliou and Ruud, 1994) do not necessarily apply. To remedy this problem, we randomize the starting point of the Halton sequence, leading to what has become known as randomized Halton sequence (Bhat, 2003).

The model so far can be dubbed hierarchical random effects generalized tobit as it allows for a multi-level nested error structure. Mundlak (1978) showed the equivalence of estimators of slope parameters under fixed effects and under the random effects specification when augmented with time-averaged regressors in a standard, linear panel data model with large N and small T. Cameron and Trivedi (2005) and Wooldridge (2001) recommend the Mundlak strategy for nonlinear models. Since our panels are families (at least in one dimension), we condition on average child characteristics. This gives our estimates the interpretation of fixed family effects estimates, while not giving up the time dimension.

Alternative, non- or semiparametric estimators that take into account the nested levels are hard to conceive. For instance, Honoré (1992) fixed effects tobit estimator, which applies to a single effects structure, relies on first differencing within the panel (say, within families). It, therefore, cannot estimate parameters on variables that are fixed within the panel (say, family characteristics). Applying fixed effects to the inner structure would take differences among repeated observations of the same child and lead to child-level fixed effects estimates. One would lose the ability to condition on both variables that are fixed at child level over time and variables that are fixed at family level over time.[9] While we have considered differencing at the child level, the resulting estimate would be uninformative for our question of interest (within family variation of transfers). We shall below, however, present Honoré estimates for data that are aggregated per child, dropping the time dimension.

Note that we divide all transfer amounts by 2 in waves where the question referred to transfers in the previous two years, as opposed to the previous year (see Subsection 3.2).

Permanent income is constructed from child-level observables and relies much on education and age (flexible polynomial) and a number of other regressors. In so doing, we follow the methodology applied elsewhere in the literature and described in somewhat more detail in Kapteyn et al. (2005). In particular, permanent income relies on a prediction from current annual log income and varies over the life-cycle of an individual. Parents are very often not able to precisely state their children's household income, so that the interviewing method widely relies on bracket information. Our underlying income model is, therefore, similar to that of Table 6, except that it only features a child-level effect, a prediction of which we also add to the linearly predicted value. Given this prediction, we calculate income levels, and subsequently the present value, discounted at 4 percent between waves. Appendix C fills in on some details.

The data structure allows in principle to deliver large additional insights into what drives transfer behavior. Recall that from our data section the questionnaire allowed transfers to be reported both a gift and a loan. Intrafamily loans would be

[9] A similar remark applies to the approach outlined in Greene (2004).

relevant in the context of *liquidity constraints*. So, equipped with a measure of liquidity constraints at the individual child level, we could tell from the data whether transfers are gifts or whether they should better be understood as loans. Liquidity constraints at the child level are not directly observed, neither is consumption. We could rely on Zeldes (1989) approach and split the sample according to asset levels. We cannot observe net worth at the child level, however, only home ownership.

We rely on a different indicator. Recall that childrens' household income is measured as current income and that for many cases, only bracket information is available. We introduce a dummy variable that flags those observations where permanent income is predicted to lie above the upper bracket value of current income (or above current income if a continuous observation is available). This measure will pick out some of those children whose current income falls short of permanent income. We interpret it as a conservative indicator of liquidity constraints. If the coefficient of that variable is estimated to be insignificantly different from zero, we can give the data the interpretation that transfers are gifts, not loans. With the present data we do not see scope for improving on this measure.

4.2 Results

Our main results refer to estimates from the hierarchical generalized tobit as outlined in Subsection 4.1. There are two levels of nested 'random effects', one at the family level and one at the child level. At the family level we control for average children characteristics. In addition, we specify that the idiosyncratic error variance be a function of observables to allow for heteroskedasticity, as in (4).

Estimating this model by Maximum Likelihood when the dependent variable is measured in levels (dollar amounts) is very difficult, since convergence problems are severe. Alternatively, we subject amounts to a nonlinear transformation in order to take into account that the (conditional) distribution of the endogenous variable y is strongly skewed. The transformation we apply is the so-called inverse hyperbolic sine transformation (Burbidge et al., 1988), which is akin to the log transformation but can instead also accommodate zeros of the original variable,

$$z_{hkt} = \sinh^{-1}(y_{hkt}) = \ln\left(y_{hkt} + \sqrt{y_{hkt}^2 + 1}\right). \tag{6}$$

This transformation has been applied elsewhere in the literature on wealth and consumption (see, for instance, Browning and Crossley, 2004; Pence, 2006). While not exactly a log-transformation, in our data the approximation is sufficiently close so that we shall interpret the coefficient estimates on the regressors as semi-elasticities.

In terms of regressors, we condition on many parental, and most child characteristics that we can observe in the data. Child characteristics are reported by the family respondent in the parent household. They include a set of standard demographics (age, marital status, having any children), whether and how much the child works, whether it owns a home. We also include our constructed regressors of permanent income and the indicator of potential 'liquidity constraints'. We exclude education from our transfer equation so as to have identification of the permanent income effect.

We also observe whether the child lives in the vicinity of the reporting parent (within 10 miles), and whether it is a child as opposed to a step-child of the current family respondent and his or her spouse. We use the panel structure of the data and lag the observations by one wave (2 years) for a number of regressors: labor supply, marital status, having children, living close, owning a home, and potentially being liquidity constrained.

Parental characteristics include demographics, as well as measures of wealth and income. We use the number of children ever reported on by the household, as opposed to the number of children currently reported on as indicator of household size.

The sample underlying Table 7 deviates from the descriptive sample in a number of ways. We exclude families with children for whom we cannot necessarily expect the economic models to apply: exclusion of families where any child in any wave was born after 1974 (younger than 18 in 1992), results in 75,825 observations. Excluding any family where any of the children present in any year are still living at home, leaves us with 36,828 observations. Excluding those families where any child in any wave is still 'at school' leaves a sample of 19,935 observations. We then exclude any family that is not intact at the parent level over the entire observation period and any family that is not intact at the child-level.[10] We then select families with at least two children. We shall explore the sensitivity to various sample exclusions in Subsection 4.3.

Due to lagging some of the regressors once and further occurrence of missing values, the sample for Table 7 contains a total of 10,831 observations, representing 2,231 children from 735 different families. There are at most 44 observations for any of the families (children-wave observations).

Estimation proceeds by maximizing the simulated likelihood function (5) via a Newton Raphson minimizer.[11] The number of Halton draws is a choice parameter for us. We use 100 Halton numbers per observation, both at the parental and the child level. Bhat (2003) and Train (2003) report that 100 Halton numbers per observation yield accurate approximations to the true likelihood function. For the models in amount levels, which is a lot harder to estimate, we rely on 35 draws, after initial trials that suggested these numbers to be a good compromise between computational burden and numerical stability.[12]

The table contains two sets of coefficient estimates and associated standard

[10]We flag those families where a change in family composition took place during the observation period. Examples are at the parent level death, divorce or marriage, and at the child level incomplete mentions over the years or births or deaths. We classify families as being intact if at the parent level there is no recorded change in family status, due to death, remarrying or repartnering, and if at the child level no new children are added to the household.

[11]We coded our model in Fortran and rely on routine E04KDF of the NAG Fortran libraries.

[12]The log likelihood value, parameter estimates and standard errors are not much affected in the specifications with transformed endogenous variable when chosing this lower number of draws.

errors.[13] The left panel refers to the specification using the inverse hyperbolic sine transform of the endogenous variable, the right panel refers to estimates of the model in levels. Some, but not all, of the parameter estimates deserve short comments. We start by discussing the left panel.

Model with nonlinearly transformed endogenous variable. Children's age is not important in determining the amount received, perhaps owing to the fact that all of them are adults outside the parental home, earning their own incomes. Gender, however, seems to play a role, sons receive less than daughters. Age and gender will also enter indirectly via the permanent income variable, however. Education is excluded as a direct regressor but enters via permanent income. Children of both main respondent and (if present) his or her spouse receive significantly more money than stepchildren.

Working hours two years ago do not have any significant effects on transfers. The coefficient for married children is not significant, one possible explanation is that there may exist a second donor-couple (in-laws). Children with children of their own clearly receive more. On the other hand, physical vicinity does not appear to be important in this regression. Homeowners receive significantly less.

The parameter of prime interest is the coefficient on the permanent income variable, which is negative and highly significant. Note that permanent income is allowed to vary with age and will, therefore, change over time. The interpretation of that coefficient is similar to a semielasticity, to the extent that our transformation is close enough to the natural logarithm. It thus means that a decrease of permanent income by one dollar (in real terms, base 1991) triggers an increase of the gift amount by 0.003 percent. We can translate this into an absolute change, which depends on the initial gift amount. A child who initially receives 500 dollars, which is close to the unconditional mean, would receive slightly less than 2 cents more for a 1 dollar reduction of permanent income. The conditional mean for the gift amount is around 2,500 dollars. A child who initially receives this amount would receive about 8 cents more for a 1 dollar reduction of permanent income.

Recall that we have conditioned on average child characteristics, allowing us to interpret the coefficients as in a family fixed effects regression. Therefore, our estimate suggests that it is the difference between the siblings' permanent income, conditional on the intrafamily mean, that drives transfer behavior at the individual level. Parents observe their children's permanent income and reallocate resources towards equalization of differences. These compensatory *inter vivos* transfers are consistent with altruism. Clearly, the magnitudes involved suggest that full equalization is far from being achieved. This implies that the derivative condition is not fulfilled, see Appendix D.

In addition, our (conservative) proxy for liquidity constraints does not suggest that transfer behavior is driven by overcoming temporary financial strains, as the

[13] A number of parameters estimated along with those in the table has been suppressed for brevity. These include within-family average child characteristics and the specification of σ according to (4). These estimates are available from us on request.

Table 7: Regression results, main specification, model Table 6

transformation of dep. var.:	*sinh^{-1}*		*levels*	
Regressor name	estimate	standard error	estimate	standard error
children's characteristics				
age	-0.055	0.086	-14.9	29.1
male	-1.179	0.567**	-493.7	182.6**
biological	4.341	1.337**	1,696.0	444.2**
works $\geq$ 30 hours$_{t-1}$	-1.017	0.643	-229.6	170.2
works $<$ 30 hours$_{t-1}$	-0.103	0.882	-195.8	241.2
married$_{t-1}$	-0.768	0.613	-235.5	153.1
has children$_{t-1}$	2.562	0.674**	566.9	172.5**
$\leq$ 10m from parent$_{t-1}$	0.380	0.515	137.0	140.9
owns home$_{t-1}$	-3.707	0.575**	-1,300.7	191.7**
permanent income (10k$)	-0.313	0.062**	-157.0	25.2**
'liquidity constrained'$_{t-1}$	0.330	1.588	5.1	495.5
within-family averages	on request		on request	
parent household characteristics				
age	0.094	0.119	-48.5	40.3
education	0.672	0.179**	130.6	59.4**
male	-0.136	1.725	644.0	532.2
race: black	-2.117	1.723	-1,151.6	398.4**
race: other	-4.298	3.918	-1,654.7	1,197.5
ethnicity: hispanic	-2.301	3.297	-1,105.2	892.2
US born	-5.116	1.332**	-1,651.6	521.2**
household income$_{t-1}$ (10k$)	0.078	0.031**	41.0	21.5*
household net worth$_{t-1}$ (10k$)	0.015	0.005**	3.7	2.5
subjective health$_{t-1}$ fair	0.970	1.435	146.3	337.0
good	1.602	1.415	650.1	359.4*
very good	0.995	1.417	338.4	369.3
excellent	0.690	1.465	252.7	393.5
3 children	-4.021	0.893**	-1,012.6	296.6**
4	-8.914	1.168**	-4,039.7	512.8**
5	-5.519	1.244**	-2,374.2	388.9**
6	-4.332	1.554**	76.8	565.3
7 or more	-7.737	2.252**	-3,262.8	773.1**
constants				
intercept	-14.622	6.039**	-1,809.1	2,191.2
wave 1996	0.150	0.603	1,115.5	232.3**
wave 1998	-2.147	0.746**	715.4	271.3**
wave 2000	-1.990	0.887**	-229.0	367.9
wave 2002	-2.262	1.037**	616.7	389.9
$\ln(\sigma_\alpha)$	1.073	0.093**	6.545	0.175**
$\ln(\sigma_\eta)$	1.927	0.047**	7.847	0.050**
$\ln(\sigma) = x'\gamma$	on request		on request	
log-likelihood	-8,435.53		-16,767.86	

Note: Number of observations: $N = 10,831$, $H = 735$, $K = 2,231$
Numerical integration is based on 100 randomized Halton draws for the transformed model and 35 draws for the levels model. * and ** flag values significantly different from 0 at 10% and 5% levels, respectively.
σ_α and σ_η refer to child and family level random effects, respectively

coefficient on the respective variable is not significantly different from zero. Thus, our data can be meaningfully exploited in order to tell something about whether the transfers are gifts or temporary help. Resource flows are not only compensatory temporary smoothers but compensatory *inter vivos* gifts. Whereas the literature has speculated for a long time about the true nature of observed transfers,[14] our analysis is the first that uses the two dimensions of the panel (time and children) to identify the relevant channel. This is a main contribution of this paper.

The child level random effect (nested within the family level), is an important empirical contribution to the model. We estimate the log of its standard deviation to be 1.073 with a standard error of 0.09.

Among the family level variables we include the family respondent's demographics, race, income, assets, and subjective health evaluation (measured in five categories, ranging from excellent to poor). Parents that are higher educated give more, parents born abroad likewise give more, as do those with higher wealth. The coefficient on (lagged) household income (measured in 10,000$) is 0.078. Health appears to be unimportant in the decision to give to any particular child.

Conditioning on the number of children in the family shows that the higher the number of siblings, the lower the amount that a particular child will receive. The impact is nonlinear and nonmonotonic. An interpretation of the negative sign may have to do simply with the parental budget constraint.

We include time dummies to clean out wave-specific idiosyncrasies (measurement issues in the dependent variable and shocks to the regressors). We have also considered regional dummies, but found them to be not significant in preliminary runs, so we exclude them for parsimony.

Again, we estimate the log of the standard deviation of the family level effect to be large (1.927) and very strongly significant (standard error 0.05).

The validity of the Maximum Likelihood estimates displayed in Table 7 hinges upon the correct specification of the error distribution. Heteroskedasticity is one main concern, but is already accounted for in the displayed estimates to the extent that (4) is correctly specified. A Wald test clearly rejects the null of homoskedasticity with a test statistic of 101.6 at 33 degrees of freedom in the model under transformation (6), and even more clearly does so for the levels model.

Levels model. The right panel of the table shows estimation results of our model in levels, rather than under the transformation (6). While theoretically more appealing, it is computationally a lot more cumbersome. In addition, a Vuong (1989) asymptotic test for nonnested models selects the specification with nonlinearly transformed endogenous variable.[15] We thus chose not to use the levels specification as our point of departure for the sensitivity analysis to be reported in Subsection 4.3. The effect of raising the child's permanent income by one dol-

[14]See, for example, Laferrère and Wolff (2006), section 6, and Arrondel and Masson (2006), section 4.

[15]Standard normal test statistic of 18.2. For this, we adjust the likelihood function with a Jacobian term for the model with transformed endogenous variable, resulting in a log likelihood value of -11,344.3. This term is not incorporated in the values shown in the Tables.

lar equals 1.6 cents lower gifts in this specification, this is similar to the effect in the nonlinear specification when evaluated at the unconditional mean of the gift amount. Note, that due to computational reasons we use a smaller set of covariates in the specification of (4).

There is no difference between the level specification and the nonlinear transformation in the sense that liquidity constraints do not appear to matter. The coefficient estimates vary widely between specifications, however. The effect of lagged household income on transfers equals 0.4 cent per dollar.

Honoré model. As an alternative, we consider Honoré (1992) semiparametric estimators that are based on first-differencing two observations of a panel with two measurements to remove fixed effects. Two important strengths of the approach are that it allows for some general form heteroskedasticity and does not specify a functional form for the error distribution. In our context, our data has two dimensions (family and children) and we can take differences between two children of a family or between two repeated measurements for a child. The latter would remove both fixed family effects and fixed child effects, and not allow us to display estimates on variables that vary between children but are constant over time. Resulting parameter estimates are extremely noisy.

Instead, we apply the estimator to the following data structure: we take regressor values in the first wave of the survey (1992) and use as endogenous variable the average transfer amounts reported in subsequent surveys (1994–2002). We thus use one observation per child.

We use the estimator for censored observations that is based on a smooth conditional moment condition. Since our sample includes families with more than two children (unbalanced panel data set), we can estimate the model for all perceivable pairwise combinations of children within a family. To form pairwise combinations of children, we first order children by age.

We obtain a set of estimates which will differ numerically, but we can impose over-identifying restrictions using a minimum distance criterion to obtain a single estimator. The convergence of the estimator is sensitive to the amount of censoring. We had to disregard all pairwise combinations of children where more than 90 percent of the observations were censored (no gifts). Also, we disregard all combinations of children comprising less than 100 families in order to have identification. We finally disregard all those estimates where the covariance matrix was singular.

Results are in Table 8. We cannot control for any family-level variables as they would be removed by the differencing. In addition, we drop the measure of liquidity constraints from consideration since we shifted focus away from the longitudinal dimension of our data.

Whereas the estimates in Table 8 are from a different model, the point estimates do tell a similar story as those in Table 7, and are perhaps surprisingly close, in particular the coefficient on permanent income. Some of the other coefficients are quite different, however. We see the largest deviations in the level specifications, perhaps owing to misspecification.

Table 8: Regression results, fixed effects tobit, Honoré (1992)

transformation of dep. var.:	*$sinh^{-1}$*		*levels*	
Regressor name	estimate	standard error	estimate	standard error
children's characteristics				
age	0.020	0.045	16.4	21.2
male	0.220	0.253	518.7	91.2**
biological	1.836	0.645**	56.7	226.4
works $\geq$ 30 hours	-0.491	0.365	-1,059.3	202.8**
works $<$ 30 hours	-0.044	0.546	-627.8	224.2**
married	-0.936	0.274**	502.6	104.3**
has children	1.215	0.291**	154.5	110.9
$\leq$ 10m from parent	0.801	0.260**	119.9	108.2
owns home	-0.617	0.281**	-344.8	126.7**
permanent income (10k$)	-0.402	0.069**	-61.7	29.6**

Note: number of observations: $N = 2,178$, $H = 729$, $K = 2,178$

The results reported Table 8 suggest that an average child will receive a 0.6 cent compensation for a one dollar difference in permanent income compared to a sibling (levels model), or that a 10,000 dollar lead in permanent income over a sibling will result in a 40 percent lower parental transfer (transformed model). These numbers are far from the alleged 1-on-1 compensation needed to support the conclusion of altruistic behavior.

4.3 Sensitivity analysis

Table 9 presents results obtained by deviating from our preferred specification (the left panel of Table 7). The table only shows the coefficients on child permanent income and our 'liquidity constraints' indicator, as well as on parents' household income. Given computational burden and the low performance in sensitivity checks, we disregard the levels specification from now on.

A first check relates to heteroskedasticity. While the Wald test reported earlier rejects a constant in favor of (4), a homoskedastic specification leads to inconsistent estimates. Reestimating, we find that the coefficient of child permanent income more than halves. Conclusions as to the other two main coefficients are largely unaffected, however. A likelihood ratio test confirms the conclusion from the Wald test.

Second, we can similarly test whether conditioning on within-family average child regressors is a contribution to the model. While neither the Wald test from specification of Table 7 nor an LR test (test statistics each of 18.0 at 11 degrees of freedom), reject the null, the coefficient estimate of interest is attenuated, compared to the baseline.

Third, we want to explore the sensitivity to using imputed values rather than nonimputed values. The imputation is based on simple draws from the observed

Table 9: Regression results, deviations from baseline

specification	child permanent income	'liquidity constrained'	parent household income	*N*	*H*	*K*	*log likeli-hood*
baseline, Table (7)	-0.313	0.330	0.078	10,831	735	2,231	-8,435.5
	(0.062)	(1.588)	(0.031)				
baseline, homoskedastic	-0.140	-0.440	0.078	10,831	735	2,231	-8,481.3
	(0.039)	(1.548)	(0.029)				
random family effects	-0.246	0.013	0.082	10,831	735	2,231	-8,444.5
	(0.056)	(1.577)	(0.031)				
tobit instead of interval model (multiply imputed)	-0.316	0.197	0.084	10,831	735	2,231	-8,698.1*
	(0.061)	(1.580)	(0.029)				
non-intact families dummied rather than excluded	-0.333	1.018	0.078	14,046	995	3,099	-10,887.9
	(0.054)	(1.509)	(0.029)				
first three waves only	-0.751	-0.646	0.198	6,523	1,056	3,330	-5,818.0
	(0.108)	(1.642)	(0.034)				
children at home and at school individually excluded, not the entire family	-0.409	-0.212	0.066	58,033	4,210	14,186	-44,490.7
	(0.033)	(0.876)	(0.012)				
including one-child families	-0.354	1.319	0.102	11,431	864	2,360	-9,248.7
	(0.060)	(1.229)	(0.026)				

Notes: standard errors in parentheses, * averaged over estimates obtained from 5 imputes continuous distribution, taking into account the bracket information, and is in this sense nonparametric (hot deck). Since imputed values reduce the number of observational regimes to two (zeroes and continuous observations), we can use a simpler, traditional tobit set-up instead of the generalization in Table 6. We use multiple imputation techniques in order to take into account the imputation error, and a conventional number of five imputes (Rubin and Schenker, 1986). The table reveals coefficient estimates and standard errors that are very similar to those of the baseline specification. There is no large impact of assuming normality for the within-bracket distribution. We prefer the baseline for computational reasons.

Fourth, we investigate what difference it makes to flag households that change over time (anywhere in the observation period), rather than excluding them altogether. We use two dummy variables, one for the family level, one for the child level. This increases the sample to 14,046 observations (3,099 children in 995 families). The other sample restrictions still apply. The resulting estimates of child's permanent income as well as that of parental income are very close to those of the baseline. However, it appears that the dummy proxying for potential liquidity constraints increases in magnitude (but stays insignificant).

Fifth, we assess the importance of the time horizon by reestimating and ignoring any information beyond wave 3 of the survey. (Note that permanent income is still based on all six waves.) Doing this may make it less likely that a family is subjected to our sample exclusion restrictions. This may change sample composition, and it may to a lesser extent capture long-run giving behavior. The effective sample has 6,523 observations on 3,330 children in 1,056 families. While we do not see any effect of liquidity constraints, the magnitude of the permanent income derivative is much larger than in the shorter sample (coefficient of -0.751). We also see a much larger effect of parent household income on gifts.

Sixth, instead of casting away entire families where at least one child is either at home or at school during the observation period of six waves, we instead exclude only those observations from a child that is at home or at school. This increases the sample substantially to 14,186 children in 4,210 families, yielding 58,033 observations. The effects on the coefficients of interest are minor.

Seventh, the model as such can technically be estimated on a sample of parents with a single child. This increases the sample by another 129 families and children. The impact on the coefficient of child permanent income is minor, as is the impact on the coefficient of parent household income.

5 Conclusion

Parents' transfer motives are important for understanding, e.g., macroeconomics, income (re)distribution, savings, and public finance. In this paper we use data from six biennial waves of the Health and Retirement Study (HRS) 1992–2002 to study parents' transfers to their children. The HRS is an excellent data set to study questions addressed in our paper. The coverage of the pre-retirement cohort includes those who have accumulated substantial wealth from life cycle savings. They are, therefore, in a position where they can afford to give away money. Moreover, as they are about to retire within the foreseeable future, they make conscious decisions about how to use the accumulated resources. Possibly even more importantly, the HRS contains information on two generations of the same family, parents and children.

Three fourths of all families give something to at least some of their children. Conditional on giving at all, we find that only 2 percent of parents in the HRS divide their gifts equally among their children. Equal sharing is decreasing in the number of children but increasing in the wealth of parents.

The predictions of the transfer theories are predictions of the within-family-variation in transfer behavior, not the between-family-variation. Data, therefore, need to have a family dimension and a sibling dimension. In addition, data need a time dimension to be used to test whether transfers are gifts or temporary help. We estimate grouped tobit-type latent variable models with multi-level error components. We specify the unobservables to fall into three categories: a random error term, varying over families, children, and time; a child-level specific error component, varying over children and constant over time; a family-specific error component, varying over families and constant for all children in the family (and constant over time).

Our main findings are as follows:

1. We find that intergenerational transfers flowing from parents to children are *inter vivos* gifts, not temporary help to overcome liquidity constraints.

2. Gift amounts from parents are compensatory in the sense that life-time poorer children will receive higher transfers than their life-time richer siblings.

3. We do not find, however, that gifts make up the entire difference in life-time resources. One dollar less of life-time income compared to a sibling triggers about 2 cents of expected (unconstrained) *inter vivos* gifts (levels model) or an increase by 0.003 percent (semielasticity in a nonlinearly transformed model).

The sensitivity analysis shows that these findings are robust to variations in model specifications and samples. Our parametric approach gives similar results to the semiparametric approach proposed by Honoré (1992) on a one-way model.

The empirical result that transfers are compensatory is consistent with the predictions of the altruistic model of intergenerational transfers. Parents do not, however, seem to completely equalize income differences between siblings as predicted. But there is also evidence that can be interpreted as consistent with the predictions of the exchange model of intergenerational transfers. For instance, daughters get more than sons which might have an exchange explanation. Living close to the parents does not matter for gifts in some specifications, however, which might have been expected in an exchange framework. The general conclusion is, therefore, that there is not just a single theory of transfers that is uniformly supported by the data.

There is no support, however, for the egoistic model as we do find that transfers are compensatory. The risk-sharing hypothesis does not receive support either as transfers do not seem to be temporary help.

References

Altonji, J. G., Hayashi, F., and Kotlikoff, L. J. (1992). Is the extended family altruistically linked? Direct tests using micro data. *American Economic Review*, 82(5):1177–1198.

Altonji, J. G., Hayashi, F., and Kotlikoff, L. J. (1997). Parental altruism and inter vivos transfers: Theory and evidence. *Journal of Political Economy*, 105(6):1121–1166.

Andreoni, J. (1989). Giving with impure altruism: Applications to charity and Ricardian equivalence. *Journal of Political Economy*, 97(6):1447–1458.

Antweiler, W. (2001). Nested random effects estimation in unbalanced panel data. *Journal of Econometrics*, 101(2):295–313.

Arabmazar, A. and Schmidt, P. (1982). Further evidence on the robustness of the Tobit estimator to heteroskedasticity. *Journal of Econometrics*, 17(2):255–258.

Arrondel, L. and Masson, A. (2006). Altruism, exchange or indirect reciprocity: What do the data on family transfers show? In Kolm, S.-C. and Ythier, J. M., editors, *Handbook of the Economics on Giving, Altruism and Reciprocity*, volume 2, chapter 14, pages 971–1053. North-Holland, Amsterdam.

Baltagi, B. H., Song, S. H., and Jung, B. C. (2001). The unbalanced nested error component regression model. *Journal of Econometrics*, 101(2):357–381.

Barro, R. J. (1974). Are government bonds net wealth? *Journal of Political Economy*, 82(6):1095–1117.

Becker, G. S. (1974). A theory of social interactions. *Journal of Political Economy*, 82(6):1063–1093.

Bernheim, B. D. and Severinov, S. (2003). Bequests as signals: An explanation for the equal division puzzle. *Journal of Political Economy*, 111(4):733–764.

Bernheim, B. D., Shleifer, A., and Summers, L. H. (1985). The strategic bequest motive. *Journal of Political Economy*, 93(6):1045–1076.

Bhat, C. R. (2003). Simulation estimation of mixed discrete choice models using randomized and scrambled Halton sequences. *Transportation Research B*, 37(9):837–855.

Blinder, A. B. (1974). *Toward an Economic Theory of Income Distribution*. MIT Press, Cambridge, MA.

Browning, M. and Crossley, T. F. (2004). Shocks, stocks, and socks: Smoothing consumption over a temporary income loss. CAM Working Paper 2004-05, Centre for Applied Microeconometrics, Institute of Economics, University of Copenhagen.

Burbidge, J. B., Magee, L., and Robb, A. L. (1988). Alternative transformations to handle extreme values of the dependent variable. *Journal of the American Statistical Association*, 83(401):123–127.

Cameron, C. A. and Trivedi, P. K. (2005). *Microeconometrics. Methods and Applications*. Cambridge University Press, Cambridge, UK.

Cardoso, A. (2000). Wage differentials across firms: An application of multilevel modelling. *Journal of Applied Econometrics*, 15(4):343–354.

Coate, S. and Ravallion, M. (1993). Recoprocity without commitment: Characterization and performance of informal insurance arrangements. *Journal of Development Economics*, 40(1):1–24.

Cox, D. (1987). Motives for private income transfers. *Journal of Political Economy*, 95(3):508–546.

Cremer, H. and Pestieau, P. (1996). Bequests as heir "discipline device". *Journal of Population Economics*, 9(4):405–414.

Davies, J. B. (1981). Uncertain lifetime, consumption, and dissaving in retirement. *Journal of Political Economy*, 89(3):561–577.

Dunn, T. A. and Phillips, J. W. (1997). The timing and division of parental transfers to children. *Economics Letters*, 54(2):135–137.

Fitzgerald, J., Gottschalk, P., and Moffitt, R. (1998). An analysis of the impact of sample attrition on the second generation of respondents in the Michigan Panel Study of Income Dynamics. *Journal of Human Resources*, 33(2):300–334.

Friedman, B. and Warshawsky, M. (1990). The cost of annuities: Implications for saving behavior and bequests. *Quarterly Journal of Economics*, 105(1):135–154.

Greene, W. H. (2004). Fixed effects and bias due to the incidental parameters problem in the Tobit model. *Econometric Reviews*, 23(2):125–147.

Hajivassiliou, V. A. and Ruud, P. A. (1994). Classical estimation methods for LDV models using simulation. In Engle, R. F. and McFadden, D. L., editors, *Handbook of Econometrics*, volume 4, chapter 40, pages 2383–2441. North-Holland, Amsterdam.

Honoré, B. E. (1992). Trimmed LAD and least squares estimation of truncated and censored regression models with fixed effects. *Econometrica*, 60(3):533–565.

Hurd, M. D. (1989). Mortality risk and bequests. *Econometrica*, 57(4):779–814.

Kapteyn, A., Alessie, R., and Lusardi, A. (2005). Explaining the wealth holdings of different cohorts: Productivity growth and social security. *European Economic Review*, 49(5):1361–1391.

Kimball, M. (1988). Farmers' cooperatives as behavior toward risk. *American Economic Review*, 78(1):224–232.

Kotlikoff, L. J. and Spivak, A. (1981). The family as an incomplete annuities market. *Journal of Political Economy*, 89(2):372–391.

Laferrère, A. and Wolff, F.-C. (2006). Microeconomic models of family transfers. In Kolm, S.-C. and Ythier, J. M., editors, *Handbook of the Economics on Giving, Altruism and Reciprocity*, volume 2, chapter 13, pages 889–969. North-Holland, Amsterdam.

Laitner, J. (1997). Intergenerational and interhousehold economic links. In Rosenzweig, M. R. and Stark, O., editors, *Handbook of Population and Family Economics*, volume 1A, chapter 5, pages 189–238. North-Holland, Amsterdam.

Laitner, J. and Ohlsson, H. (2001). Bequest motives: A comparison of Sweden and the United States. *Journal of Public Economics*, 79(1):205–236.

Light, A. and McGarry, K. (2003). Why parents play favorites: Explanations for unequal bequests. Working Paper 9745, National Bureau of Economic Research, Cambridge, MA.

Lundholm, M. and Ohlsson, H. (2000). *Post mortem* reputation, compensatory gifts and equal bequests. *Economics Letters*, 68(2):165–171.

Masson, A. and Pestieau, P. (1997). Bequests motives and models of inheritance: A survey of the literature. In Erreygers, G. and Vandevelde, T., editors, *Is Inheritance Legitimate? Ethical and Economic Aspects of Wealth Transfers*, chapter 3, pages 54–88. Springer Verlag, Berlin.

McGarry, K. (1999). Inter vivos transfers and intended bequests. *Journal of Public Economics*, 73(3):321–351.

McGarry, K. (2000). Testing parental altruism: Implications of a dynamic model. Working Paper 7593, National Bureau of Economic Research, Cambridge, MA.

McGarry, K. and Schoeni, R. F. (1995). Transfer behavior in the Health and Retirement Study: Measurement and the redistribution within the family. *Journal of Human Resources*, 30(Supplement):S184–S226.

McGarry, K. and Schoeni, R. F. (1997). Transfer behavior within the family: Results from the Asset and Health Dynamics Survey. *Journals of Gerontology*, 52B(Special issue):82–92.

Mundlak, Y. (1978). On the pooling of time series and cross section data. *Econometrica*, 46(1):69–85.

Nordblom, K. and Ohlsson, H. (2006). Tax avoidance and intra-family transfers. *Journal of Public Economics*, 90(8-9):1669–1680.

Pence, K. M. (2006). The role of wealth transformations: An application to estimating the effect of tax incentives on saving. *Contributions to Economic Analysis & Policy*, 5(1). Article 20.

Pestieau, P. (2003). The role of gift and estate transfers in the United States and in Europe. In Munnell, A. H. and Sundén, A., editors, *Death and Dollars. The Role of Gifts and Bequests in America*, chapter 3, pages 64–85. Brookings, Washington, DC.

Rabe-Hesketh, S., Skrondal, A., and Pickles, A. (2005). Maximum likelihood estimation of limited dependent variables with nested random effects. *Journal of Econometrics*, 128(2):301–323.

Rosenzweig, M. R. and Wolpin, K. I. (1993). Intergenerational support and the life–cycle incomes of young men and their parents: Human capital investments, coresidence, and intergenerational financial transfers. *Journal of Labor Economics*, 11(1):84–112.

Rubin, D. B. and Schenker, N. (1986). Multiple imputation for interval estimation from simple random samples with ignorable nonresponse. *Journal of the American Statistical Association*, 81(394):366–374.

Train, K. E. (2003). *Discrete Choice Methods with Simulation*. Cambridge University Press, Cambridge, UK.

Vuong, Q. H. (1989). Likelihood ratio tests for model selection and non-nested hypotheses. *Econometrica*, 57(2):307–333.

Wooldridge, J. M. (2001). *Econometric Analysis of Cross Section and Panel Data*. MIT Press, Cambridge, MA.

Zeldes, S. P. (1989). Consumption and liquidity constraints: An empirical investigation. *Journal of Political Economy*, 97(2):305–346.

A Data

A.1 Introduction

We use data from the core HRS cohort of the HRS and from the RANDHRS distribution of the same data. All data is available online via the University of Michigan's Institute for Social Research web site. This website has last been accessed for checking 'data alerts' and possible download of data on November 11, 2005.

The data is a panel data set with biennual sampling and contains information on households where at least one respondent was born in between the years 1931 and 1941. These people are followed over time in subsequent interviews. There is no refreshment sampling for respondents that drop out. Respondents leave the sample due to death or due to refusal to participate. The survey was started in 1992, and we use six waves through 2002 (final release versions).

We disregard 'exit' interviews, i.e., interviews conducted with a proxy respondent eliciting information on former respondents that have died since their previous interview. Some crucial variables have been imputed by HRS staff and (single) imputations are distributed in imputation files. Whereas imputations on incomes, wealth, and transfers to children are of interest, we rely on own mulitple imputations for transfers, and on income and wealth imputations from RANDHRS files. To link data across time for the same respondents, 'tracker' files are provided in the main distribution. These tracker files come in two versions: one respondent level file, and one 'other person' level distribution, LOPN.

The data are suitable to estimate economic models based on observed behavior of intrafamily monetary transfers. For this, we need information on a parental household and their children. The information needed consists of

1. identifiers for all households, parents and children in the family, consistent over time
2. individual characteristics of parents and children
3. indicators of whether or not transfers were made between parents and children in a given wave
4. amounts of transfers made in a given year

Household composition changes over time and we want to minimize as far as possible measurement problems that are due to information being reported from different sources. The data distribution consists of hundreds of data files with information at different levels of aggregation. We, therefore, need identifiers that consistently allow us to track which information on what child was reported by whom. Building a data set suitable for analysis, including a number of consistency checks and imputation of missing values on crucial variables is not a straightforward exercise. A document giving some more detail on procedures applied is available from the authors on request.

A.2 Identifiers

There are various levels of identifiers in the data, information on which is taken from the two tracker files (respondent and other-person tracker files):

1. A set of household identifiers, `HHID xSUBHH`. `xSUBHH` is a wave-specific sub-household identifier for wave `x`, and becomes relevant when an existing household splits in two. All sub-households emerging from original households `HHID` are in principle followed up.

2. A person-level identifier, typically a respondent's person number, `PN`, within the household, or, a 'longitudinal other person number', `LOPN`. `LOPN` identifies non-respondents (such as children).

3. We constructed an additional identifier from available information in the data to associate the report of a particular parent (family respondent) to a particular child (nonrespondent), `FAMSTORY`, used for merging data sets of parental level information with those of child level information.

An observation in our analysis will be uniquely identified by `HHID LOPN FAMSTORY wave` where `wave` identifies the survey wave. Using the `FAMSTORY` linker effectively lets us abandon the `xSUBHH` identifier.

A.3 Data extraction and main changes to data

Data preparation involves a number of steps, which we briefly describe.

Parent-level identifiers and definition of intact households

We collect basic fixed characteristics of respondent and spouse, including their relationship (i.e., which person to match with whom at what wave) from the main tracker file. We use this information on respondent level to assess whether or not household composition has changed over time. This gives rise to various possible definitions on whether a household is 'intact'.

The definition we employ is based on the following: a household is 'intact' either if it contains only one respondent over time, or if it contains two respondents who are and stay a couple. Not intact, according to this definition, are couples that split into sub-households, households where a new partner enters, or where a partner dies.

We also determine person-level identifiers for family and financial respondents.

Children-level story indicators

All information on nonrespondents (children) stems from respondents (parents). In case an original household splits, subsequent information on children may be supplied by all split off sub-households, giving rise to multiple 'stories' about the same child.[16] While the HRS distribution contains an identifier linking sub-households and children and identifying such ‘stories’, there may be remaining variation in terms of child information within a given ‘story’ when the identity of the family respondent changes within a subhousehold over time. To remedy this, we create the additional linker, `FAMSTORY`, mentioned above. `FAMSTORY` keeps the `PN`/`LOPN` association constant over time and, therefore, identifies within `HHID LOPN` the source of the information given. We exclude all observations from further analysis where respondents report on children that do not receive a `LOPN` identifier or where no link between family respondent and child could be established.

Select family respondent and determine relationship with child

We fix the identity of a family or financial respondent for our purposes, to avoid changes in parental characteristics over time (so, the actual family respondent may change in the data, but we instead use the characteristics of a fixed person and refer to that person henceforth as the family respondent). The family respondent is typically that of the first wave, and will be substituted with some other respondent if not available in the first wave.

Child-level nonrespondents may or may not be children of at least one of the main respondents. We clean up inconsistencies between waves due to different wordings of identifying questions between waves, and fill in gaps. Where interviewees are being presented with data from previous interviews, the more recent information may be more accurate and serve as an update. We determine the relationship between child-level nonrespondents and respondents and subsequently exclude those from consideration that are not a child of either family respondent or the spouse (for instance, grandchildren or children in-law).

Definition of children-intact sub-households

The number of children mentioned by a given sub-household can change by either children not mentioned before ‘entering’ or children mentioned before not being mentioned anymore. There are very few occasions where a child is born into a household or dies between waves. It is more likely that a new spouse entering the household also has children of his or her own.

This requires to define whether a sub-household is intact at the child level and information on parent/subhousehold level. We define as intact between waves at the child level those sub-households whose composition is unchanged at child level.

[16] Suppose there are initially two respondents in a household, and the household splits in two after the first wave, then both subhouseholds may have their own family respondent who delivers a ‘story’ on the child (or other person) in question.

Parental characteristics

Respondent-level characteristics are from three types of sources. Characteristics can refer to a particular respondent or to the entire (sub-)household. Fixed characteristics, such as age, sex, education etc., plus some other background characteristics, originate from the Tracker file. Health, income and wealth information is from wave-specific files.

Since both income and wealth are composites of a variety of sources, on which missing values tend to aggregate, we rely on imputed aggregates. For consistency reasons, we use the regression-based RANDHRS (version E) imputations on household incomes and assets.

Transfers to children

We extract information on whether and how much money was transferred to each child. From wave 1996 on, transfers could be mentioned in the form of 'same amount to all my children/grandchildren'. We impute in those cases the relevant amount to each child based on the full set of children of the respondent. Many amounts are only given in brackets (if at all), and we impute missing values ourselves (hotdeck conditional on bracket information) to iron out inconsistencies in available HRS imputations and to have multiple imputations available.

Child characteristics

We clean and impute demographics and background characteristics for children across waves. Not all variables were elicited for each child in each wave, in which case missing values need to be filled in. In addition, there are recall errors or other types of response errors that lead a certain respondent to characterize their children differently over time even in terms of fixed characteristics (such as sex and year of birth). We correct 'errors' as far as possible and reasonable, yielding sex and year of birth information that does not change over time for a given child. Education is made to be nondecreasing over time, and having children in two non-adjacent waves but not in a wave in between is also interpreted as error (we do otherwise allow for children of a child to die or to be born, though). Other characteristics, such as whether or not a child attends school, stays at home, works, or its income, are not updated.

Monetary values

All monetary values relating to wealth, income, and transfers, have been converted into 1991 dollars using the 'All Urban Consumers' Consumer Price Index as published on the Bureau of Labor Statistics' website (series ID : CUUR0000SA0), rebased where necessary. Monetary flow variables are, when necessary, transformed to 12 month frequencies.

Table 10: Sample statistics 1992, children.

variable	individual children: mean	SD	min	max	family means: mean	SD	min	max
gifts received	.159				.210	.322	0	1
gift amount, 1991 $	507	2,367	0	80,000	799	2,955	0	80,000
permanent income, 1991 k$	40.91	39.85	1.85	1,521.1	41.88	28.71	3.39	535.40
does not work	.247				.236	.298	0	1
works $<$ 30 hours per week	.091				.098	.197	0	1
works $\geq$ 30 hours per week	.662				.667	.330	0	1
homeowner	.363				.373	.348	0	1
biological child	.844				.878	.281	0	1
male	.511				.515	.312	0	1
grandchildren	.536				.482	.362	0	1
married	.509				.499	.353	0	1
lives $<$ 10 miles from parents	.527				.536	.367	0	1
age	29.0	7.07	0	60	28.57	5.85	0	54.7
years of education	12.6	2.60	0	17	12.99	2.14	0	17
lives at home	.198				.216	.310	0	1
goes to school	.171				.191	.295	0	1
chg. in fam. comp. 1992 – 1994	.067				.068	.251	0	1
chg. in fam. comp. 1994 – 1996	.092				.073	.259	0	1
chg. in fam. comp. 1996 – 1998	.088				.065	.246	0	1
chg. in fam. comp. 1998 – 2000	.067				.049	.216	0	1
chg. in fam. comp. 2000 – 2002	.118				.086	.280	0	1

Note: weighted sample; based on 15,795 children in 4,499 families; different variables have different numbers of missing values; family means are sample averages over children within the same family; statistics refer to the 1992 wave (except changes between waves indicators)

B Sample statistics

The weighted sample statistics for the children can be found Table 10. The columns to the left report sample statistics for the individuals while the columns to the right concern the sample statistics of the means of the children in each family. Table 11 reports the weighted sample statistics for parents. In case of two-parent households, the characteristics are for the family respondent. Exceptions are net worth and income which refer to both spouses.

Table 12 shows dollar amounts given by parents to all their children, aggregated over all sample waves. These amounts decrease in the number of children. Parents who share equally give more than other parents.

Table 11: Sample statistics 1992, parents.

variable	mean	family respondent: SD	min	max
gifts made	.369	.483		
gift amount, 1991 $	1,780	5,417	0	160,000
net worth, 1991 1,000 $	199,495	399,903	-319,000	6,202,000
income, 1991 1,000 $	45,510	47,628	0	1,010,000
number of children	3.51	1.98	1	19
male family respondent	.064			
health, poor	.064			
health, fair	.135			
health, good	.264			
health, very good	.299			
health, excellent	.239			
age	54.2	5.15	27	72
years of education	12.1	3.97	0	17
Caucasian	.796			
African American	.172			
other non–Caucasian	.033			
Hispanic	.083			
chg. in fam. comp. 1992 – 1994	.032			
chg. in fam. comp. 1994 – 1996	.049			
chg. in fam. comp. 1996 – 1998	.059			
chg. in fam. comp. 1998 – 2000	.054			
chg. in fam. comp. 2000 – 2002	.067			

Note: as in Table 10.

Table 12: Aggregate amounts given.

number of children	number of families giving	amount: family mean	amount: family SD	number of families giving equally	amount: family mean	amount: family SD
1	538	9,824	23,124	—	—	—
2	1,361	8,447	21,844	63	22,702	38,766
3	1,155	4,715	9,599	21	7,871	9,737
4	873	3,343	6,961	9	12,458	25,836
> 4	1,283	1,868	5,520	8	5,352	3,647

Note. Weighted statistics.

C Permanent income

Our measure of children's permanent income is based on a random effects model that regresses current log income (in 1991 dollars) on a number of observables. We include as regressors linear splines in age and education, an age/education interaction, sex, and the child's race and ethnicity derived from both parent's race and ethnicity. Also, time (wave) dummies are included. We follow the methodology set out in Kapteyn et al. (2005) which calculates a time-varying measure of permanent income from a regression of log current income on observables. Unlike these authors, we do not take into account cohort effects.

The measure of current income differs across survey waves. In the 1992 wave, only qualitative information is available, that is, we know if the child's annual (nominal) family income fell short of 10,000 dollars, exceeded 25,000 dollars, or fell in between. In the 1994 wave, parents are actually requested to supply an estimate of the amount of children's incomes, and if they were unable to do this, they were presented with a range card and asked to indicate an appropriate bracket with threshold values of 10,000, 25,000, and 40,000 dollars, respectively. In 1996 and in subsequent waves, child income was elicited in a similar manner, except that the income thresholds were 10,000, 35,000, 50,000, and 100,000, and that brackets were elicited subsequently (unfolding bracket technique).

The regression model used takes this heterogeneous information into account in that it allows for continuous, discrete, and bracketed values in the endogenous variable; hence, the model is a generalized censored regression model, much as that of Table 6. It includes a composite error that has an individual specific random effect and an idiosyncratic error.

The model was estimated in the same way and with the same software as the two-level model for transfers. We add an estimate of the random effect to our predicted current income before calculating permanent income. This estimate, conditional on data and estimated parameters, obtains from the likelihood contribution as

$$\hat{\alpha}_k|(y^\ell_{kt}, y^u_{kt}, x_{kt}, \alpha^s_k; \theta) = \frac{\sum_{s=1}^S \alpha^s_k \prod_{t=1}^{T_k} f_{kt}(y^\ell_{kt}, y^u_{kt}, x_{kt}, \alpha^s_k; \theta)}{\sum_{s=1}^S \prod_{t=1}^{T_k} f_{kt}(y^\ell_{kt}, y^u_{kt}, x_{kt}, \alpha^s_k; \theta)}. \tag{7}$$

Having obtained the linear prediction, we convert log income to levels, and calculate permanent income by assuming a working life span ranging from 18 to 65. We discount future incomes at 4% per year. The resulting estimate of (annualized) permanent income is then obtained, and we discard a handful of observations with negative values and those with permanent income of more than \$2m annual.

We have convinced ourselves that changes in specification and assumed interest rate in the calculation of permanent income do not reverse our conclusions from the main estimates presented.

D The derivative condition in fixed family effects models

Theory predicts that an altruistic parent will equalize the consumption possibilities of her children and choose:

$$G_i - G_j = -(Y_i^c - Y_j^c) \qquad i, j = 1 \ldots n, j \neq i. \tag{8}$$

where G_i is the gift to child i, Y_i^c is the income of child i, G_j is the gift to child j, Y_j^c is the income of child j, and n is the number of siblings. The consumption possibilities will then be the same for all children. The derivative of (8) is:

$$\frac{d(G_i - G_j)}{d(Y_i^c - Y_j^c)} = -1 \qquad i, j = 1 \ldots n, j \neq i. \tag{9}$$

Suppose that (8) holds. The separate derivatives of the difference in gifts with respect to the two incomes are:

$$\begin{aligned} \frac{d(G_i - G_j)}{dY_i^c} &= \frac{\partial G_i}{\partial Y_i^c} - \frac{\partial G_j}{\partial Y_i^c} = -1 \\ \frac{d(G_i - G_j)}{dY_j^c} &= \frac{\partial G_i}{\partial Y_j^c} - \frac{\partial G_j}{\partial Y_j^c} = 1. \end{aligned}$$

Rearranging the difference between these derivatives somewhat gives:

$$\frac{\partial G_i}{\partial Y_i^c} - \frac{\partial G_i}{\partial Y_j^c} + \frac{\partial G_j}{\partial Y_j^c} - \frac{\partial G_j}{\partial Y_i^c} = -2.$$

Symmetry between children requires equal treatment which implies:

$$\frac{\partial G_i}{\partial Y_i^c} - \frac{\partial G_i}{\partial Y_j^c} = \frac{\partial G_j}{\partial Y_j^c} - \frac{\partial G_j}{\partial Y_i^c} = -1. \tag{10}$$

This is the derivative condition. In other words, the derivative condition (10) is equivalent to the consumption possibilities derivative (9).

For the empirical analysis this implies that testing (9) is equivalent to testing (10). Suppose that the econometric specification includes fixed family effects. We will then for family h have that

$$\hat{G}_{hi} - \hat{G}_{hj} = \ldots \hat{\beta}_{Y^c}^{\text{within}}(Y_{hi}^c - Y_{hj}^c) \ldots \qquad i, j = 1 \ldots n, j \neq i. \tag{11}$$

If the outcome of the test is that we can reject that $\beta_{Y^c}^{\text{within}} = -1$, the derivative condition (10) is also rejected.