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THREE ESSAYS IN LABOR ECONOMICS AND
ECONOMICS OF AGING

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Olena Y. Nizalova

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THREE ESSAYS IN LABOR ECONOMICS AND ECONOMICS OF AGING

By

Olena Y. Nizalova

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the Degree of

DOCTOR OF PHILOSOPHY

Department of Economics

2006

ABSTRACT

THREE ESSAYS IN LABOR ECONOMICS AND ECONOMICS OF AGING

By

Olena Y. Nizalova

First chapter analyzes the relationship between labor markets and supply of informal care focusing on the wage elasticities. Unlike most of the previous research estimating wage elasticities of informal care supply, this study recognizes that the wage rate may be measured with error or may be correlated with omitted variables, and thus has to be treated as endogenous. Using data from the Health and Retirement study, this paper exploits instrumental variable techniques to show that after controlling for wage endogeneity wage elasticity of informal care supply is negative and larger in magnitude than has been found before. Additional findings suggest that informal care supply is more elastic among individuals with siblings, and that it differs by the type of care provided to elderly parents.

Second chapter investigates whether the choice of the net versus gross measure of monetary transfers from adult children to their elderly parents can explain the differences in the estimates of the wage effect on money transfers found in earlier studies. It carefully documents the transfer pattern and points to the limitations of

the OLS specification in the analysis of net transfers. A three-part model consisting of a multinomial probit and two OLS equations for negative and positive net transfers is offered as a better alternative for the analysis of net transfers. The results from estimating this model shows that wage effects differ for net recipients and net givers with the strongest effects observed at the extensive margin. These features provide a useful guidline for future theoretical research. One of the possible theoretical models that posesses such features is outlined in this paper.

Third chpater is devoted to the study of minimum wages exposure to which at young ages may lead to longer-run effects. Among the possible adverse longer-run effects are decreased labor market experience and accumulation of tenure, lower current labor supply because of lower wages, and diminished training and skill acquisition. Beneficial longer-run effects could arise if minimum wages increase skill acquisition, or if short-term wage increases are long-lasting. We estimate the longer-run effects of minimum wages by using information on the minimum wage history that workers have faced since potentially entering the labor market. The evidence indicates that even as individuals reach their late 20's, they work less and earn less the longer they were exposed to a higher minimum wage, especially as a teenager. The adverse longer-run effects of facing high minimum wages as a teenager are stronger for blacks. From a policy perspective, these longer-run effects of minimum wages are likely more significant than the contemporaneous effects of minimum wages on youths that are the focus of most research and policy debate.

Acknowledgements

I would like to express my gratitude to my thesis advisor, Jeff E. Biddle for his help, guidance, and unlimited patience. I am deeply indebted to Steven J. Haider for his advice, financial support, and encouragement that greatly accelerated my movement towards the degree completion. I would also like to thank John Strauss for his supervision and support.

Special thanks are given to David Neumark for providing me with the financial support and initial access to the Health and Retirement Study restricted data. This made possible my research on the wage elasticity of informal care supply that resulted in the first chapter of my dissertation.

I also thank my thesis committee members, John Goddeeris, John Giles, and Dale L. Belman for their help during my studies and for their careful reading of the manuscript.

I thank everybody at the Economics Department: the professors, the secretaries, and my officemates. It was very pleasant to be a graduate student here.

Last but not least I thank my American friends, Karen and David Glossops and Zeynep Altinsel, my mother, and my mother-in-law for their continuous love and support. I dedicate this work to my husband Denys and my children Anastasiya and Nikolai for their love, understanding, and patience.

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Chapter 1. The Wage Elasticity of Informal Care Supply: Evidence from the Health and Retirement Study

Introduction

For the past two decades the policy debate on issues related to the aging population has been growing at unprecedented rates throughout the world. On the one hand, policy measures suggested in the debate include removal of the disincentives for labor force participation for individuals in their 60s (CBO, 2004; U.S. DHHS, 1997; Apfel, 2004). This would effectively raise the wage rate faced by the targeted group, as in the case of the elimination of the Social Security earnings test for those older than 65. On the other hand, the role of informal caregiving is emphasized as a means to "keep many individuals at home who would otherwise require expensive institutional care" (U.S. DHHS, 1997, p.6). Policies targeting these two objectives may turn out to conflict with each other: higher wages may decrease hours devoted to informal care for elderly parents, while policies encouraging informal care may lead to fewer working hours. Research on the effects of the Social Security earnings test is mostly focused on labor supply and claiming behavior of the affected group (Haider and Loughran, 2005; Baker and Benjamin, 1999; Burtless and Moffitt, 1984; Friedberg, 2000; Gruber and Orszag (1999)) with little attention paid to the potential interaction between incentives for paid employment and caregiving choices. This interaction may have adverse implications for the well-being of the oldest old, given that the prevalence of caregiving is highest among individuals in their late mid-life.[1]

Central to the analysis of this interaction is the concept of the wage elasticity. The labor supply literature suggests a positive wage elasticity of labor supply for females and close to zero wage elasticity for males. Therefore, if the wage elasticity of informal care supply is close to zero (as has been found in earlier studies), higher wages would lead to more labor supplied with negligible effects on the provision of informal care. In this case one might hope that benefits from increased labor supply will not be offset along other dimensions. If, on the contrary, the wage elasticity of informal care supply is relatively large and negative, one should be more cautious when evaluating the effects of increased labor supply among individuals in their late mid-life. A substantial negative wage elasticity of informal care supply would mean that along with increasing labor supply younger generations would cut back on the hours of informal care to their elderly parents. This may potentially lead to more people turning to the government in their quest for help with covering formal care costs.

Previous studies that have examined the effect of wages on transfers to elderly parents have emphasized the substitution between time and monetary modes of informal care (Sloan et al., 2002; Zissimopoulos, 2001; Ioannides and Kan, 1999; Sloan et al., 1997; Couch et al., 1999). The estimated effect of wages on informal care hours is negative (as theory predicts), but very small in magnitude and usually not

[1]McGarry (2003a) cites that according to the Commonwealth Fund's (1999) report, the fraction of women providing care is highest among the 45-64 age group (13 percent compared to 10 percent for women of 30-44 years old and 7 percent of women 65 years old or older).

significant statistically. At the same time the estimated wage effect on money transfers is positive and quite significant both statistically and economically. However, these studies have been limited in their ability to address the issue of possible bias in the estimates of the wage elasticities due to a likely measurement error and omitted variables concerns.

The goal of this paper is to study the wage elasticity of informal care supply, directly adressing the issues of omitted variables and measurement error in the time allocation equations. Informal care supply is defined as annual time spent helping elderly parents with basic personal needs as well as household chores, errands, and transportation. This paper uses a unique opportunity of access to restricted geographic identifiers for the Health and Retirement Study respondents to instrument hourly wages with the industry structure in the state of residence.[2]

The main finding of the paper suggests that the wage elasticity of informal care supply is negative and substantially larger in magnitude than previously estimated. For example, according to the current estimates a 33 percent increase in wages associated with the elimination of the Social Security earnings test is translated into a 56 percent decrease in average informal care provided by males and 92 percent decrease in average informal care provided by females. Additional findings include the following: (i) the wage elasticity of informal care supply is larger for people who have at least one sibling; (ii) the wage elasticity of help with personal needs is smaller in magnitude than the wage elasticity of time spent helping parents with chores, errands, transportation, etc.; (iii) estimates of the wage elasticity of net monetary transfers do not support the hypothesis that individuals replace time transfers to parents with monetary transfers as their wages go up.

The paper is organized in the following way. Section 2 presents some background

[2]Initial access to the restricted data have been provided through the project under the supervision of David Neumark and Elizabeth Powers. The author is thankful to the ISR Data Enclave and Steven Haider for their help with continued access to the data.

information, followed by the theoretical model and econometric specification used in the empirical analysis in Section 3. Data are described in Section 4. Section 5 presents the empirical findings. Finally, Section 6 offers a discussion of possible extensions to the main analysis of informal care supply.

Background

Thus far, studies of informal care supply to elderly parents have been mostly descriptive even though this area is receiving growing attention in various social science disciplines. In economics a considerable body of research has been developed on the motivation behind intergenerational transfers (mainly financial) and some on residential decisions, while sociologists, for instance, have focused on identifying the determinants of the incidence and intensity of informal care supply with little attention paid to economic variables.

A few papers that consider the effect of changes in care giver's wages on time transfers to elderly parents studied the trade-off between time and money dimensions of help to elderly parents using cross-sectional data from the Health and Retirement Study, the Panel Study of Income Dynamics, and the National Long-Term Care Survey of Informal Caregivers (Sloan et al., 2002; Zissimopoulos, 2001; Ioannides and Kan, 1999; Sloan et al., 1997; Couch et al., 1999). The results from these studies suggest that the effect of wages on informal care is not significant, either economically or statistically. The estimates of the wage elasticity range from -0.78 to 0.07 (Table 1). However these estimates may be subject to attenuation and omitted variable bias. Attenuation bias and upward bias from omitted variables may explain small magnitudes of the wage elasticities estimated earlier.

In contrast to the scarcity of research on the wage elasticity of informal care supply, there is a large literature that has been devoted to the study of the wage elasticity of labor supply. There exist several generations of research in this area and they are extensively reviewed by Pencavel (1986, 1998, 2002), Killingsworth and Heckman (1986), Mroz (1987), and Blundell and MaCurdy (1999) in an attempt to reconcile the variation in the estimates found and determine the sources of these differences. This body of research provides some guidance to the current analysis. In particular, Mroz's (1987) finding that controlling for the self-selection does not have a substantial impact on the estimates of the wage elasticity of labor supply once the labor market experience is not treated as an exogenous determinant of wages is used to justify the focus of the current study on the working population. Furthermore, Blundell and MaCurdy's (1999) review provides a basis for the discussion of the life-cycle considerations in the context of informal care supply to elderly parents.

Theory and Econometric Specification

Model

The theoretical framework underlying the empirical analysis is based on the simple one-period model of time allocation that involves two individuals: the care recipient (R) and the care giver (G). Care recipient refers to an elderly parent and care giver to his/her adult child. The model can generalize to any pair of individuals with one not working and in need of care and the other working and being potentially able to provide care to the former.

Assuming altruistic motives, the care giver gets utility from consumption (X_G), leisure (l_G), and utility of the care recipient (U^R). The care giver's time endowment is allocated between care (t_g), work (t_w), and leisure (l_G). The non-labor income (I_G) and labor income ($w * t_w$) of the care giver are spent on consumption goods (X_G) and a monetary transfer to the care recipient (D). Monetary transfers can be positive as well as negative.[3] Utility of the care recipient, in turn, depends on own consumption

[3]Monetary transfers incorporate flows of money in both directions but time transfer in the model

(X_R) and care (Z_R), which is produced with the only input of time of other people (market-purchased time t_m or t_g provided by the care giver), and is subject to the budget constraint.

So, the care giver solves the following problem:

$$U^G = U^G(X_G, l_G, U^R; S_G), \tag{1}$$

subject to:

$$X_G = I_G + wt_w - D, \tag{2}$$

$$X_R + p_t t_m = I_R + D, \tag{3}$$

$$t_g + t_w + l_G = T \tag{4}$$

$$U^R = U^R(X_R, Z_R; S_R), \tag{5}$$

$$Z_R = \gamma t_m + Z^R(t_g; \delta), \tag{6}$$

where w is the care giver's hourly wage rate, p_t = price of market-purchased time t_m, I_R = R's non-labor income, δ = parameters of the care production function,[4] S_G and S_R are vectors of taste shifters.

Substituting all of the constraints into the care giver's utility function results in

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flows only from the care giver to the care recipient for two reasons. First of all, the model is developed to describe the relationship between a care giver and a care recipient. The latter being in need of care from other people, unable of producing that care for him/herself, and therefore unable to provide any care for others. Secondly, the empirical evidence suggests that the prevalence and magnitude of time flow from elderly parents to adult children is quite low. Figure A1 in Appendix A shows that it is virtually zero after the age 80. On the contrary, as the description of the data will show later, financial transfers are likely to flow in both directions, no matter how old the involved parties are.

[4] It is assumed that production function is linear in market-purchased help: formal care can be purchased in increments by hiring different people to perform different tasks, so that it is not subject to diminishing returns.

the following optimization problem:[5]

$$\max_{t_m, t_g, t_w, D} U^G = U^G(I_G + wt_w - D, T - t_g - t_w, \tag{7}$$

$$U^R(I_R + D - p_t t_m, Z^R(t_m, t_g; \delta); S_R); S_G),$$

$$t_w > 0, t_g \geq 0, t_m \geq 0 \tag{8}$$

Figure 1 summarizes the results of the analysis of Kuhn-Tucker conditions for the optimization problem of the care giver.[6] The horizontal axis measures time spent in caregiving, while the vertical axis shows marginal product of time in the production of care. Line $MP(t_g)$ reflects diminishing returns in the production of care by the care giver. Line 1 corresponds to the marginal product of market-purchased time in care production $MP(t_m)$.

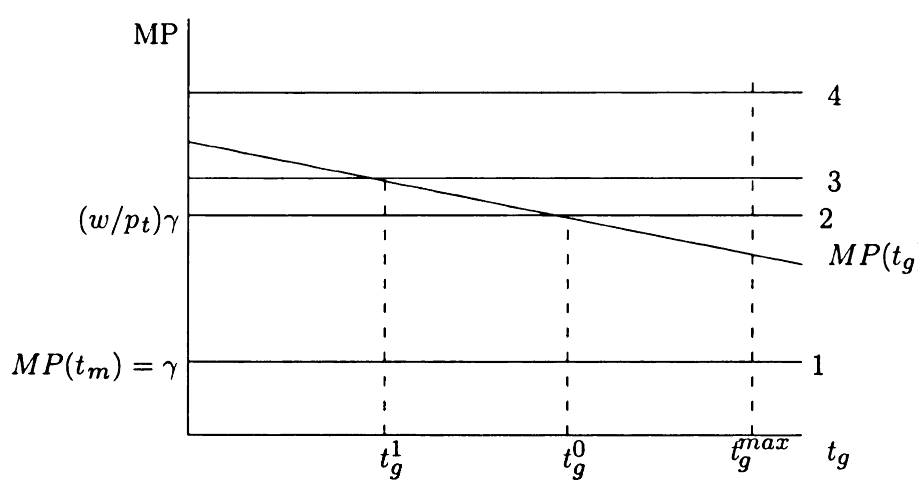

Figure 1: Graphical Representation of the Conditions for Interior Solution (Time in Caregiving and Market-Purchased Help)

No informal care will be provided if the productivity of time in caregiving eval-

[5]Standard assumptions include: $U_X^G > 0$, $U_{XX}^G < 0$, $U_l^G > 0$, $U_{ll}^G < 0$, $U_U^G > 0$, $U_{UU}^G < 0$, $U_X^R > 0$, $U_{XX}^R < 0$, $U_Z^R > 0$, $U_{ZZ}^R < 0$, $\gamma > 0$, $Z_{tg}^R > 0$, $Z_{tgtg}^R < 0$.

[6]Appendix A contains full set of Kuhn-Tucker conditions and intermediate results from their analysis.

uated at zero is smaller than $(w/p_t)\gamma$, the price-adjusted productivity of market-purchased help (line 4 in Figure 1). Likewise, no help will be purchased from the market if the marginal product line of time in caregiving will be located above the $(w/p_t)\gamma$ line over the whole range $(0, t_g^{max})$. In the figure this situation is possible if, for example, the wage rate is equal to the price of informal care (line 1).

An interior solution implies equalities in all four first-order conditions. As a result, the quantity of informal care provided will be determined from the equality of the marginal product of time in caregiving with the price adjusted marginal product of market-purchased care. Graphically, this suggests that the $MP(t_g)$ line and the price-adjusted $MP(t_m)$ should intersect in the area $(0, t_g^{max})$. The point of intersection would determine the optimal amount of care provided. Note that in this case the care production decision does not include any parameters of utility functions.

As can be seen from Figure 1, if the wage rate goes up (meaning that the price-adjusted productivity line (2) moves up, to line 3, for example), the new optimal caregiving time decreases from t_g^0 to t_g^1. If the wage rate decreases or the price of formal care increases, optimal caregiving time will increase. It can also be shown that when formal care is positive and the optimal level of informal care occurs at the corner, the probability of providing informal care increases if the price of formal care goes up and decreases if the wage rate goes up. Similarly, in the case when the optimal caregiving time is positive and the optimal formal care is zero the probability of an interior solution increases if the wage rate goes up and decreases if the price of formal care increases. In short, the analysis of the model shows that as formal care becomes relatively more expensive, the probability of positive informal care provision and its intensity increases, while the probability of positive formal care and its intensity decreases.

The solution to the optimization problem discussed above will involve labor supply, informal care supply, purchased help and net monetary transfer as functions of the wage rate, R's and G's non-labor income, price of market-purchased help, parameters of the household production function, and R's and G's taste parameters:

$$t_w = f(w, p_t, I_G, I_R, \delta, \gamma, S_R, S_G) \tag{9}$$

$$D = f(w, p_t, I_G, I_R, \delta, \gamma, S_R, S_G) \tag{10}$$

$$t_m = f(w, p_t, I_G, I_R, \delta, \gamma, S_R, S_G) \tag{11}$$

$$t_g = f(w, p_t, I_G, I_R, \delta, \gamma, S_R, S_G) \tag{12}$$

Substituting optimal working time, leisure and caregiving time into the time constraint and differentiating budget constraint with respect to wages allows for the comparison of the relative magnitudes of the effects:

$$\frac{dt_g}{dw} = -\frac{dt_w}{dw} - \frac{dl_G}{dw} \tag{13}$$

Under the standard assumption that leisure is a normal good, at least for a certain range of wages and non-labor income, this expression shows that the absolute value of the effect of the wage on the time in informal caregiving should be smaller than the absolute value of the effect of the wage on working time.

Unfortunately, a similar conclusion cannot be drawn on the effects of the price of formal care on working time and time in caregiving.

$$\frac{dt_g}{dp_t} = -\frac{dt_w}{dp_t} - \frac{dl_G}{dp_t} \tag{14}$$

In fact, even the sign of the effect of the price of formal care on the working time is ambiguous. If the effect of p_t on t_w is positive, then the magnitude of the effect on leisure must be larger than that on working time to allow for the positive effect on caregiving. This can happen when the care giver's initial involvement in care for the elderly parents as well as his/her hours of work are relatively low, so that with an increase in the price of formal care it is possible to increase the time in caregiving as well as working hours. Nothing can be said, though, about the relative magnitude of the effect of p_t on t_w and t_g in this case. On the other hand, if the effect of the price on working time is negative, then the magnitude of the effect of p_t on labor supply should be smaller than the magnitude of its effect on informal care supply. It is plausible to think that first the care giver would draw on his/her free time to increase the supply of informal care in response to increase in the price of formal care, and only when the time constraint is pushed to a limit would the working time start decreasing. This explains the smaller in magnitude effect of the price of formal care on working time when compared to its effect on the time in caregiving. So, the switch from the positive effect of the price of formal care on the working time to the negative one is likely to happen when the care giver's leisure is at its lower limit, probably at a limit necessary for the normal functioning of the human body.

Estimation Strategy and Econometric Specification

The theory suggests a four-equation econometric model. However, the data used in the empirical analysis do not provide any information on market-purchased help by care recipients or prices they face, thus reducing the system to three equations. Even though estimating the system equation by equation may produce less efficient estimates of the parameters, the choice is restricted by the technical difficulties of simultaneous estimation of linear and Tobit equations with instrumental variables.

The three equations to be estimated are the following:

$$t^*_{gi} = \alpha_g log\, w_i + X_i\beta_g + u_{gi} \tag{15}$$

$$t_{gi} = max(0, t^*_{gi})$$

$$t_{wi} = \alpha_w log\, w_i + X_i\beta_w + u_{wi} \tag{16}$$

$$D_i = \alpha_D log\, w_i + X_i\beta_D + u_{Di} \quad (17)$$

where t_{gi} is annual hours of informal care for elderly parents, t_{wi} = annual working hours, D_i = annual net money transfer to elderly parents, w_i = individual's hourly wage rate, and X_i is a vector of controls for individual i, discussed later.

The wage effect on informal care supply is estimated using the Tobit model, as in most of the studies on informal care supply, to incorporate corner solutions into the estimation. A linear-log specification is chosen for the labor supply[7] and money transfer equations.[8] All three equations are estimated separately for men and women.

Econometric Issues with the Wage Effect Estimation

In the theoretical model presented above, wage rates are assumed to be exogenous. However, this is unlikely to be true empirically. Many of the factors that enter the supply/demand functions (Equations 9-12) are not available in the data and are potentially correlated with the wage rate. For example, information on some of the important determinants of the informal care supply, such as the price of formal care as well as other unobserved personality traits (responsibility, respect for seniors, etc.) may not be available to researchers.

Lack of the information on the price of formal care and characteristics of the care production function is likely to lead to the problems associated with omitted variables. The estimates of the wage elasticity of informal care supply would not be biased if the assumption of zero correlation between wages and omitted variables were plausible. However, in the current setting this is a very restrictive assumption. For example, the price of formal care is likely to be higher for people living in high-wage areas, and failure to control for this variable would result in an upward biased estimate of the wage effect on informal caregiving time. Also, the productivity of an individual in

[7] Mroz (1987) used a similar specification to test different specification issues and their effect on labor supply elasticity estimates.

[8] Even though the distribution of net monetary transfers has most of its mass on zero, the OLS still provides best linear prediction and allows for consistent estimates of parameters around mean.

the informal care setting may be positively correlated with his/her productivity on the job, and thus omitting these controls would also result in an upward bias. So, omitting the factors positively correlated with the wage rate may lead to an upward bias in the estimates of the wage effect on different time uses.

Another important factor is the productivity of the care giver in caregiving activities. Failure to control for productivity in caregiving may have ambiguous implications for the estimates. On the one hand, the productivity in caregiving may be positively correlated with productivity on the job leading to an upward bias in the wage effect estimate. On the other hand, if specialization takes place, the productivity in caregiving may be negatively correlated with the productivity on the job, leading to a downward bias in the wage effect estimates. In practice, it is likely that some individuals are more productive in everything or their job requires similar characteristics as caregiving tasks do, and some individuals are highly specialized being productive either on the job or at home, but not both. Thus it is difficult to infer the average effect in the population.

The endogeneity problem in the caregiving analysis is similar to that found in estimating standard labor supply elasticities. Over the last few decades, a number of attempts have been undertaken to use different instrumental variables to account for wage endogeneity in the labor supply setting. Pencavel (1986) mentions sets of instruments used in the early literature which have since been disqualified (Mroz, 1987). These included such variables as own education, experience, and the reported hourly wage rate used to instrument the wage rate calculated from earnings and hours. Somewhat arguable variables are education of parents and their socio-economic status, lagged values of the wage rate, urban residence indicator, and polynomials in age and education. Sets of aggregated information such as unemployment rates in the region of residence, cohort average schooling, other group level variables, and polynomials in regional and time trends have also been used extensively. Since the aggregate instruments (such as regional unemployment rates) may be weak in explaining the variation in individual wages, this study offers a careful examination of the first stage results. In addition, testing the approach in the labor supply setting provides some guidance for the use of the instruments in the case of informal care supply.

This study uses three main sets of instruments: (IV-1) state unemployment rate and state industry structure described by the percentages of the working population employed in each of the manufacturing, service, and government sectors[9], (IV-2) same as in (i) but manufacturing is divided into three sectors – trade-impacted concentrated industries, competitive industries, and other durables industries[10] (as in Borjas and Ramey, 1995), and (IV-3) same as in (IV-2) adding interactions with the education variable to allow for the differential impact on wages of individuals with different levels of education. In addition, a third order polynomial in age and education (IV-4)[11] is used for the purpose of comparison with the suggested sets of the state level instruments and findings from the earlier studies on labor supply.

Data and Descriptive Analysis

The main analysis in this paper is implemented using the Health and Retirement Study (HRS) data from the 1998 wave. The analysis of labor supply is supplemented by the data from the Current Population Survey March files (CPS) for the period 1991-2001.

The Health and Retirement Study is a national longitudinal survey representing a

[9]Similar instrumental variables have been chosen by Senesky (2003) and Bacolod (2003).

[10]Trade-impacted concentrated industries include: Stone, Clay and Glass Products; Primary Metal Industries; Industrial Machinery and Equipment; Motor Vehicles and Equipment; Other Transportation Equipment. Trade-impacted competitive industries include Apparel and Other Textile Product Industries. Other durables industries include the rest of the manufacturing sector: Lumber and Wood Products; Furniture and Fixtures; Fabricated Metal Products; Electronic and Other Electric Equipment; Instruments and Related Products; Miscellaneous Manufacturing; and Ordinance.

[11]Even though Murphy and Welch (1990) find the fourth-order polynomial to be a better approximation, third-order polynomial is used here to follow Mroz (1987), who finds that the third order polynomial as an instrument set is not rejected in favor of higher order polynomials.

rich source of information on the lives of older Americans, including their health and economic status. It also includes extensive data on intergenerational transfers and characteristics of parents and children. The Study consists of people born in 1947 and earlier, totaling to more than 21000 respondents. The 1998 wave provides the largest set of cross-sectional information available from the HRS.[12]

The analysis focuses on working individuals[13] who can potentially provide time to their parents or parents-in-law (both are referred to as "parents" throughout the paper). In the present study parents (including in-laws) are treated as a group, similar to Ioannides and Kan (2000).

Focusing on only working individuals may raise the issue of selectivity bias, especially in the labor supply context. However, Mroz (1987) shows that even for the sample of married women, the population for which the selectivity issue has always been considered important, selection does not have a significant impact on the estimates of the wage elasticity of labor supply as long as labor market experience is not treated as an exogenous determinant of wages (i.e., as an instrument).

Dependent Variables

The dependent variables used in the main analysis are annual working hours, annual hours spent helping parents with basic needs and household chores, and net annual monetary transfer to parents. Annual working time is the product of usual weekly hours of work and number of weeks worked across all jobs.

The questions concerning intergenerational transfers are asked as follows:

1. *Now about help to and from parents...*

[12]HRS started in 1992 with the cohort of individuals born in 1931-1941, and AHEAD started in 1993 with the cohort of individuals born in 1923 and earlier. The survey of those cohorts continued every two years till 1998 when both surveys were combined into one and two other cohorts, Children of Depression Age (CODA) cohort born in 1924-1930 and War Babies (WB) cohort born in 1942-1947, were added.

[13]Workers include those who report positive working hours and do not report full retirement in 1998.

Not counting any shared housing or shared food, did you give financial help to your parents or parents-in-law amounting to $500 or more (since Previous Wave Interview Month-Year/in the last two years)?

2. *How about another kind of help: Have you spent 100 or more hours in the past 2 years helping your parent(s) (or parents-in-law) with basic personal needs like dressing, eating, and bathing? [with other things such as household chores, errands, transportation, etc.?]*

If the answer is "yes" to these questions, then the respondent is asked the amount of the transfer provided. Overall, the effective annual time spent helping parents is defined here as the sum of the time spent helping parents with basic personal needs and time spent helping parents with household chores. Although the question is asked so that the dependent variable should be left-censored at 50 hours a year, in practice, time use smaller than that is reported as well.[14] Thus, practical lower limit on time use is zero. Results are also tested for sensitivity with 50 hours as a lower limit.

Explanatory Variables

The hourly wage rate is taken directly if reported on an hourly basis. Otherwise, it is constructed by dividing earnings from the main job over a certain time period (year, month, two weeks, week) by the total reported working hours in the main job over that period.[15]

The set of individual controls in all specifications include the following: age, education, current non-wage income,[16] marital status (sample size does not allow separate treatment of married versus single individuals), non-white, hispanics, region dummies (Blundell, 1999), number of young children (0-6 years old), number of 6-18 year-old

[14]A considerable number of individuals reported time in caregiving smaller than 50 hours (112 out of 387 cases with positive care hours among males and 68 out of 495 cases among females).

[15]50% of males and 56% of females report being paid hourly.

[16]Non-wage income is defined as capital income in the HRS sample and as a difference between the total household income and individual's earnings for the CPS sample.

children (Mroz, 1987), and number of siblings.[17] The last three variables aim to control for other possible time demands. Mulligan (1995) suggests the inclusion of health measures since disutility from work may increase as people age. Therefore, health measures may become significant determinants of labor supply, as well as of informal care supply, for older workers. However, health measures are excluded from the current analysis due to a potential endogeneity and a lack of plausible instruments.

In addition to the individual characteristics, all of the specifications in the main analysis on the sample of potential caregivers include characteristics of the parents. These characteristics refer to the set of all living parents and include the number of surviving parents (maximum four), the ratio of the number of mothers to the number of living parents, the age of the oldest parent, the indicators if at least one of the parents (i) is single, (ii) has memory related disease, and (iii) is identified by the respondent as being financially worse off or better off than the respondent.

Sample Description

The sample is limited to working, not self-employed[18], age-eligible[19] individuals who have at least one parent or parent-in-law alive in 1998.[20] Individuals retired in 1998 are excluded from the study. The resulting sample consists of 1434 males and 1358 females who have complete data on all of the variables of interest. See Appendix Table A1 for the sample construction details.

Table 2 presents a description of the sample. All financial variables are in 2002 dollars and non-labor income is in thousands of dollars.[21] As can be seen from Panel A, the mean annual working hours for male workers is on average 400 hours higher

[17]For married individuals this includes both siblings and siblings-in-law.

[18]Self-employed are excluded to follow the labor supply literature and thus allow for the comparision of the extimated labor supply elasticities to the earlier estimates.

[19]Age-eligible individuals are defined by the HRS as those who were born in 1947 or earlier.

[20]AHEAD cohort is excluded from the sample as there are very few working individuals with living parents in this age group.

[21]All financial variables are converted to 2002 dollars for consistency with the 1991-2001 CPS sample used for the evaluation of the instrument sets in the labor supply setting.

than for females (2290 hours vs. 1897 hours). Unconditional (on caregiving status) caregiving hours average at 44 hours per year for males and 91 hours per year for females (2% and 5% of the average annual working time respectively). The difference widens to about 100 hours between males and females when conditioning on actual caregiving status (163 vs. 250 hours). Differentiating by the type of caregiving suggests that the difference between men and women involved in caregiving is greater in time devoted to help with personal needs than in time devoted to help with chores, transportation, errands, etc.[22] Net money transfer is negative indicating that money flows into the care givers' households from their parents.

As Panel B in Table 2 reveals, males receive on average much higher hourly wages than females (the difference is about 6 dollars). They are a bit older, slightly more educated, and have much higher non-labor income (about 6 thousand dollars more) defined as capital income. Males are also more likely to be married, have more siblings and more living parents, and their parents are younger than those of females.

Figure 2 shows a non-parametric relationship between labor supply, informal care supply and wages for males and females. As can be seen, the labor supply schedule for both males and females has a small positive slope. As to the informal care supply, the relationship with wages seems to be negative, especially at the lower end of the wage distribution, for both men and women. However, the pattern is more pronounced for females.

As mentioned earlier, it is important to know whether parents are compensated in some way as the hours of care they receive directly from their children decreases. Earlier research showed significant positive wage elasticities of monetary transfers to parents. However, the measure of the monetary transfer used in these studies referred only to out-transfer from adult children to parents. It did not take into account possible in-transfers from parents. As the theoretical model suggested, money

[22]This fact may simply reflect the higher life expectancy for women and preferences for the same-sex helper with the basic needs.

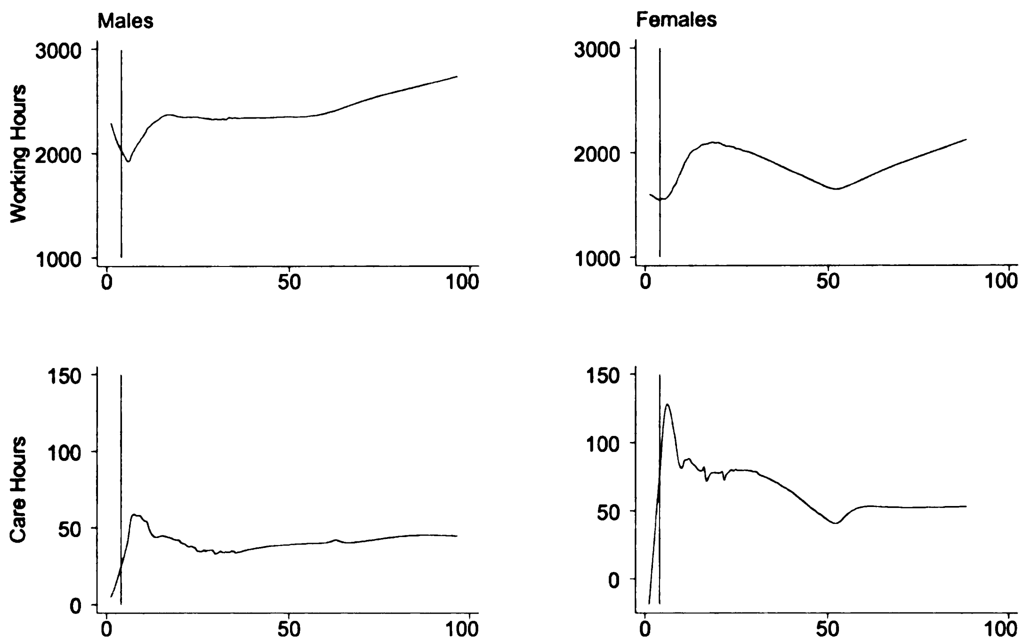

Figure 2: Time Allocation by Hourly Wage Rate

transfers can be both positive and negative; thus in empirical work, the more relevant measure would be net monetary transfer. Figure 3 shows the importance of the distinction between net and gross money transfers. The upper two graphs show money transfer schedules using only money transfered to parents by children. For both men and women, the out money transfer schedule is upward sloping. However, the two graphs at the bottom show that the reliance on the one-way money transfer may be misleading as the net money transfer exhibits, if anything, a negative slope for both men and women. It slopes upward only at the high end of the wage distribution and is most likely driven by outliers, since there are very few people in the sample with wage rates higher than 50 dollars per hour.[23]

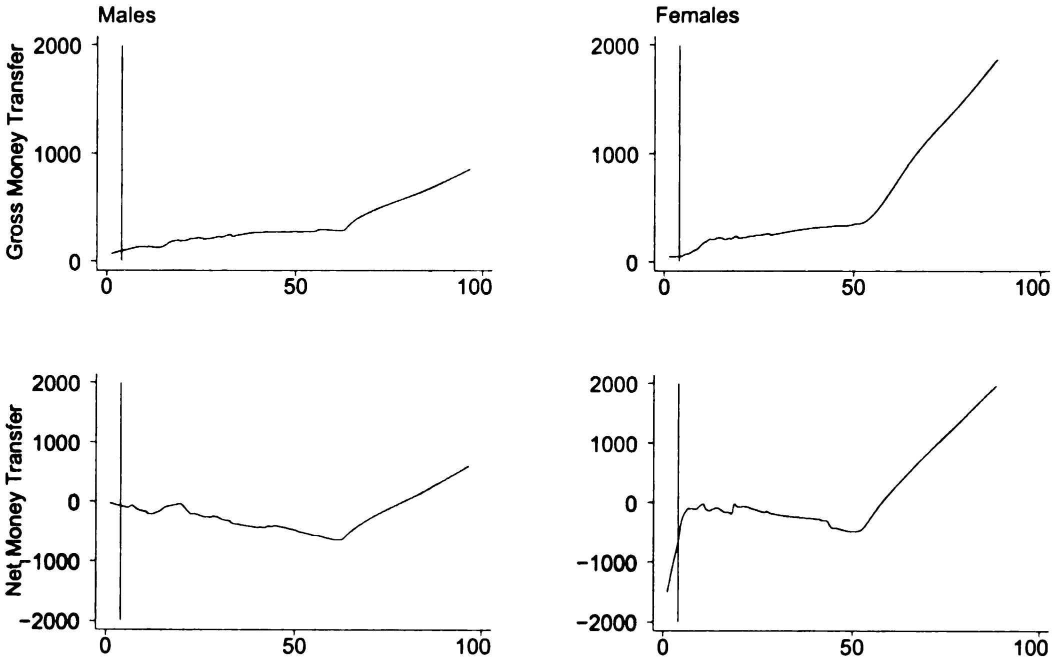

Figure 3: Gross and Net Monetary Transfer by Hourly Wage Rate

[23]Only 3% of males and 0.6% of females have hourly wage rates in 2002 dollars higher than \$50.

Empirical Results

Labor Supply

Tables 3 and 4 show the results from the OLS and the TSLS instrumental variable estimation for both males and females.[24] As can be seen, almost all estimated parameters, except for the wage effects, are relatively consistent across specifications in terms of statistical significance and sign. The effect of non-labor income is negative, as expected, although not always statistically significant. The effect of age is negative and significant, and it is twice as big for men as it is for women. Being married is negatively associated with working hours among working women, while no significant effect of marital status is found for men. More interesting effects in the context of labor supply are the effects of parental characteristics (something that has rarely been included in the labor supply equations), and these effects are different between men and women. Males who have at least one single parent are working fewer hours, while those who have older parents are working more (holding everything else constant). On the contrary, females with at least one single parent work significantly more and the maximum age of parents has negative effect on women's labor supply. Having at least one parent with memory-related disease has significant negative effect on males' labor supply with no effect on females' labor supply. The wage effect on labor supply is positive in every case both for males and females and increases in magnitude after instrumenting. However, in the latter case it becomes insignificant for females.[25]

[24]Appendix Tables A2-A4 replicate the analysis of labor supply for samples of all working individuals from the HRS-1998 (including those with no living parents) and from the Current Population Survey, 1992-2000. The estimates of the wage elasticity of labor supply and the behavior of the instrumental variables are quite similar across different samples. Naturally, the performance of the instrumental variables is improved in larger samples.

[25]The effect of wages on annual working hours for females is found to be largest in magnitude and statistical significance using the IV-2 set of instruments. However, the first stage statistics in this case are very low (F-statistics less than 2) thus questioning the credibility of this particular estimate.

Care Supply

Tables 6 and 8[26] show the results from the Tobit and instrumental variable Tobit estimation of the linear-log care supply equations. Very few of the conventional individual controls are significant in those equations. Being married has significant negative effect on care provided by females. Having siblings is negatively associated with the care supply, and this effect is larger in magnitude for females. Both males and females provide less care if they have young children. Having more living parents has positive effects in both equations; those with parents with memory-related disease, older parents and poorer parents also provide more care. The ratio of mothers among living parents is positively associated with the amount of care provided, however the effect on women's care supply is much larger than that on men's. The effect of wages on the care supply is negative and it is increasing in magnitude after instrumenting. The magnitude of the wage elasticity of informal care supply is smaller for men than it is for women, similar to the non-parametric function that was shown earlier.

Net Money Transfer

Results from the money transfer equations are presented in Tables 8-9. They show almost no statistically significant determinant of money transfer besides the respondent's perception about the financial situation of her parents, in the equation for females. For males, being married has significant negative effect and being non-white has a significant positive effect on net money transfer. Having young children has a positive effect, while having older children has a negative effect. Among parental characteristics only those describing perceived financial situation are statistically significant.[27]

[26]Tables 7 and 9 show the results using ordinary least squares.

[27]To circumvent the problem with the unusual distribution of the outcome (80% of the mass in the net monetary transfer distribution is concentrated on zero), multinomial logit model has been estimated on three outcomes: being net receiver, being net giver, or none of the above. The results are similar qualitatively to the OLS estimates. However, the important assumption underlying the multinomial logit model, Independence of Irrelevant Alternatives (IIA), is violated automatically in the case of omitted variables even if they are not correlated with the regressors. This problem

To conclude, the analysis of the net monetary transfer between adult children and elderly parents does not provide evidence for either rejection or acceptance of the hypothesis of substitution between time and monetary transfers, suggesting the existence of the more significant problem than just the irregularities with the distribution.[28] One of the potential explanations may be the possibility that the measure of the net monetary transfer over the certain short period of time does not allow for that substitution effect to take place. For example, what if the decrease in the informal care provided today is balanced with the decrease in the end-of-life transfer from parents? Indeed, Brown (2004) finds that parents on average bequeathe more to children who are currently providing informal care or who are expected to become care givers in the future.

Interpreting the Estimates

The estimates of the wage elasticities are summarized in Table 11.[29] Columns (1) and (2) allow for the distinction between the wage elasticities of informal care supply evaluated at the unconditional and conditional means. Column (3) shows the estimated wage elasticities of labor supply, and column (4) provides the estimated wage elasticities of the net monetary transfer.

Estimates of the wage elasticity of informal care supply at the conditional mean of care hours are much smaller (as would be expected for the elasticity at the intensive margin) than those evaluated at the unconditional mean of informal care hours. They are estimated to be less than one for males in every case, but are close to unity for women. Unconditional estimates range from -1.7 to -2 for men and from -2.8 to -3 for

is not solved by the use of instrumental variables. And the results from the instrumental variable multinomial logit regression prove that: the coefficients are noisy, they flip signs and show significant variation in magnitudes between the specifications with different instrumental variable sets.

[28]However, if one lets the data speak for itself, endogeneity is rejected in the net monetary transfer equation for both males and females. In this case the hypothesis of substitution between time and money transfers should be rejected as the estimates of the wage effect are quite small in magnitude and not statistically different from zero.

[29]Appendix Table A4 shows robustness of the estimates of the wage elasticity of labor supply and performance of the instruments' sets to changes in specification, sample size, and the data used.

women. These estimates are considerably larger in absolute value than the estimates from the previous literature which range from 0.07 to -0.78 (Table 1).

It is instructive to think about the magnitudes of the estimated elasticities. Considering the results from the last column (IV-3), for example, suggests that a 10% increase in wages (2.1 for men and 1.5 for women in 2002 dollars) would lead to an average decrease by 17% for men and 28% for women in unconditional hours of care provided (4.5% for men and 10% for women at the intensive margin). That would translate into 7.5 fewer hours of care provided by men and 25.5 fewer hours of care provided by women per year. The same increase in wages, with the labor supply elasticity of 0.15 (men) and 0.28 (women) estimated using the same instrument set, implies 34 hours more work per annum for men and 53 for women. This comparison suggests that the wage elasticity of care supply is far from being trivial. On the contrary, in absolute terms it makes up about 22% of the labor supply response for men and about 50% of the labor supply response for women.

The economic significance of the estimated coefficients can be illustrated by considering the recent elimination of the Social Security earnings test. This reform effectively translates into a 33 percent increase in the wage rates faced by the affected group. Current estimates suggest that this increase would be associated with a 56 percent decrease in care provided by males to their parents and a 92 percent decrease in that provided by females. Even though these estimates of the wage elasticitiy may seem to be too large, one should remember that they are computed from the relatively small base of annual care provided to elderly parents when compared to such significant time uses as working time or leisure.[30]

[30]These estimates are 53 percent and 76 percent respectively if the sample is restricted to individuals with wage lower than $50. The wage elasticity estimates for this sample are given in Appendix Table A5.

Extensions and Limitations: Informal Care

Since the focus of this study is on the informal care that adult children provide to their elderly parents, this section will consider some important limitations and extensions related to its analysis in turns: (i) control for the price of formal care; (ii) parents' living arrangements; (iii) different types of caregiving; and (iv) possible variation in the wage elsticity across differnt groups of population.

Price of Formal Care

Formal care in the theoretical model described earlier refers to the time or services of other people that can be purchased from outside the parent-child dyad. Although no information on the formal care prices is available from the data, state level proxy variable can be used to get an idea of the importance of this variable. One of the possible cadidates would be mean wage of the personal home care aides.[31] The duties of workers in this occupation very closely correspond to the list of activities mentioned in the HRS questions from which the informal care variable was constructed.

Table 12 reports selected coefficients from the regressions similar to those reported in Tables 5 and 6 without instrumenting and with IV-3 set of instruments used for the wage rate. As could be seen inclusion of the proxy for the price of personal home care in the form of the mean wage of personal home care aides in the state of residence does not have much impact on the estimated wage effect. In addition, the estimated coefficient for the proxy is quite small with very large standard errors.

This analysis, however, has some drawbacks that may explain the weak results. First, the state average is assigned to the respondent based on his/her own state of residence, since the information on the parents' state of residence is not available.

[31] "Personal and home care aides also called homemakers, caregivers, companions, and personal attendants provide housekeeping and routine personal care services. They clean clients' houses, do laundry, and change bed linens. Aides may plan meals (including special diets), shop for food, and cook. Aides also may help clients get out of bed, bathe, dress, and groom. Some accompany clients to doctors' appointments or on other errands" (BLS, 2006-2007).

Also, the state average wage maybe a poor proxy for the price of paid home care that the individuals face. There exists a vast array of possibilities to choose from. Some people may turn to companies specializing in elderly care, others may hire a nurse or a maid to assist them in everyday life, or pay certain amount of money to their relatives to provide occasional services. Yet another possibility available for poor elderly would be to have Medicare paid personal home care services which are prescribed by the doctor.

To circumvent the problem related to the state of residence of the parents and test whether the state average wage of personal care workers is a good proxy for the price that individuals face the analysis of its effect on the demand for paid home care has been implemented. The right hand side variable in this analysis is the number of hours of paid personal home care per month that the HRS respondents get. A set of controls is chosen so as to get more similarity with the main anlysis in the current paper, accounting for the fact that the respondents are now care recipients, not care givers. Results in Table 13 show that in spite of the fact that the own price effect is negative (as would be expected), the estimated standard errors are quite large making the estimates not significant statistically. This test clearly indicates that even if the information on the parental state of residence was available, the state average wage would be a poor proxy for the price of formal care.

Parents' Living Arrangements

Current analysis does not account for the differences in the living arrangements of elderly parents. It includes parents living independently, with the respondent or other relatives, or in nursing home. At the same time this potentially may have considerable impact on the results. On the one hand, amount of the care provided may depend on how far the parent lives from the child, suggesting living arrangement as another control in the model. On the other hand, parents' living arrangements can be considered as part of the informal care supply decisions: when the need for more care arises, parents may relocate closer to the child, or start coresiding together. Earlier literature tried to circumvent this problem by excluding pairs with coresident parents and parents in nursing homes. Table 15 shows the results for the samples of individuals excluding individuals with at least one parent (i) coresiding with them, (ii) in nursing home, (iii) either coresiding or in nursing home. Although, the estimated elasticities change slightly, and in some cases the estimated effects become statistically insignificant, they are still quite close to the ones obtained from the basic analysis.

Wage Elasticities by Different Types of Caregiving

While the analysis in the previous section focused on the total time devoted to parents, the detailed information in the data also allows studying the wage effects on the care supply disaggregated by the type of activities. Figure 3 shows non-parametric relationships between wages and care for time spent helping parents with personal needs (top row) and help with household chores (bottom row). It is reasonable to expect the latter to be more elastic: it may be less burdensome and not as urgent as personal care; hence, it is easier to postpone the task or to find a substitute. For example, the frequency of household chores or money management can be easier to reduce in the face of increasing wage rates compared to the tasks of bathing, dressing, and feeding the elderly parents. Indeed, even though for males there is very little difference between time for personal needs and for chores, for women it is clear that the time spent helping parents with household chores has more pronounced downward sloping pattern.

Columns (1)-(3) in 14 provide for the comparison of the wage elasticities of informal care supply by different types of caregiving activities not conditioning on the actual caregiving status. As can be seen, the non-parametric description is supported by the empirical analysis. The estimated wage elasticity of help with household chores

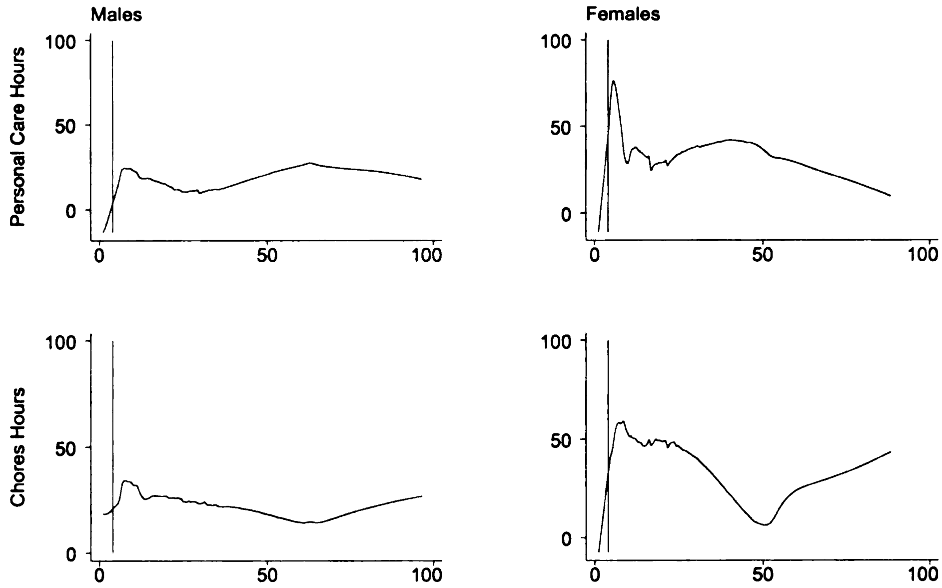

Figure 4: Care Supply by Hourly Wage Rate and By Types of Care

is generally larger than that of help with basic personal needs, and the difference is greater for females.

Variations in the Wage Elasticities of Informal Care Supply Across Individuals

The last two columns of Table 14 show the wage elasticities estimated by using different samples of potential caregivers from the HRS. With the inclusion of the self-employed individuals, the estimates change considerably, suggesting more elastic informal care supply for men than found before and less elastic for women. This may indicate more flexible working arrangements for self-employed men compared to self-employed women. Column (5) in 14 shows that individuals with siblings (the majority of the sample) are more responsive to changes in the wage rates, with the estimates of the wage elasticities being considerably large than in the benchmark case in column (1).

Conclusions

This paper extends the existing literature on informal caregiving by addressing the inconsistency in the estimates of the wage elasticity of informal care supply stemming from measurement error and omitted variables. Existence of this inconsistency would suggest that the previous estimates may be too small. Using the state level unemployment rate and industry structure as instruments for wages, this study has found that the elasticity of informal care supply with respect to wages is negative and large in magnitude. For example, the most preferred set of estimates implies that the 33 percent increase in wages associated with the elimination of the Social Security earnings test translates into a 56 percent decrease in informal care hours provided by men and 92 percent decrease in informal care hours provided by women. As in the case of labor supply, the supply of informal care by females tends to be more elastic than that by males, both at the extensive and at the intensive margin.

Furthermore, informal care supply is more elastic for individuals who have siblings, and is more elastic when considering time spent helping parents with household chores, errands, transportation, etc., suggesting that the availability of the substitutes matters a lot for the magnitude of the informal care supply response to wages.

Given the findings of the current work, there are several natural extensions worth exploring in the future. First of all, it is important to identify all possible effects that may compensate elderly for such a decrease in informal care provision associated with the increase in wages of potential care givers. The theoretical model and empirical analysis can be augmented to allow for the direct substitution of time for money in intergenerational exchange, while at the same time incorporating the possibility of the end-of-life transfer adjustment in response to today's caregiving decisions of adult children. Another important extension would be to incorporate into the model siblings' behavior and test whether siblings are more likely to provide informal care as respondents cut back on its provision. Finally, analysis of the formal care markets and inclusion of the actual price, quality, and availability of formal care in the informal care analysis would enrich the understanding of the effect wages on care provision in the current institutional setting.[32]

[32] Inclusion of the variable reflecting price of formal care at the state level, such as average wages of the personnel, occupancy rates and staff ratios in nursing homes, did not change the estimates of the wage elasticity considerably while having virtually no explanatory power on their own. This may suggest that these measures are poor proxies for the variation in the prices at the individual level.

Chapter 2. Net versus Gross Measure of Monetary Transfers in Intergenerational Exchange

Introduction

Decreased provision of informal care to elderly parents in response to increasing wages of adult children may translate into a significant cost to the society unless compensated in some way within the families. According to Nizalova's (2006) findings, the 33% increase in wages associated with the recent elimination of the Social Security Earnings test leads to a 56% decrease in average informal care provided by males and a 92% decrease in average informal care provided by females. One of the potential counter effects of increased wages is increased financial assistance provided to elderly parents. However, evidence on these effects is mixed and scarce. Nizalova (2006) finds close to zero effect of wages on monetary transfers while earlier literature documented a negligible wage elasticity of informal care supply and a considerable wage effect on monetary transfers. The difference in findings could be explained by the wage endogeneity addressed in Nizalova (2006). However, it turns out that this is not the only methodological difference among the earlier studies.

In addition to employing an instrumental variable approach to deal with wage endogeneity, Nizalova (2006) uses the net measure of monetary transfers and estimates average effects in the population by OLS. Earlier studies use gross transfers to elderly parents and adopt Tobit specifications. Thus, it is not clear what is responsible for the observed differences in the estimated effects.

The current paper represents a first attempt at systematic analysis of the choice of the transfer measure, focusing on the literature that studies the effect of wages on monetary transfers to elderly parents. The paper does not aim to establish the superiority of one measure over the other. Rather the goal is to show the importance of the choice and the circumstances when one would be more appropriate than the other, and to emphasize the consequences that the choice may have for the conclusions. In addition, the paper addresses the difficulty related to the empirical analysis of net monetary transfers and, by means of a three-part empirical model, uncovers important features of the reality. For example, it is found that the adult child's wage has a significant positive effect on the movement of the child from being a net recipient of transfers to a non-participant to a net giver. This confirms that children do substitute time transfers for money transfers, although the effect estimated is quite small in magnitude. Furthermore, the effect of wages at the intensive margin is positive for net givers, but the effect for net recipients is imprecisely estimated. Another interesting finding is that some characteristics have similar effects on both probability of being a net recipient and probability of being a net giver, which can be interpreted as a probability of participation in intergenerational exchange. Among these are education (more educated children are both more likely to get transfers and more likely to give transfers) and number of parents (more parents living means more chances to either give or receive). Overall, empirical findings suggest that the theoretical models used to explain transfers in the literature on the wage effects on intergenerational exchange do not adequately capture the reality. Therefore, this paper concludes with an outline of a new theoretical model that is potentially capable of explaining the documented patterns and relationships.

The paper is organized as follows. Section 1 provides the background and documents the coexistence of the different measures of transfers in the literature on intergenerational exchange. In addition, this section provides a detailed review of results from the studies that focus on the effect of wages on monetary transfers to elderly parents. Section 2 provides empirical evidence that the differences in the earlier studies are attributable to the choice of the transfer measure. Actual transfer patterns and limitations of OLS specification are discussed in Section 3 as possible sources of the difference in the wage effects. Section 4 outlines a three-part empirical model for the study of net monetary transfers and presents empirical results. Finally, Section 5 discusses the existing theoretical model of intergenerational exchange and outlines a new model that incorporates the findings from the current paper.

Background

This paper focuses on the effect of individuals' wages on intergenerational monetary transfers between them and their elderly parents. In the related literature and the broader intergenerational exchange literature two measures of transfers are used: gross and net. Table 16 shows different measures of transfers used in the empirical literature where the adult child is the unit of analysis. Thus, gross transfers received (in-transfers, downstream transfers) are defined as flows of money to adult children from their elderly parents. Gross transfers given (out-transfers or upstream transfers) are flows of money to elderly parents from their adult children. Net transfers are defined as the difference between gross out-transfers and gross in-transfers. Therefore, net transfers can be thought of as a balance, or outcome, of the two processes of giving and receiving in the exchange between a generation of adult children and a generation of their elderly parents. Negative /positive net transfers refer to the flows of money received /given by adult children over and above what they have given /received themselves.

There exist circumstances when one measure would be preferred over the other, depending on the research and policy questions that are being analyzed. For example, if one is interested in the effect of a tax reform on gifts and inter-vivos transfers, then the gross measure of transfers given should be studied. Also, if the question is how an individual adjusts his/her support to elderly parents in response to a change in the opportunity costs of time then again the gross measure of transfers is the right measure to analyze, as the focus here is on the process of giving itself. However, if one seeks to evaluate costs of a policy reform to the whole society, e.g. to find out whether decreased provision of informal care is compensated by an increase in the financial assistance received by the elderly, as in Nizalova (2006), then the analysis of the gross flow of money from the younger generation to the older generation is not sufficient. If the gross transfer measure is analyzed in search for such compensatory effects, both those who do not give any money and those who receive money from their parents are treated as non-contributors to that gross flow. This may overestimate the wage effect if some of the parents will actually increase their transfers to adult children in response to the increased opportunity cost of their children's time. It is also possible that the analysis of the gross transfer measure may provide some insight as to the net transfer measure and vice versa. But this question has not been addressed in the literature.

The literature studying the role of wages in intergenerational exchange is very scarce. Thus it is beneficial to consult first with the more developed literature on transfer motives. Keeping in mind that net transfers in the transfer motivation literature are net transfers received, this literature can be classified along several dimensions: (i) the origin of the data (developed and developing countries), (ii) the choice of the transfer measure (net transfers received, positive net transfers received, or gross transfers received[33]), (iii) the use of econometric technique (OLS, Tobit, Cragg's (1971) two-part model).

Intergenerational Transfer Motives Literature

Net transfers tend to be used more often in the studies on developing countries (Kuhn and Stillman, 2004; Frankenberg and Kuhn, 2004; Cox, Eser, and Jimenez, 1998; Cox, Hansen, and Jimenez, 2004; Kazianga, 2003; Cox, Jimenez, and Okrasa, 1997). These studies usually use OLS specifications. A few studies (Cox, 1987; Cox and Rank,1992) use net transfer measure with the US data. However, they define their measure of transfer as positive net transfer received, thus assigning a status of non-participants to net givers. They follow Cragg's (1971) two-part specification consisting of one equation for the decision to transfer (either probit or logit) and one selectivity-corrected OLS equation estimated on the sample with non-zero positive transfers. The rest of the literature uses the gross measure of transfers in either OLS or Tobit specifications with the data from the United States (McGarry and Schoeni, 1995; McGarry, 1999; Schoeni, 1997). Two papers by Lillard and Willis (1997) and Frankenberg, Lillard, and Willis (2002) stand apart from this classification. They both apply two-part models to study the gross measure of transfers given by respondents to their children and transfers received by respondents from their children in the context of Malasiya and Indonesia.

Several papers have footnotes or short comments about the choice of the transfer measure. For instance, Schoeni (1997) makes his choice based on the argument that a child (or a parent) can only make his/her own decision and cannot force the other party to make a transfer no matter how negative the desired amount of transfer may be. Cameron and Cobb-Clark (2002) provide a footnote where they mention that empirical estimation using a net measure of transfers does not give the same

[33] Positive net transfers received and gross transfers received treat net givers and givers respectively as non-contributors to intergenerational exchange.

statistical power as estimation using a gross measure, thus explaining their choice of the gross measure.

What adds to the confusion about the appropriateness of gross versus net measure of transfers is the use of different measures by the same author(s). This can be illustrated by the following example. Cox and his coauthors (Cox, 1987; Cox and Rank, 1992; Cox, Eser, and Jimenez, 1997) mostly analyzed the positive net monetary transfer measure from parents to their adult children in a two-part specification.[34] However, Cox, Hansen, and Jimenez (1999) use the full net transfer measure for the analysis instead with no justification for the switch from the positive net transfer measure. A possible explanation for this may be related to the choice of an empirical approach. The authors estimate a regression with an endogenous spline. This task may not be technically feasible with a two-part model.

Another possible explanation for the observed pattern of the use of different measures of transfers and different econometric techniques may be the prevalence of participation in intergenerational exchange. With the low prevalence of exchange, the distribution of net monetary transfers possesses very undesirable features that create difficulties for econometric modeling and further estimation. So, the larger is the prevalence of exchange, as for example in developing countries,[35] the more likely it is that the net transfer measure will be used in the analysis. On the contrary, studies based on the Health and Retirement Study data (HRS), which has lower prevalence of non-zero transfers, usually rely on the gross measure of transfers and adopt either OLS or Tobit specifications (McGarry and Schoeni, 1995; McGarry, 1999; Schoeni, 1997; Pezzin and Schone, 2000; McGarry, 2003b).[36]

[34]The word "elderly" is omitted here because these studies include a wide range of ages on the side of givers and the side of recipients.

[35]For example, 80% of Filipino households (Cox, Hansen, and Jimenez , 2004) 25-50% of Bangla and 70-79% of Indonesiona households (Frankenberg and Kuhn, 2004) are involved in intergenerational exchange, while this number varies from 15 to 25 % in the United States.

[36]A small share of non-zero transfers leads to noisy estimates in the second part of the model.

Literature on the Wage Effect on Intergenerational Transfers

There are only five studies of the wage effects on intergenerational transfers that the author is familiar with (Sloan et al., 2002; Zissimopoulos, 2001; Ioannides and Kan, 1999; Sloan et al., 1997; Couch et al., 1999; Nizalova, 2006). All of them use data from the United States, but they differ in their choice of the transfer measure and their empirical approach. The studies that utilize data from the Panel Study of Income Dynamics (PSID) and the National Survey of Families and Households (NSFH) include much younger children than do those based on the Health and Retirement Study, thus the adjective "elderly" is omitted in this discussion. Table 17 contains a summary of these studies. It includes the estimated wage effects and the wage elasticities of monetary transfer calculated where possible.

Couch, Daly, and Wolf (1999), using data from the 1988 wave of the PSID, estimate a system of four Tobit equations for *gross* money transfer to parents, time transfer to parents, labor market time, and housework time simultaneously. Subsamples of coupled households, households headed by unmarried men, and households headed by unmarried women are considered separately. They find that a 1% increase in wages is associated with a 1.44% increase in the amount of money transfered by adult married males to their elderly parents, and a 2.44% increase in the gross money transfer to elderly parents originating from single females. The response to a 1% increase in female wages in a coupled household and in wages of single males is found to be about 0.4%.

Similar to Couch et al. (1999), **Ioannides and Kan (2000)** use PSID data but study two-directional transfers using both univariate Tobit models and bivariate Probit models for different combinations of *gross* transfers to and from parents. They find that higher wages of children are associated with more money being transfered to parents and with less money being received from parents. However, the estimated effect on transfers received is not precise. As could be seen from Table 17 their estimates are quite modest in comparison to those from Couch et al. (1999).

Zissimopoulos (2001) studies the existence of a substitution between time and monetary transfers from the perspective of elderly parents. She estimates separate Tobit equations for *gross* monetary transfers measured in logarithms finding a small but significant positive effect of wages.

Sloan, Zhang, and Wang (2002) use a two-part model consisting of a logit for the probability of giving a transfer and OLS for the logarithm of the actual amount given to explain the decisions of adult children - HRS respondents. They find significant positive effect of wages on *gross* monetary transfers to elderly parents, at both extensive and intensive margins. The estimated elasticity at the extensive margin is quite high, making the overall wage elasticity estimate close to 3.

Finally, **Nizalova (2006)** uses data from the 1998 HRS wave and estimates OLS and a two-stage least squares model for *net* monetary transfers. She finds no evidence of positive effect of wages on net financial flows from adult children to their elderly parents. Even more puzzling is a negative two-stage least squares estimate of the wage effect on the net monetary transfer given by female children.

To summarize, the wage elasticity of gross transfers to elderly parents is always positive and in most cases statistically significant, but this is not found in studies that use the net transfer measure. The hypothesis explored in this paper is that the differences in the earlier results are mainly due to the choice of the transfer measure. Alternatively, those differences may be explained by the data used or the choice of the empirical methods. Therefore, this study will first focus on a single data set and employ the same empirical strategy to gross and net transfers to explore the consequences of the choice of the transfer measure alone.

Empirical Test

Data

The analysis in this paper relies on the data from the Health and Retirement Study (HRS). The Health and Retirement Study is a national longitudinal survey representing a rich source of information on the lives of older Americans, their health and economic status. It also includes extensive data on intergenerational transfers and characteristics of parents and children. The Study consists of people born in 1947 and earlier, totaling to more than 21000 respondents.[37]

The analysis covers the period 1992-2000 and focuses on HRS respondents with at least one living parent linked to the information on all living parents as a group. Information on transfers is taken from the next wave survey. Since transfer variables are based on a two-year recall period, this information is linked to the information on respondents and parents available at the previous wave. Thus it is assumed that individuals made transfer decisions during the wave i to $(i+1)$ based on the information available at wave i (Sloan, Zhang, and Wang 2002). The analysis is implemented separately for men and for women. The samples contain 3142 males and 2999 females with 7673 and 7576 observations respectively. Table 18 presents summary statistics for the two samples. For details on sample restrictions see Nizalova (2006).

Nonparametric Evidence

Figure 5 depicts the non-parametric estimation[38] of the wage effect on monetary transfers using different measures separately for men and women. The first and the

[37]HRS started in 1992 with the cohort of individuals born in 1931-1941, and AHEAD started in 1993 with the cohort of individuals born in 1923 and earlier. The survey of those cohorts continued every two years till 1998 when both surveys were combined into one and two other cohorts, Children of Depression Age (CODA) cohort born in 1924-1930 and War Babies (WB) cohort born in 1942-1947, were added.

[38]Locally weighted smoothing (lowess) kernel estimator using the ksm routine in STATA with a bandwidth setting of 0.4.

second columns of the graph show that the association between the wage effect and the transfers differs depending on which measure of transfers is used. While the effect of wages on gross out-transfers is clearly positive, it slopes negatively for the net out-transfer measure for a certain range of wages. Although non-parametric analysis does not take into account many other factors it indicates the importance of being careful with the choice of the measure, as this choice may have important effects on the conclusions.

OLS Estimation Results

To supplement the non-parametric evidence, Tables 19-20 present the results from the multivariate OLS regressions.[39] As could be seen from columns (1)-(2) and (4)-(5), the estimates of the coefficients on wages differ depending on the measure of transfers used. This exercise suggests that the use of different transfer measures and not the use of different data sets (and/or different econometric techniques) is responsible for the differences found in the estimates of the wage effects. Up to this point, the objective has been to document the existence of the difference between the wage effects on net

[39]Justification for this specification including discussion of the control variables can be found in Nizalova (2006).

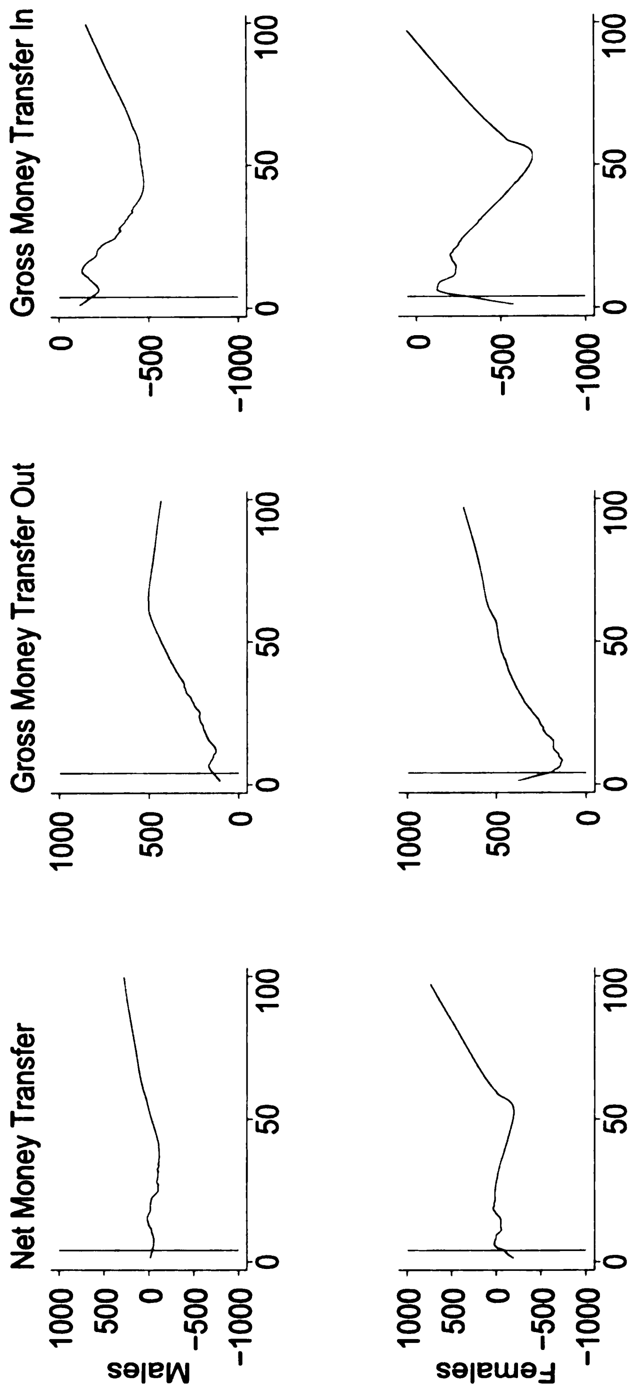

Figure 5: Monetary Transfers by Hourly Wage Rate

versus gross transfers, controlling for other possible contributors to the difference, such as the data set and the empirical specification. The next section is devoted to the discussion of possible sources of the difference in the estimates.

Possible Explanations for the Difference in the Wage Effects

As the previous section has shown, the difference in the estimated wage effects does seem to stem from the choice of the transfer measure. In this case two issues deserve investigation. On the one hand, net transfers and gross transfers may be totally different variables for each particular observation and this may be the primary reason for the differences in the results. On the other hand, the degree of possible inconsistency in two measures maybe different. As Wooldridge (2002, p.524) shows, the OLS estimates for censored outcome are inconsistent since the relationship between dependent and independent variables is non-linear.[40] Nothing is known about the possibility of inconsistency for the dependent variables similar to the net transfer measure, but by analogy one may suspect presence of inconsistency in these estimates as well. This section will consider these two issues in turn.

Transfer Pattern

The upper part of Table 2 describes the pattern of intergenerational transfers as observed in the HRS data. As can be seen, the transfer incidence is quite low - only about 20% of individuals engage in intergenerational exchange of money with their parents.[41] Although the percentage of givers is higher than the percentage of

[40]Wooldridge also shows that the estimates are inconsistent by the same multiplicative factor, so that the relative effects can effectively be recovered from the regression.

[41]Prevalence of monetary transfers between younger children and their parents is somewhat higher and their pattern is different in HRS. For example, Ioannides and Kan (2000) numbers from the Panel Study of Income Dynamics are the following: 4.1% of children's households give monetary

recipients in the generation of adult children (13% versus 5%), conditional on the transfer status, the amount of money given is considerably smaller than the amount of money received (about $1500 versus $ 4700).

Another interesting observation in the transfer pattern is that less than 1% of individuals engage in both giving and receiving at the same time.[42] So for the majority of the participants in intergenerational exchange the actual magnitude of transfers is the same whether one considers the gross or the net measure. Then the only difference between the gross and the net measure of transfers is the way in which the net recipients are treated. When the gross measure is considered individuals who are receiving transfers from their parents are treated as non-participants in the process, while the net measure differentiates them from non-participants.

Assuming no overlap between gross givers and gross recipients (which is close to the observed pattern), it is possible to derive an algebraic relationship for the estimates of marginal effects and elasticities for the two transfer measures. Appendix B presents the calculations documenting this relationship. Intuitively, the relationship between the estimates of the wage effect on gross versus net transfers depends on the relationship between wages and transfers among the recipients. By replacing negative transfers with zeros the distribution of the dependent variable is suppressed. So, if higher wages are associated with less transfers being received by the recipients then the estimated coefficient on net transfers is larger in magnitude than the estimated coefficient on gross transfers. If, on the contrary, the relationship between wages and transfers for the recipients is negative, then the wage effect on gross transfers is smaller than that on net transfers. Finally, if there is no relationship between wages

transfers and 22% receive them. However, average age of children in PSID is 36, while in HRS it is about 55 years old. So, this age difference may explain the reverse pattern of transfers in the HRS compared to the PSID.

[42]This number would have been even smaller if the data were organized at the level of child-parent dyads as in some of the early studies on the topic. As the data in the current study links the information on an individual to the data on all related parents (including in-laws) as a group, this explains the existence of the small group of simultaneous givers and recipients.

and transfers for recipients, then the estimated coefficients should be the same:

$$\beta_{Net} = \beta_{Gross} + \beta_{Neg} P(y < 0) \frac{Var(x|y < 0)}{Var(x)} \tag{18}$$

Also, as is obvious from the formula, the difference between the estimates depends on the relative variation in wages among the recipients and in the population as a whole: a larger spread of wages among the recipients increases the gap between the effects estimated for net versus gross transfers.

As the results in Tables 19-20 confirm, the relationship between wages and transfers among recipients is different from that among givers. However, as mentioned earlier both estimates may have different asymptotic biases. The next subsection discusses the implications of that.

OLS Shortcomings

Although the OLS estimates approximate the effect of x on $E(y|x)$ when x is near its population mean (Wooldridge, 2002, p. 525) no matter what the distribution of y is, there exist two significant shortcomings that have implications for the parameter estimation. These shortcomings become especially important in the context of net monetary transfers.

First, the distribution of net transfers has considerable concentration of mass on zero. The majority of the population chooses not to participate in intergenerational exchange of money and OLS does not account for this fact. The other has to do with the nature of the net transfer measure. It is an outcome of the two processes of giving and receiving, and arguably it would be too restrictive to assume that both processes are governed by the same rule. However, OLS imposes this restriction by assuming that the effects of explanatory variables are the same throughout the net transfer distribution.

Comparison of the columns (2) and (3) in Table 19-20 confirms the possibility of the existence of different effects for different parts of the net transfer distribution. Some of the factors have the opposite effect on gross in-transfers when compared to gross out-transfers. Focusing only on the factors that are statistically significant in at least one of the equations reveals the following. More educated individuals both give more and receive more. Having more young children is associated with less money given to parents while having virtually zero effect on the amount of money received from parents. The number of parents and the number of living mothers is associated both with more money given and more money received. This may be suggestive of the effect of certain factors on the decision to participate in exchange, not on the amount of transfers, as the estimates shown in Tables 19-20 combine both effects at the extensive and at the intensive margin.

To summarize, the OLS results mask some of the features of the intergenerational exchange that may be important for understanding the reality. Taking into account all of the mentioned considerations, an empirical model that would overcome these difficulties should provide for the following: (i) the existence of the differential effect of wages on net transfers depending on whether it is a negative or a positive part of the net transfer distribution, (ii) the separation of the effect at the extensive and intensive margins. The next section will be devoted to an exploration of a three-part model as an alternative in estimating the wage effect on net monetary transfers.

Three-part Model as a More Flexible Estimation Strategy for the Analysis of Net Transfers

One of the possible models that possesses the features described in the previous section could be a variation of the Cragg's (1971) two-part model. It consists of an equation(s) describing the decision to participate in exchange (either give or receive transfers) and two separate OLS equations for the amount of positive net transfers given conditional on net giver status and for the amount of negative net transfers conditional on net recipient status. Multinomial Probit is adopted to model the transfer status decision as it allows for the non-linear effect on the probabilities of observing different transfer statuses. Ordered Probit and Multinomial Logit have been attempted in the earlier stages of the analysis. However, both of them have been dismissed in favor of a Multinomial Probit for two reasons. Ordered Probit places a restriction of linearity on the effects independent variables have on a latent variable. This forces the effects on probabilities to be monotonic. This is an undesirable feature given that some of the variables seem to have a qualitatively similar effect on net giver and net recipient status: for example, more parents alive naturally leads to higher probability of either being a net recipient or a net giver. However, with the Ordered Probit if the effect on the probability of being a net recipient is positive, it will be negative on the probability of being a net giver simply by the structure imposed by this estimation method. The Multinomial Logit, although it allows one to overcome the restriction mentioned earlier, requires the Independence of Irrelevant Alternatives (IIA) assumption. Multinomial Probit relaxes both restrictions and thus is used in the current analysis.

Model Description

Assume that the transfer is determined in two stages. In the first stage an individual decides which transfer status to assume and then in the second stage the amount of the transfer is determined. There exist three possibilities $j = 1, 2, 3$ with one corresponding to a status of the net recipient, two to a status of the non-participant, and three to a status of the net giver. Adopting Dow and Endersby's (2004) approach, assume that an individual i's utility in case he/she chooses status j is U_{ij}. It is a function of both child's (X_i^C) and parent's (X_i^P) characteristics and its parameters may differ depending on the chosen status. Also, for simplification, this utility function can be thought of as a weighted sum of the utilities of both the child and the parent. Thus,

$$U_{ij} = \beta_j log\, w_i + (X_i^C, X_i^P)\gamma_j + \epsilon_{ij} \tag{19}$$

It is assumed that individuals are utility maximizers and after calculating values of the utility function in three different cases they choose the transfer status that gives them the highest utility. Therefore, the probability that the individual i will choose to be, for example, a net recipient is the following:

$$P_{i1} = P(U_{i1} > U_{i2}, U_{i1} > U_{i3}) \tag{20}$$

So, for any m in the transfer status set:

$$P_m = P\Big[\epsilon_{im} - \epsilon_{ij} < (\beta_j log\, w_i + (X_i^C, X_i^P)\gamma_j) - (\beta_m log\, w_i + (X_i^C, X_i^P)\gamma_m), j \neq m\Big] \tag{21}$$

It is assumed that the errors are distributed multivariate normal with mean zero and a symmetric covariance matrix.

Although this model has its own important limitations, like, for example, the assumption of a multivariate normal distribution of errors, it offers several significant advantages for the study of net monetary transfers. First of all, it tackles the highly skewed distribution of net transfers by disaggregating analysis into several pieces. This strategy has a potential of producing more consistent results by allowing for non-linearity of the effects. Second, by separating the intensive and extensive margins it sheds some light on where the effect of the wage change comes from: whether more people quit taking money from their parents and start giving back to them or whether those people who are already providing financial assistance start to transfer larger amounts. A third advantage is that it allows for heterogeneous effects. For example, people with certain characteristics (e.g. more or less educated, married, or those having more parents) are simply more likely to participate in any exchange (either giving or receiving). Finally, it does allow for differences in the parameters depending on the transfer status. All the listed advantages bring a better understanding of how the net transfers are determined and thus may provide further guidance for theoretical modeling.

Results

Tables 21 and 22 show the results from the estimation of a three-part model.[43] Columns (1)-(3) in the tables present the estimated marginal effects from the multinomial probit regression. The last two columns show the results from the OLS estimation for strictly positive (net givers) and strictly negative (net recipients) amounts of transfers.

Transfer status

As could be seen from the results on transfer status, wages have unambiguously positive effect on the movement of an individual from being a net recipient to a non-participant to a net giver. One should be careful, however, as the wage variable is in logarithms, so the true marginal effects of wages would be one hundred times smaller than those reported in the table. So, in spite of the fact that the wage effects are statistically significant, they are quite small in magnitude. For example, the effect of a 10% increase in wages of males is associated with a 0.085 percentage point decrease in the probability of being net recipient ($P(y < 0|x, males)$=5.45%) and a 0.265 percentage points increase in the probability of being net giver ($P(y >$

[43]Adding the inverse Mill's ratios for conditional equations to correct for selectivity did not have important implications for the estimates of interest, but led to a significant increase in the standard errors.

$0|x, males)$=12.48%). The numbers for females are 0.084 and 0.394 correspondingly ($P(y < 0|x, males)$=4.50%, $P(y > 0|x, males)$=14.28%). To compare the effects, an equivalent increase in non-labor income[44] ($4000) leads to a 0.76 percentage points decrease in the probability of being a net recipient and a 0.96 percentage point increase in the probability of being a net giver for males. This effect is a bit smaller for females but still considerably larger than the wage effect. Being non-white or hispanic significantly raises the chances of being a net giver compared to a net recipient. Having more siblings decreases the chances of being a net recipient, as would be expected. Having more educated parents and/or parents who are better off financially than the respondent increases the chances of getting transfers and decreases the probability of providing transfers for both males and females. Those who have parents financially worse off are more likely to be net givers than net recipients.

An interesting feature is that some of the variables have similar effects on both net recipient and net giver status, thus affecting the probability of engagement in any exchange. More educated, non-married individuals are more likely to be involved in exchange. Obviously the number of parents has significant positive effect on the probability of any exchange.

Transfer amount

The last two columns in the tables show the estimation results on the sample of net recipients and net givers separately. It should be emphasized that the dependent variable in the last column is strictly negative, so that a positive coefficient implies less money being received from parents. Although the estimates of the wage effects are not statistically significant, they are far from being similar. In fact, according to them, higher wages lead to more money given among the net givers and more money being received among the net recipients. However, the latter effect is not statistically

[44] 10% increase in wages of males is about $2 out of sample average of 20.31. For a full-time male worker (2000 hours a year) this implies a $4000 increase in earnings.

significant.

As the samples of net givers and net recipients are quite small, the results may be partly driven by outliers. Tables 23-26 provide results following alternative estimation procedures. Columns (1) and (4) repeat the results from OLS estimation, columns (2) and (5) omit the 1% of the sample with the highest transfers reported, and columns (3) and (6) give the least absolute deviation results. As could be seen, the estimated results do seem to be driven in part by outliers. Obviously, the Least Absolute Deviations estimator is a better alternative to OLS as it is more robust to outliers. However, this approach has some shortcomings in the current context. LAD is a median-based estimator and it would not be possible to translate the estimates into the form of elasticities for the purpose of comparison to the OLS results. Therefore, although the results from LAD estimation should be kept in mind, the OLS estimates are used in further exploration and comparison of the wage elasticities obtained from the three-part model to those from the OLS.

Table 27 summarizes the elasticities from the three-part model at the extensive and intensive margins for net recipients and net givers. They correspond to four parts of the following equation[45]:

$$\begin{aligned}\frac{\partial E(y|x)}{\partial x_j} &= \frac{\partial P(y<0|x)}{\partial x_j} * E(y|x,y<0) + P(y<0|x) * \frac{\partial E(y|x,y<0)}{\partial x_j} + \\ &\quad + \frac{\partial P(y>0|x)}{\partial x_j} * E(y|x,y>0) + P(y>0|x)\frac{\partial E(y|x,y>0)}{\partial x_j}\end{aligned} \tag{22}$$

In terms of the wage elasticities, the lower part in Table 27 shows the estimates of the wage elasticity at the extensive and intensive margin as well as those for net givers and net recipients. The overall elasticity of net monetary transfers with respect

[45]Derivation of the marginal effect and the elasticity for the net monetary transfers is given in Appendix C.

to wages is given by the following formula:

$$\epsilon_{Net} = (\epsilon_{NegExt} + \epsilon_{Neg}) * \frac{P(y < 0|x) * E(y|x, y < 0)}{E(y|x)} + \\ +(\epsilon_{PosExt} + \epsilon_{Pos}) * \frac{P(y > 0|x) * E(y|x, y > 0)}{E(y|x)}, \tag{23}$$

where $NegExt$ and $PosExt$ refer to the elasticity for negative transfers and positive transfers respectively at the extensive margin.

Table 28 shows the decomposition of the marginal effects and elasticities for a 1% increase in wages at both extensive and intensive margins. Estimates in this table imply that the overall wage effect is to a great extent coming from the changes in the transfer status: higher wages induce more individuals to quit the net recipient and the non-participant categories and join the net givers category by either increasing their own monetary transfer to parents or receiving less money from parents. At the same time, although those who retain net recipient status either cut back on their transfers or receive more from parents, those who have been net givers significantly increase the amount they give. As the upper panel in Table 27 shows, the overall wage elasticity of net transfers calculated using results from the three-part model is significantly larger than the ones calculated from the OLS results.

Implications for Further Theoretical Development

As the above empirical analysis suggests, explaining net monetary transfers in a theoretical model is not a trivial exercise. This section will consider the theory used to model time and monetary transfers between adult children and their elderly parents. Then it will sketch a theoretical model based on the results from the previous section.

Discussion of the existing model

Three of the five studies reviewed earlier build their analysis on a theoretical model of informal care and monetary transfers from adult children to elderly parents (Nizalova, 2006; Zissimopoulos, 2001; Sloan et al., 2002[46]) with slight modifications.

It is a simple model that involves a giver and a recipient (Nizalova, 2006), with an adult child assumed to be a giver and an elderly parent a recipient. The giver obtains utility from own consumption, leisure, and utility of the recipient. The giver's time endowment is allocated between care, work, and leisure; and his/her labor and non-labor income is spent on consumption goods and monetary transfers to the recipient. Monetary transfers can be positive as well as negative. Utility of the recipient, in turn, depends on own consumption and care, which is produced with only the input of time of other people (market-purchased time or time provided by the giver), and is subject to the budget constraint.

In this model a non-negativity constraint is not placed on the amount of monetary transfers. So, monetary transfer can be thought of as an auxiliary mechanism equalizing the marginal utility of consumption of the giver to that of the recipient after all other decisions have been made. The likelihood of observing positive net transfer from an adult child to his/her elderly parent is greatest when the time in caregiving is zero. This happens when the wage rate is higher than the price of formal care adjusted for the differences in productivity of hired help compared to that of an informal care giver. As the wage rate decreases, the model predicts more time devoted to care giving, and thus a smaller monetary transfer from the giver to the recipient to adjust for the differences in marginal utilities of consumption. As the wage rate decreases further, the time in caregiving increases and net monetary transfer decreases. This potentially leads to a reversal of the net monetary flow, i.e. to the negative net monetary transfer. Some individuals would essentially be paid for their caregiving services to parents when their wages are too low and/or their productivity in caregiving is too high compared to the price of formal care.

[46]Sloan, Zhang, and Wang (2002) assume non-negative transfer from children to their parents in the theoretical model.

The decision to participate in intergenerational exchange as well as the amount of transfers received is solely governed by the comparison of the productivity adjusted wages with the price of formal care faced by an individual. No utility is derived directly from time or money transfers given or received. For instance, parents' preferences for caregiving may be biased towards the provision of informal care (e.g., when the parents place additional value on the time spent with their own children rather than with a stranger). This may actually make parents give more money to their children to induce caregiving as the children's cost of time increases.

Although this approach presents a simple way of modeling net transfers and allows for the existence of negative net monetary transfers, it has some important limitations. First of all, the major criticism is that there is no mechanism in place for the parent to be willing to make a transfer. As the child makes all the decisions, he/she cannot force the parent to provide the money no matter how negative the desired monetary transfer may be. Another limitation is that it does not explain the existing pattern of transfers, in particular, the mass at zero in the distribution of net transfers is not explained by the model. Finally, it treats net transfer as being a single choice while it is actually a variable that describes an outcome of the two processes originating from two parties. Hence, it is not capable of explaining the heterogeneous effect of wages on transfers in the subpopulations of net givers and net recipients.

The simplest way to incorporate the high concentration of probability mass on zero into a theoretical model is to allow for fixed costs of monetary transactions and/or of providing informal care. Low liquidity of assets may explain the low probability of observing close to zero transfers in theory. However, this will not ensure either the mechanism for the parent to be willing to make a transfer or the differences in the wage effects for net givers and net recipients. Another possibility would be to use a bargaining model. Failure to reach a mutually beneficial agreement on the amount of time and monetary transfers would explain in this case the high probability of observing zeros in the empirical data (e.g. divorce threat or separate spheres type bargaining model), and at the same time may allow for the heterogeneous wage effects.

Sketching a new theoretical model

Although the development of a theoretical model is not the focus of the current paper, a sketch of a model that potentially describes the existence of the documented transfer pattern and the relationships between wages and net transfers estimated is presented here. The model suggested is an extension of a bargaining model to a two-stage decision-making process. At first it is decided which roles with respect to monetary transfer the parties will assume. The outcome of this stage would be to assign the status of either a giver, or a recipient[47], or neither. The choice of a transfer status defines "separate spheres" (Lundberg and Pollak, 1993): only the giver is unilaterally deciding on the amount of monetary transfer. Separate spheres bargaining seems a more natural way to model an adult child-parent relationship since the "divorce" option does not seem credible between the two. In this type of bargaining models a solution to the noncooperative game is used as a threat point in the cooperative game.

The preferences of an adult child are represented by a utility function $U^C(X_C, l_C, U^P)$, where X_C is the child's consumption, l_C = leisure, and U^P = utility of the parent. The preferences of the parent are to some extent symmetric with the only difference being that the recipient requires care produced with the help of other people: $U^P(X_P, Z_P, U^C)$, where X_P is consumption, and Z_P is care. The following equations represent the time and budget constraints as well as the production function for care:

$$X_C = I_C + wt_w - (D_C - D_P), \tag{24}$$

[47]Remember that the transfer pattern suggests that the net and gross monetary transfer are only different in magnitude for less than 1% of the population, i.e. for the most part a giver is the same as a net giver, and a recipient is not different from a net recipient.

$$X_P + p_t t_m = I_P + D_C - D_P, \tag{25}$$

$$t_g + t_w + l_C = T \tag{26}$$

$$Z_P = \gamma t_m + Z^P(t_g), \tag{27}$$

where t_w is working time, t_g = time provided by the child to the parent, w = the child's hourly wage rate, p_t = price of market-purchased time t_m; I_C, I_P = the child's and parent's non-labor income respectively. Note that the net transfer is modeled as an outcome of the two processes here to allow for the differences in the determinants of positive versus negative transfers: D_C = transfer from the adult child to the elderly parent, and D_P = transfer from the elderly parent to the adult child.

If the child has the status of a giver (which means that there is no monetary transfer from the parent), in a noncooperative game he/she is deciding unilaterally on the amount of money transfered to parents and the amount of time provided, taking into account the amount of help purchased by the parent in the market. In this case the parent is assigned the status of a recipient and decides unilaterally on the amount of formal care purchased from the market. If instead the child is assigned the status of a recipient and the parent that of a giver, then the child is deciding unilaterally only on the amount of time transfer, and the parent is deciding unilaterally on the amount of money transfer and the amount of market-purchased help taking as given the child's informal care provision. If they are deciding not to participate in any kind of monetary exchange, then the child is only deciding on the amount of informal care provided, and the parent is deciding on the amount of formal care purchased. In either case, Cournot equilibrium will produce the indirect utility functions T^G and T^R that are then considered as threat points in the cooperative Nash bargaining framework:

$$max(U^G - T^G)(U^R - T^R) \tag{28}$$

To connect this theoretical model with an empirical three-part model described earlier, suppose $U_{ij} = max(U_{ij}^G - T_{ij}^G)(U_{ij}^R - T_{ij}^R)$ is a Nash social welfare function evaluated at the optimal allocation for the adult child - elderly parent pair i under the j's assignment of transfer status. j can take values of 1,2, or 3 as described in Section 4. The model is solved backwards. First, the parties calculate the product of their utility surpluses over the separate spheres outcomes for different assignments of monetary transfer status. After that they compare the corresponding values of the Nash social welfare function and choose the assignment of transfer status that produces the maximum welfare.

As could be seen, the model allows for a separate decision rule at the extensive versus intensive margin by modeling the choice of transfer status explicitly. After the decision on the transfer status is made, the amount of the transfer is chosen by the giver accounting for both the giver's and the recipient's characteristics. This allows for a possibility of heterogeneous effects for negative and positive parts of net transfer distribution. For example, as the estimates show, once the transfer status has been decided upon, the wage rate of the adult child may be an important factor when he/she decides on the amount of the transfer, but it may play no role in the parent's decision making after controlling for other variables describing the child's current position. Similarly, while parents' education may play important role in the transfer status determination with higher education increasing the probability of receiving transfer from parents, it is a very significant determinant of the amount of transfer the parent provides but has virtually zero effect on that provided by the child.

Conclusions

This paper has undertaken a systematic evaluation of the choice of the net versus gross measure of monetary transfers in intergenerational exchange. The main finding is that the results of the empirical analysis are very sensitive to the choice of the transfer measure and thus this matter calls for special attention in the analysis design. It is shown that the wage effect is much larger in magnitude when estimated using the gross measure of transfers compared to the net measure of transfers. This happens mostly because net recipients are treated as non-participants in exchange, and the wage effect is different for that part of the population.

To relax the restrictions placed on the analysis by the OLS specification, an alternative three-part empirical model is proposed to analyze the effect of wages on net monetary transfers. This model allows for a separate treatment of the transfers at the extensive and intensive margins as well as for the differences in the process of giving compared to that of receiving. It consists of three equations: (i) multinomial probit for the transfer status (net recipient, non-participant, net giver), (ii) OLS for the amount of transfers received conditional on the net recipient status, (iii) OLS for the amount of transfers given conditional on the net giver status. The effect of wages at the extensive margins is unambiguously positive for both males and females: as the wage rate increases adult children move from being net recipients to non-participants to net givers. The estimates of the wage effects at the intensive margin show that conditional on net giving status high wage individuals tend to give more money to their parents. At the same time, conditional on net receiving status, high wage individuals tend to receive more money from their parents, but this estimate is not statistically significant.

Combining the estimates of the wage effects from all the parts of the model allows for a comparison with the OLS estimates using net monetary transfers. It appears that the OLS in general underestimates the marginal effect of wages on net monetary transfers. Most of the effect stems from the extensive margin: as their wages go up individuals are more likely to stop getting money from their parents and more likely to start giving money to their parents. There is also a significant positive effect of wages on transfers given to parents among the net givers with that being negative but insignificant among the net recipients. In addition to providing more consistent estimates of the wage effects, the estimation of the three-part model uncovers other important features of the reality: some of the characteristics have similar effects on both probability of being a net recipient and probability of being a net giver, which can be interpreted as a probability of participation in intergenerational exchange. Among these are education (more educated children are both more likely to get transfers and more likely to give transfers) and the number of parents (more parents living - more chances to either giver or receive). Overall, empirical findings suggest that the theoretical models used to explain transfers in the literature on the wage effects on intergenerational exchange do not fully describe the reality. Therefore, this paper concludes with an outline of a new theoretical model that is potentially capable of explaining the documented patterns and relationships. The model suggested in the paper is a two-stage game-theoretic model based on the Lundberg and Pollak's (1993) separate spheres bargaining framework.

Chapter 3. Minimum Wage Effects in the Longer Run

Introduction

Exposure to a high minimum wage during the years in which teens and young adults enter the labor market may generate adverse effects that persist in the longer run. If so, then an exclusive focus on contemporaneous, short-run effects of minimum wages on youths-which is a reasonable characterization of most research and policy debate on minimum wages-may miss significant components of the potential effects of minimum wages.

How might the longer-run effects of minimum wages arise? Most directly, perhaps, the shorter-run effects of minimum wages on youths that have been studied so extensively can have lasting impacts that extend into adulthood. Existing research suggests that minimum wages may lower training among young workers, reduce the accumulation of labor market skills and experience by deterring their employment, and discourage school enrollment, although these conclusions are not without controversy.[48] This paper does not revisit these controversies, although it reports new

[48]The employment effects literature is well known and extensive. See Burkhauser, et al. (2000) for a recent study in the lengthy literature presenting evidence of disemployment effects for young individuals, but Card and Krueger (1995) for the contrary view. For evidence on the effects of minimum wages on training see Hashimoto (1982), Acemoglu and Pischke (2003), Grossberg and Sicilian (1999), Fairris and Pedace (2003), and Neumark and Wascher (2001). For evidence that minimum wages reduce school enrollment, see Chaplin, et al. (2003) and Neumark and Wascher

evidence on some of them. But if, in fact, minimum wages reduce training, employment, and schooling of young individuals, then we should expect more lasting adverse effects on both wages, employment, and other labor market outcomes. Furthermore, these longer-run effects could be exacerbated by factors sometimes interpreted as the "scarring" effects of non-employment at young ages (e.g., Ellwood, 1982), which amplify the consequences of reduced early labor market experience. On the other hand, longer-run effects that counter some of the potential adverse short-run effects are also possible. Minimum wages could lead to increased skill acquisition if a higher wage floor raises the productivity level necessary for a worker to be employable.[49] And the initial wage increases stemming from minimum wages could have persistent effects.

In this paper we explore the longer-run effects of minimum wages. Of course, research on the effects of minimum wages on training and schooling implicitly addresses the longer-run effects of minimum wages, because, for example, teens or young adults who leave school will on average have lower schooling as adults. What is different and unique in this paper is the direct estimation of these longer-run effects.[50] Instead of simply asking how outcomes such as employment, wages, etc., among 16-19 year-olds (or 20-24 year-olds) are affected by contemporaneous minimum wages, we estimate the effects of exposure to higher minimum wages at these younger ages-when minimum wages were most binding-on outcomes for somewhat older individuals (25-29

(2003); earlier, more ambiguous evidence is presented in Ehrenberg and Marcus (1982), Mattila (1978), and Ragan (1977).

[49]See, for example, Cunningham's (1981) discussion of the possible positive effects of minimum wages on the decision of teenagers to stay in school or acquire more schooling. Although increased schooling is possible, predictions are ambiguous for a number of reasons, including that raising the wage floor for unskilled workers could reduce the return to education.

[50]The only other paper of which we are aware that attempts to estimate longer-run effects of minimum wages is a study by Behrman, et al. (1983), who use Social Security earnings records to study the effects of time-series variation in the minimum wage on various measures of the earnings distribution. They incorporate a distributed lag of minimum wage effects, but do not distinguish, as we do here, effects of minimum wages at young ages when minimum wages were most likely to be binding-and their sample includes individuals up to 65 years old. Baker, et al. (1999) study the effects of minimum wages on teenagers, but focus on the employment effects that arise with relatively long lags. Although their work does not speak to the effects on adults of minimum wages experienced as teens, it does emphasize that minimum wage effects may arise over a longer run than is typically assumed in studies of the effects of minimum wages on employment and other outcomes.

year-olds).[51]

Data

Our data set comes from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) files for the years 1979-2001. We first extract data on individuals aged 16-29. We then aggregate these data to the state-year-age cell, giving us measures of averages in these cells (using CPS earnings weights)[52], and we append to these cells information on state and federal minimum wages.

To be able to study the longer-run effects of exposure to minimum wages, it is necessary to characterize the minimum wage "history" that each individual has faced. Because state-level variation in minimum wages is important in obtaining better statistical experiments, this history is characterized in terms of the higher of the state or federal minimum. The strategy used is to construct the history of minimum wages in the state in which the individual currently resides. This is a potential limitation, because with some migration from state to state the minimum wage history based on the current state of residence will measure the true minimum wage history with error.[53] Longitudinal data that followed individuals as they moved from state to state would better capture their minimum wage history, but would

[51]We follow much of the minimum wage literature and define "youth" as ages 16-24.

[52]In our regressions, we always weight by the number of observations used to construct these cell averages, multiplied by the average CPS earnings weight for the cell to account for over- or under-sampling of states.

[53]According to the Census Historical Migration Database, in each year over the period 1979-2001, 15-17 percent of the population changed place of residence. Of this total migration, 95-97 percent is migration within the United States. On average, 80 percent of domestic migrants change their residence within the same state, and the rest (20 percent) migrate to a different state. Thus, approximately 2.7-3.0 percent of the population moves to a different state each year. (See http://www.census.gov/population/socdemo/migration/tab-a-1.txt.) Later, we report results from the 1990 PUMS indicating that about 13 percent of 20-24 year-olds and 16 percent of 25-29 year-olds moved between states in the previous five years.

perhaps be more plagued by the endogeneity of migration.[54][55]

The specifications estimated below use three different measures of the minimum wage. The first is simply the current "effective" minimum wage in the state, defined as the log of the higher of the state minimum wage and the federal minimum wage. This parallels the typical study of contemporaneous, short-run effects of minimum wages in the existing literature. In the models we estimate we include fixed year and state effects, so the time-series variation induced by the federal minimum is swept out, and identification comes from variation in state minimum wages that are set above the federal level. The second measure captures not just the current effective minimum wage, but also the history of the minimum wage to which an individual has been exposed, by adding up and averaging the log of the effective minimum wage to which an individual in any cell defined by state, year, and age has been exposed in each year, from age 16 to the present age. Because we also condition on single-year age dummy variables, identification again comes from variation in state minimum wages set above the federal minimum. Finally, in what we regard as the most informative specifications, we distinguish between exposure at younger ages when minimum wages should have been more binding and exposure at older ages. Specifically, we compute the average effective log minimum wage to which an individual in a state-year-age

[54]Yet another possibility would be to instrument for the measured minimum wage history in the state of residence with the history in the state of birth (pegged to the same birth cohort), thus isolating the exogenous variation in the minimum wage history faced by individuals. In the CPS, however, data on state of birth are not available. Furthermore, while we have little doubt that this instrument would explain a great deal of variation in the minimum wage history based on state of residence-given relatively low inter-state migration-its exclusion from the equations we estimate would be questionable, as unmeasured factors associated with the state of birth might affect the labor market outcomes we study through channels other than the minimum wage history.

[55]This discussion suggests that the NLSY79 might be of some use for this research, by providing longitudinal information including state of residence for a cohort of teenagers as they age into their 30's. However, most of the cross-state variation in minimum wages on which our identification relies begins in 1987 (as reported below in Table 2). Given that the NLSY79 cohort was aged 14-22 in 1979, none are teenagers as of 1987. There is also the NLSY79 Young Adult file, based on the offspring of the mothers in the NLSY79. This began in the mid-1990s with teenagers, and at this point there are very few observations on individuals in their 20's (Center for Human Resource Research, 2002, Chapter 3).

cell was exposed in each of three periods: ages 16-19, ages 20-24, and ages 25-29.[56]

The CPS ORG files start in 1979, but we use information on minimum wages going back to 1973. To avoid the potential confounding influences of the Vietnam War on youth labor markets we do not go back earlier than 1973, when the draft and U.S. involvement in the war ended. As a consequence, the only birth cohorts we can consider for 1979 are the cohorts that were age 16 or younger in 1973, or 22 or younger in 1979. Table 1 arrays the ages and years that are covered by our analysis. The first cohort-those aged 16 in 1973-are 22 in 1979, 23 in 1980, etc. The second cohort-those aged 16 in 1974-can be picked up at an age one year younger. And the seventh cohort-those aged 16 in 1979-can be covered for the full set of years.[57] Then, toward the end of the sample period, we lose observations on later cohorts at older ages. For example, the 29th cohort is 16 in 2001, and that is the last year of data for which they are covered. Of course, we do not have the actual longitudinal observations on members of these cohorts as they age. But we can infer the effects of minimum wages on these cohorts at different ages because the CPS repeatedly draws random samples from these cohorts as they age.[58]

Table 2 reports federal minimum wages for the sample period, and all state minimum wages that exceeded the federal minimum wage. The minimum is defined as of May of the calendar year; we chose this date because the greatest number of state minimum wage increases occurred in April (followed by January). Table 2 displays considerable variability in the level of state minimum wages, with some states in some

[56]We compute this only up to the current age. For example, for a 17 year-old we would use the minimum wage faced at ages 16 and 17.

[57]We explored the sensitivity of the results to using information on minimum wages back to 1966 and hence covering all cohorts back to age 16. The results were very similar.

[58]In principle we could look at individuals past age 29. However, for older individuals the minimum wage history that would be used to identify the effects of exposure to minimum wages at young ages comes from the early part of the sample period, when there was not much state variation in minimum wages. For example, looking at Table 1, the latest birth cohort of 34 year-olds is cohort 11, which left its teens by 1987, which is when most of the state variation in minimum wages began. In addition, as Table 1 illustrates, even with 22 years of data we would get relatively few complete sets of observations on cohorts for these older individuals all the way back to age 16.

years having a minimum only a shade higher than the federal minimum,[59] and other states and years with minimums exceeding the federal minimum by well over a dollar.

Table 3 provides some examples as to how this information is used to construct the average effective minimum wage to which an individual in a particular state-year-age cell was exposed. Although we use logs of minimum wages in our empirical analysis, Table 3 presents calculations for levels, to make the links to Table 2 more clear. The example covers Oregon for 1989, 1990, and 1991; a higher minimum wage was first enacted in 1990. We first show some representative computations for the minimum wage exposure measure for all ages (i.e., not distinguishing the age at which the exposure occurred). In 1989, the federal minimum wage was binding, and had been at $3.35 since 1981 (see Table 2). So for 16-24 year-olds the minimum wage measure is $3.35.[60] For 25 year-olds, the federal minimum was $3.35 for nine of the 10 years they were in the labor market (beginning at age 17), and $3.10 at age 16 (in 1980), so that average is shown. And for 29 year-olds, the average is computed over the 14 years beginning at age 16, and includes the earlier federal minimum wage from 1976-1979. In 1990 the higher state minimum wage of $4.25 takes effect, and each age group is exposed to this higher minimum for one year. For 16 year-olds the average effective state minimum wage is simply $4.25, for 17 year-olds the average of $4.25 and the federal minimum wage of $3.35 in the previous year, etc. Clearly this case will generate variation relative to a 16 year-old in a state in which the lower federal minimum wage prevails in 1990. The computation for 1991 follows the same logic. The last row provides examples of computing the minimum wage variable that distinguishes exposure by the age at which it occurred. For 20 year-olds in 1991, for example, the average effective minimum wage is the average computed over the

[59]For example, the state minimum wage in Connecticut was only one or two cents above the federal minimum from 1974 through 1987 as a result of legislation mandating a state minimum 0.5 percent above the federal level.

[60]For clarity, examples in table are done for nominal minimum wages. In the empirical analysis, all wage and earnings measures are converted into 2001 dollars.

federal minimum wage from 1987-1989 and the higher state minimum wage for 1990.

Table 4 reports descriptive statistics for the sample, for the three age groups studied. The number of observations for the whole sample for each age group comes from taking the number of times any single-year age appears in Table 1, multiplied by 51 (for the 50 states and Washington, D.C.). For example, between 1979 and 2001 ages 16 through 19 appear 92 times, which multiplied by 51 yields 4,692.

The four outcome variables we study are wages, employment, hours, and earnings. In studying weekly hours of work and weekly earnings, we do not condition on employment, so that we estimate the overall effects of minimum wages on hours and earnings. It is important to look at total hours and not just employment because employment could fall but hours conditional on employment rise, with ambiguous net effects on the total amount of labor hired.[61] Similarly, looking at earnings without conditioning on employment gives us a summary measure of the overall effects of minimum wages on workers' earnings.

The construction of wages, hours, and weekly earnings is sometimes complicated because of apparently bad data, missing data for those who report that they are working, etc. As a consequence, there are sometimes fewer valid observations on individuals for these outcomes.[62] However, in the full sample it turns out that in each single-year age group in each state and year there are always individuals with valid measurements on each outcome, so that the sample size for the data collapsed to state-year-age cells is always the same. But in analyses disaggregated by race (discussed later) this is not always the case.

[61] Past research has sometimes looked at outcomes such as hours conditional on employment, part-time status, etc., to explore whether employers respond to minimum wages by reducing employment but increasing hours, hence economizing on fixed costs of employment. Our interest in this paper, though, is in the overall benefits or costs to workers of minimum wages, so the unconditional estimates are the most pertinent. Some evidence on the effects of minimum wages on hours of work is reported in Gramlich (1976), Brown, et al. (1983), Cunningham (1981), and Michl (2000).

[62] In general, those who reported working last week or with a job but not working last week are considered as employed. Wages are treated as invalid if they are below one half of the federal minimum or above $100 (in 2001 dollars).

To provide some information on how the minimum wage variables vary, the descriptive statistics are broken down by whether or not the current state minimum wage exceeds the federal minimum. Not surprisingly, the average effective log state minimum wage since age 16 is higher in the group of observations in which the current state minimum exceeds the federal. For example, for teenagers the average value for this group is 1.75 (corresponding to a minimum wage in 2001 dollars of $5.72), compared with 1.68 (corresponding to a minimum wage of $5.42) for the other observations. The difference is a bit smaller for the average effective state minimum wage, because states currently bound by the federal minimum sometimes had higher state minimums in the past.[63]

Looking at the outcomes, for 16-19 year-olds employment is lower in the states with high minimum wages, and wages are higher, consistent with minimum wages raising wages and lowering employment contemporaneously. Hours are also lower, but weekly earnings are higher, suggesting that the wage gains offset the employment and hours reductions. The employment difference is smaller for the 20-24 year-old group, and for 25-29 year-olds employment is actually a shade higher in the states with high minimum wages, consistent with minimum wages having a stronger contemporaneous disemployment effect on younger individuals. Interestingly, though, the wage and earnings differences increase with age. Given that states with higher minimum wages in any period are more likely to have had high minimum wages in the past, this finding provides a hint that exposure to high minimum wages when young may adversely affect wages and earnings of 25-29 year-olds. However, these are only univariate comparisons that do not account for other factors controlled for in the regression estimates that follow.

The final row of the table shows the percentage at or below the minimum wage. Not surprisingly, of course, this percentage is highest for teenagers, consistent with

[63]The difference varies slightly across the age groups because of small differences in the years represented in each age group; see Table 1.

their lower wages, and falls sharply with age. Notice, also, that for each age group the percentage at or below the minimum is lower in the states with high minimum wages than in the other states, indicating that state minimum wages tend to be implemented in higher wage states. This is why, as explained below, it is important to control for persistent differences in the levels of wages (and other variables) across states. However, we verified that for high minimum wage states, relative to other periods for the same states when the minimum wage was non-existent (or lower), a higher minimum is associated with a higher percentage at or below the minimum (i.e., the within-state correlation is positive), which is the critical identifying information used in the empirical analysis.

Empirical Methods

We begin with simple specifications for the effects of the contemporaneous minimum wage on wages, employment, hours, and earnings, using specifications paralleling existing ones in the literature, of the form

$$Z_{ijt} = \alpha + \beta MW_{it} + S_i\theta_S + Yt\theta_Y + A_j\theta_A + \epsilon_{ijt}. \tag{29}$$

In equation 29, 'i' indexes states, 'j' indexes single-year age groups, and 't' indexes years. Z is alternatively: the log average wage of workers in the state-year-age cell; the percentage employed in the cell; the average hours worked of all individuals in the cell; and the log average weekly earnings of all individuals in the cell. MW is the log of the effective contemporaneous minimum wage (the higher of the state or federal minimum). S, Y, and A are vectors of state, year, and single-year age dummy variables, respectively. Controls are not included for productivity-related characteristics that are potentially endogenous, such as schooling, because we do not want to control for variation in characteristics that may be influenced by minimum wages; instead, we want to obtain reduced-form estimates that capture both direct effects on wages (for example), as well as indirect effects via the accumulation of skills.

The state dummy variables account for persistent state-level differences in the dependent variables (such as higher-wage states). The year dummy variables sweep out common changes across all states that could be driven by changes in aggregate economic conditions that are correlated with minimum wage changes. With the year dummy variables included, no identifying information comes from variation in the federal minimum wage. Instead, any effects of exposure to higher minimum wages are identified from variation in state minimum wages above the federal minimum.[64]

Observations within state-year cells for different single-year age groups may be non-independent, as, for example, state-level economic conditions affect age groups similarly. Furthermore, Bertrand, et al. (2002) have underscored the potential for understated standard errors in panel data sets when errors are positively serially correlated and the "treatment" (in this case the minimum wage) is positively serially correlated. This is not likely to be as severe a problem in our application, as there is less persistent variation in the minimum wage than in a dummy variable treatment that turns on and stays on for some states. Nonetheless, to flexibly allow for serial correlation as well as possible non-independence across age groups, we report standard errors that are robust to arbitrary correlation patterns among all observations for each state-i.e., across age or time-as well as arbitrary heteroscedasticity across states.[65]

[64]As noted earlier, most of the state-level variation in minimum wages begins in 1987. Earlier, state minimum wages also acted, in some cases, to extend coverage to workers not covered by federal minimum wages. But given that federal minimum wage coverage was nearly universal by 1979 (Brown, 1999)-or by 1985 if account is taken of coverage of state and local government workers-our estimates should be interpreted as largely identifying the effects of changes in minimum wage levels, rather than the effects of changes in coverage.

[65]Allowing correlation across time also accounts for the non-independence stemming from overlapping samples in the CPS. We experimented with more restrictive versions, including clustering only on state and year-which allows for arbitrary correlations across different age groups in the same state and year-and clustering on state and age-which allows arbitrary correlations across time on the same age group in the same state, but not correlations across different age groups. For the estimates reported in the tables, standard errors using these alternatives were always smaller, and often substantially so.

Equation 29 is estimated for three age groups: 16-19 year-olds; 20-24 year-olds; and 25-29 year-olds. Minimum wage research has usually focused on the first group-teenagers-as those most likely to be adversely affected by minimum wages, because teenagers have generally accumulated few skills and therefore are strongly over-represented among minimum wage workers. This conjecture can also be examined in the framework used here, when we estimate the effects of the current minimum wage. In contrast, the older group is unlikely to be affected by current minimum wages, but is of greater interest in looking at the effects of past exposure to high minimum wages, using the specifications explained next.

The first approach to estimating the longer-run effects of minimum wages is to substitute for the contemporaneous minimum wage variable in equation 29 a measure of the average effective log minimum wage to which the individual was exposed, beginning at age 16,

$$Z_{ijt} = \alpha + \gamma EXP_{it} + S_i\theta_S + Y_t\theta_Y + A_j\theta_A + \epsilon_{ijt}. \tag{30}$$

where EXP measures this minimum wage exposure.[66] This equation, too, is estimated for the three different age groups. In this specification γ identifies the effect of exposure to high minimum wages.[67] The inclusion of year effects removes the influence of common movements in the exposure variable generated by variation in the federal minimum.

Finally, equation 30 is modified by dropping the restriction that exposure to a

[66]Note that because equation 30 includes state dummy variables and is estimated for separate age groups, it allows for differences across states in the age profiles of the dependent variables. Thus, the findings we obtain cannot be attributable to fixed differences in these age profiles between states that are correlated with the minimum wage variable. Similarly, the inclusion of the year dummy variables allows for shifts in the age profiles of the dependent variables over time.

[67]We also estimated specifications with a contemporaneous minimum wage variable and the exposure variable. The conclusions were very similar but more complicated to present and interpret, in part because there are always two cases for the same level of exposure-with and without a current high minimum-and in part because the contemporaneous minimum wage variable, conditional on a given level of exposure, also captures information about the time pattern of exposure to a higher minimum.

higher minimum wage has equal effects on the dependent variables regardless of the age at which the exposure occurred. In particular, equation 30 is augmented to include separate measures of exposure during each age range. For example, for the 25-29 year-olds the specification is

$$Z_{ijt} = \alpha + \gamma 1 EXP_{it}^{1619} + \gamma_2 EXP_{it}^{2024} + \gamma_3 EXP_{it}^{2529} + S_i\theta_S + Y_t\theta_Y + A_j\theta_A + \epsilon_{ijt}. \quad (31)$$

The three variables EXP^{1619}, EXP^{2024}, and EXP^{2529} measure exposure to a higher minimum during the specified age ranges. When this specification is estimated for 20-24 year-olds, EXP^{2529} is of course dropped. For 16-19 year-olds we would also drop EXP^{2024}, in which case this specification would be equivalent to equation 30, reflecting the fact that the question of the effects of exposure at earlier ages for the youngest age group is nonsensical; consequently this specification is not estimated for 16-19 year-olds.

The motivation for specification 31 is straightforward. Whatever the consequences of minimum wages-reducing employment directly, lowering training, etc.-they are likely to be more severe when the minimum wage is more binding. Suppose, for example, that a negative effect on wages of exposure to a higher minimum wage stems from reduced labor market experience (which we cannot measure directly in the CPS). Any such reduction is likely to have been stronger if the exposure occurred when an individual was younger, rather than older, because the disemployment effects of minimum wages are likely to be strongest for the youngest and therefore least-skilled individuals (which turns out to be the case based on estimates of equation 29). Equation 31 therefore tests whether exposure to higher minimum wages when an individual was young indeed generates stronger longer-run effects.

Finally, we report estimates disaggregating the observations by race (looking at whites and blacks). It has often been conjectured that the effects of minimum wages on minorities will be stronger because their wage levels are lower-whether because of lower productivity or discrimination-and hence a minimum wage is more binding, although the existing literature on minimum wage employment effects (mainly older time-series studies) does little to establish stronger disemployment effects for minorities (Brown, 1999).[68] Here, though, we are asking a quite different question about minimum wages and we are using more recent data and state-level variation in minimum wages, so the race difference merits revisiting.

Results

Contemporaneous Minimum Wage Effects Estimated effects of contemporaneous minimum wages, based on equation 29, are reported in Table 5. The estimates in the first column are consistent with a positive and significant effect of minimum wages on wages of teenagers. Given that the log minimum wage gap between states with and without a higher minimum is 0.07, multiplying the estimated minimum wage coefficient by 0.07 yields the effect of an "average" higher state minimum wage. The estimated coefficient of 0.2216 therefore implies that imposing the average state minimum wage results in wages for teenagers that are higher by about 1.6 percent (0.222*0.07). Of course for this double-log specification the estimated elasticity is the coefficient estimate. Existing research has reported elasticities of wages near the minimum with respect to minimum wages and found higher values, but the 0.22 figure is an average across all teens-many of whom earn above the minimum wage.[69] No doubt reflecting in part the considerably lower share of 20-24 and 25-29 year-olds at the minimum, the estimated wage effects for these older age groups are smaller, near

[68]Linneman (1982) uses a different approach, and finds similar contemporaneous effects of minimum wages on earnings of blacks and whites.

[69]Neumark, et al. (2004) find a contemporaneous wage elasticity for minimum wage workers of around 0.8, falling to 0.4 or less for workers more than 10 percent above the minimum but still near the minimum.

zero, and statistically insignificant.

Column (2) reports estimates for employment. There is evidence of a significant negative employment effect for teenagers. With an estimated coefficient of -9.40, the implied elasticity is -0.20.[70] This elasticity is within the range of existing estimates of the elasticity of teen employment with respect to minimum wages. The estimates for the older groups are not statistically significant, and only for 20-24 year-olds is the estimate negative. Column (3) looks at hours worked. The hours effects parallel the employment effects, with the estimates indicating a significant negative effect only for teenagers (and an elasticity of -0.19).

Finally, column (4) looks at weekly earnings. Here, for teenagers especially, there are anticipated offsetting effects as higher wages compete with lower employment or hours. In fact this is borne out in the estimates, which suggest that a higher contemporaneous minimum wage has little or no effect on average earnings, either for teenagers or for the other age groups.

The estimates in Table 5 point to contemporaneous effects of minimum wages only on employment and hours of teenagers. This does not imply that there are not other adverse effects from minimum wages experienced by those aged 20-24. For example, Neumark and Wascher (2001) report stronger adverse effects of minimum wages on training of 20-24 year-olds than teenagers, and evidence presented later in this paper suggests that facing higher minimum wages in the older age range reduces completed schooling. These effects on training and schooling of those in their early 20's are not surprising, as these are ages at which jobs are more likely to entail training in the first place (as shown in Neumark and Wascher, 2001) and at which many individuals are still on the margin between staying in or leaving school. Thus, despite the evidence in Table 4, longer-run adverse effects of minimum wages could stem from exposure in

[70]The estimate of -9.40, multiplied by the average log minimum wage gap of 0.07, coupled with an average employment rate of 46.5 percent for teenagers, implies a 1.4 percent decline in employment, which dividing by the approximately 7 percent higher minimum yields the -0.20 elasticity.

the teens or the early 20's.

Cumulative Minimum Wage Exposure Estimates The estimates discussed thus far do not address the issue of the effects of cumulative exposure to a higher minimum, and hence the longer-run effects of the minimum wage. This issue is first taken up in Table 6, which reports estimates of equation 30 using the cumulative minimum wage exposure measure. To interpret the estimates, we calculate their implications for the effects of exposure since age 16 to a minimum wage that is on average higher by 0.06 log points. This is the difference between states with and without higher contemporaneous minimum wages faced by teens and young adults-as shown in the first row of estimates in Table 4-and it is exposure at these ages on which we focus most of our attention. Based on the estimates in Table 6, for 16-19 and 20-24 year-olds the implied effect of this exposure to a higher minimum wage is a 1.1-1.2 percent higher contemporaneous wage. But for 25-29 year-olds the estimated longer-run effect of exposure to a higher minimum is negative and larger, potentially reflecting longer-run cumulative effects of exposure to a higher minimum wage in the past; we return to this question below.

Turning to the employment and hours results, in columns (2) and (3), the estimated exposure effects are negative and statistically significant for all three age groups for both employment and hours (in some cases at the ten-percent level). For 20-24 year-olds, for example, the cumulative effect of exposure to a higher minimum wage since age 16 is to reduce hours by 0.36, or 1.4 percent.

Finally, column (4) reports the earnings effects. For teenagers there is a negative and insignificant effect. For 20-24 year-olds the estimated effect is small and insignificant. And for 25-29 year-olds the effect is negative, quite large, and statistically significant. For this oldest group, the estimates imply that an individual exposed to same average current difference in minimum wages we have used throughout earns 3.6

percent less because of exposure to a higher minimum wage.[71] Those older individuals exposed to a higher minimum wage also have lower employment and hours. These latter estimates suggest that by the time they reach the age when almost everyone has left school, detrimental effects of longer-term exposure to a high minimum wage become evident.

Exposure at Different Ages Given that minimum wages are more binding at younger ages, their contemporaneous effects should be greatest when individuals are young, and therefore their longer-run effects should be strongest for exposure at young ages. Estimates of equation 31, examining this issue, are reported in Table 7. To summarize briefly, although there is not much evidence of effects of past exposure to higher minimum wages on 20-24 year-olds, for 25-29 year-olds the evidence points quite clearly to adverse effects of exposure to higher minimum wages at younger ages. It makes sense that it is only for the older group that these effects become apparent, as 20-24 year-olds are often still enrolled in school (which may itself be influenced by the minimum wage), and if working are more likely to be observed well before the overtaking age. Both of these influences imply that for this younger group it will be more difficult to infer the effects of minimum wages. For example, if minimum wages deter training, then at very young ages during which workers receiving training are paid lower wages, adverse effects of minimum wages on wages would be obscured. And if minimum wages cause young adults to leave school earlier and to seek employment, then at ages when many are still in school an adverse employment effect of minimum wages would be masked. Consequently, we view the estimates for 25-29 year-olds as most informative.

[71]Table 2 indicates that Alaska, Connecticut, and the District of Columbia have had higher state minimum wages for the entire sample period, and for considerably longer than any other state. This raises the possibility that the effects of longer-run exposure to a higher minimum wage are disproportionately identified from these three states, which have to generate all of the variation at the high end of the distribution of years of exposure. However, dropping observations from the two states and the District of Columbia had little impact on the estimates, if anything strengthening the conclusions somewhat.

Looking first at wages, the estimates for exposure at younger ages (16-19 and 20-24) are negative and statistically significant in both cases. The estimates imply, for example, that exposure to the average higher minimum wage during ages 16-19 reduces "adult" wages by about 1.3 percent (0.06*0.215). The effects for exposure of 20-24 year-olds is a shade weaker. This evidence points to adverse longer-run effects on wages of exposure to high minimum wages during one's early years in the labor market. The evidence on employment and hours similarly points to adverse longer-run effects of exposure to a high minimum wage when young, with the estimates higher in absolute terms for exposure at ages 20-24. For 25-29 year-olds, exposure to the average higher minimum wage during ages 20-24 reduces hours by about 0.4 hour, or 1.3 percent.

The final column of Table 7 reports results for earnings. Here, again, there is strong evidence of negative longer-run effects of exposure to minimum wages as a teenager or young adult, as the estimated longer-run effects for exposure both as a teenager and a young adult are negative and statistically significant for 25-29 year-olds. For example, exposure to the average higher minimum wage, as a teenager, is estimated to reduce weekly earnings as an adult by 1.8 percent, and similar exposure as a 20-24 year-old to reduce weekly earnings by 2.3 percent. All told, the general pattern in these estimates is that exposure to higher minimum wages at younger ages has adverse longer-run effects on labor market outcomes. Later, we consider more carefully how these effects might arise.

Effects of Exposure by Race Next, we turn to results estimated separately for whites and blacks.[72] As a preliminary, Table 8 reports descriptive statistics for whites and blacks. These reveal lower average wages for blacks, especially at the older ages, as well as lower employment, hours, and earnings for blacks. Interestingly, for teenagers

[72]In all of our analyses using the CPS that do not distinguish by race, we include all observations. Here we include only whites and blacks.

the share of blacks at or below the minimum is lower than for whites. As indicated by the descriptive statistics at the top of the table for the state minimum wage gap, this is not because blacks and whites are exposed to very different minimum wages. Rather, the answer lies in the much lower employment rates for blacks, suggesting that far fewer blacks whose wages would be bound by the minimum remain in the workforce. Thus, the lower employment rate coupled with the lower wages of those blacks who do work is consistent with minimum wages being more binding for blacks because of their lower potential wages, despite the lower share black at the minimum; the regression results described below confirm this.

Panel A of Table 9 reports the contemporaneous specification (equation 29) for teenagers, simply to shed some light on race differences in minimum wage effects in these data using a specification paralleling much of the existing literature. The estimates are consistent with minimum wages being more binding for black teenagers, with a larger positive point estimate on wages of blacks, and larger negative point estimates on employment and hours of blacks, although the differences are not statistically significant. Stronger contemporaneous effects for black teenagers make it more likely that exposure to a higher minimum wage in the early years in the labor market will have more adverse longer-run effects for blacks, although these stronger adverse longer-run effects could arise regardless of differences in contemporaneous effects for teenagers, as discussed earlier.

The next specifications, in Panel B of Table 9, explore whether blacks are more adversely affected in the longer run by exposure to minimum wages, especially during the earliest years in the labor market. The evidence points quite clearly to more adverse longer-run effects of minimum wages for blacks. Focusing again on the estimates for 25-29 year-olds, we find that, for blacks, exposure to a higher minimum wage during ages 16-19 or 20-24 is associated with significant negative reductions in wages, employment, hours, and earnings. The estimates for whites are often about one-quarter to one-third as large, although still generally statistically significant.

A natural question is why there are such sharp differences between blacks and whites in the longer-run effects of exposure to a high minimum at young ages. The explanation presented thus far is simply that because minimum wages are more binding for blacks, their consequences should be more severe.[73] However, the racial differences in the longer-run effects, in Panel B, seem much more pronounced than the racial differences in the contemporaneous effects in Panel A, although the estimates for 20-24 year-olds in Panel B suggest that at these ages the contemporaneous adverse effects of minimum wages are more severe for blacks. A potential additional factor that may amplify the race differences is that minimum wages and the ensuing disemployment effects may lead to increased criminal activity.[74] Previous researchers have considered whether a higher minimum wage increases or reduces crime-especially property crimes or other crimes that provide illicit income-by teenagers and young adults. Theoretical predictions are ambiguous, as higher wages paid to some may deter crime, while reduced employment probabilities may increase it. Empirical evidence is also ambiguous.[75] But if minimum wages do lead to increased criminal activity among youths, and even more so if the criminal justice system generates harsher consequences of criminal behavior for blacks, then this may help to explain the sharper effects for blacks of exposure to high minimum wages as teenagers and young adults, as incarceration can result in the destruction of human capital and

[73]Given stronger disemployment effects of minimum wages for blacks, the longer-run impact of minimum wages would be amplified if returns to experience were higher for blacks than for whites. However, the evidence suggests that the opposite is probably the case (e.g., Oettinger, 1996).

[74]See Grogger (1998) for a discussion of research on race differences in participation in crime.

[75]Research on the economics of crime that tries to isolate the effect of exogenous variation in wages concludes that higher wages deter crime (e.g., Grogger, 1998). Research on the effects of the minimum wage, per se, is mixed. Time-series evidence in Hashimoto (1987) indicates that minimum wages increase property crimes but not violent crimes, while Chressanthis and Grimes (1990) suggest that the evidence is quite fragile. The specifications used by Chressanthis and Grimes are perhaps more suspect. For example, they include the school enrollment rate as a control variable despite the fact that it is an endogenous outcome when we are thinking about youth time allocation decisions in response to minimum wages. Nonetheless, the point remains that the conclusions are quite sensitive to model specification (see also Kallem, 2004).

criminal records can lead to subsequent labor market difficulties (e.g., Kling, et al., 2001). At this point, though, this explanation of the race differences in longer-run effects of minimum wages is speculative.

The results by race are inherently interesting given the focus on race differences in the effects of minimum wages in the earlier contemporaneous effects literature, and given worse labor market outcomes for blacks. But the race differences are also of interest for a more general reason. Specifically, by identifying two groups that should be differentially affected by longer-run exposure to high minimum wages, and finding evidence of stronger effects on the group for whom this would be expected (in this case, blacks), the race results provide additional evidence that the longer-run effects of minimum wages identified by our approach are causal. Essentially, the race differences provide a third level of differencing, relative to the difference-in-differences identification strategy that relies solely on the variation in exposure across time and states.

Exposure to Differing Economic Conditions Our estimates thus far have focused on the effects of a history of exposure to high minimum wages on wages, earnings, and work. However, the only history that we have included in our equations estimated thus far is the minimum wage history. The history of economic conditions to which one was exposed as a youth may also affect subsequent labor market outcomes,[76] and if this history is correlated with the minimum wages to which one was exposed when younger, then the preceding estimates may be biased. We therefore augment the specifications from Table 7, for 25-29 year-olds, with controls for exposure to unemployment rates.

There is a potential endogeneity problem because the dependent variables may be determined jointly with at least the more recent unemployment rates. But these

[76]Mroz and Savage (2003) study the longer-term effects of earlier spells of unemployment. See also Beaudry and DiNardo (1991).

unemployment rates are calculated for all ages to pick up general economic conditions, and therefore should not be much influenced by changes in employment, hours, etc., for a narrow age group. Moreover, we omitted the contemporaneous unemployment rate to avoid this problem. Nonetheless, if the higher minimum wage in earlier years contributed to the higher unemployment rate at the same time, then this specification may over-control for the minimum wage and hence understate its effect.

The estimates in Table 10 indicate that the history of unemployment rates to which individuals were exposed does in fact impact contemporaneous outcomes, with numerous cases (six out of eight) where higher past unemployment rates have negative effects on current wages, employment, hours, and earnings.[77] Moreover, the estimated minimum wage effects moderate as a result of the inclusion of the unemployment history. In particular, the wage and earnings effects of exposure to a higher minimum wage as a teenager fall by a third or more, while remaining significant (at the ten-percent level or better). The effects of exposure to a higher minimum wage at ages 20-24 do not fall nearly as much, declining by about one-quarter to one-third, with all of the estimates remaining statistically significant. We read the results as indicating that the evidence of adverse longer-run effects of exposure to a higher minimum wage in the early 20's is quite strong and robust, while that for exposure as a teen is a bit weaker. Finally, these findings also suggest that the longer-run adverse effects of minimum wages may be more attributable to the lasting impact of effects of minimum wages on training and schooling than on employment or hours, since-as shown in Table 5-there is little evidence of the latter effects for 20-24 year-olds.

Another relevant set of influences on young individuals' labor market experiences is changes in welfare and taxes. For example, as documented in Meyer and Rosenbaum (2001), the 1990s-and especially the late 1990s after welfare reform-witnessed sharp

[77]Note that the sample is a bit smaller here because prior to 1979 smaller states were not separately identified in the CPS and therefore unemployment rates by state are not always available for the earlier cohorts at young ages.

changes in welfare and tax policy that strongly affected work incentives among single mothers. It is unlikely that these drive our results. For 25-29 year-olds, in particular, very little identifying information comes from the late 1990s, as the sample ends in 2001 and we are estimating the effects of minimum wages many years earlier.[78]

The Minimum Wage "History" and Migration Next, we return to the potential mismeasurement of the minimum wage history faced by workers, given that this history is based on state of residence at the time they are observed. The implication of such measurement error is that as we look further back in time from the CPS observation on each individual, the minimum wage history is likely to be more error-ridden, and the estimated effects of exposure more biased toward zero. Thus, the evidence of negative effects of past exposure to higher minimum wages seems unlikely to be attributable to this measurement error.

Another possible source of bias pertaining to the minimum wage history is the endogenous choice of the current state of residence. Insofar as this choice is related to minimum wages, we would expect that individuals move so as to offset adverse effects of minimum wages or to take advantage of beneficial effects; that is, migration should arbitrage away some of the costs or benefits of higher minimum wages. Thus, for example, less-skilled teenagers or young adults in states with high minimum wages might be more likely to move to lower minimum wage states to try to offset whatever adverse effects on skill formation, etc., would be generated by exposure to a high minimum wage. A migration pattern like this would tend to understate negative effects of exposure to a higher minimum wage, given how we measure this exposure; another way to think about this is simply that endogenous migration generates a

[78]We also computed estimates separately for males and females. If the minimum wage effects we have found thus far represent effects of omitted changes in these types of policies, we might expect quite different results for men and women, with the effects more apparent for women. As it turns out, though, the evidence of longer run effects of minimum wages is relatively similar for males and females, and if anything somewhat stronger for males. While indirect, this is suggestive evidence that the minimum wage effects we detect do not primarily reflect these other changes in work incentives.

positive correlation between skill and minimum wages. Again, then, this source of bias seems unlikely to account for our findings.[79]

To address this issue more directly, we turned to data from the 1990 and 2000 Census of Population PUMS files, which include information on some measures related to skill or wages, age, and mobility between states. We looked at those aged 20-24 and 25-29 in 1990 or 2000, who were therefore teenagers or young adults five years earlier (age ranges for which the earlier estimates indicated adverse effects from exposure to high minimum wages), and identified those who had changed states of residence since five years ago; the share of such movers is about 13 percent for 20-24 year-olds and 16 percent for 25-29 year-olds.[80] We then matched these records to the effective minimum wage by state and year, and estimated a regression model for the change in the minimum wage associated with inter-state migration as a function of race, sex, ethnicity, and an indicator for education less than a high school degree. This tells us whether, among those who move between states, those with less skills or lower wages (whether because of skill or discrimination) exhibit a tendency to move to states with higher or lower minimum wages.

The estimates are reported in Table 11. With respect to education, sex (in the 1990 data), and race (in the 2000 data), characteristics associated with lower wages and skills are also associated with moves to states with lower minimum wage gaps.

[79]Another type of migration that may be relevant is illegal immigration into a state. This may be somewhat more likely to occur when minimum wages are high, because the high minimum makes such employees-for whom minimum wage laws may be more likely to be violated-more attractive to employers. If so, then the increased supply of unskilled workers in response to a high minimum wage may lead to worse labor market outcomes for legal workers, who may be more likely to be surveyed. However, this does not undermine the results reported thus far. It simply points to one of the responses to a higher minimum wage that may exacerbate the contemporaneous, and hence also the longer-run, effects of the minimum wage. Illegal immigrants are simply another input toward which employers may substitute in response to a higher minimum wage.

[80]In-state moves are also identified, although these are not relevant to our inquiry. We excluded those who lived abroad five years earlier. The non-movers are also potentially of interest, since that in itself may be viewed as a migration decision. However, we suspect that for many individuals staying in the same state is largely exogenous, and therefore want to avoid the relationship between wage- or skill-related measures and changes in the minimum wage that arise simply because of changes in the minimum wage in the state in which one resides.

For these three cases, then, the evidence is consistent with the conjecture that lower-wage or lower-skill workers, when they move, migrate to states with lower rather than higher minimum wages; such a migration pattern would if anything bias our earlier estimates against finding adverse effects of exposure to a high minimum wage as a teenager. Some of the result go the other way, however, in particular for Hispanics in both years, and for race in the 1990 data.[81] On balance, there is no reason to infer from these estimates that endogenous migration leads to overly strong adverse impacts of exposure to high minimum wages at young ages; across the different skill- or wage-related measures, the positive and negative effects on changes in the minimum wage gap associated with migration are roughly offsetting.

Accounting for the Longer-Run Effects of Minimum Wages The key evidence points to longer-run negative effects on earnings of exposure to higher minimum wages at earlier ages when minimum wages were more likely to have been binding. It is instructive to think about the magnitudes of the estimated earnings effects reported in Tables 7 and 10 to try to understand what might underlie the adverse longer-run effects of minimum wages that we find. Given the stronger results for exposure in the early 20's, we focus on the effects of exposure at these ages on 25-29 year-olds. We use the same type of calculation we have been using throughout. More specifically, in this section we consider the effects of exposure of 20-24 year-olds to an average minimum wage higher by 0.06 log points. Averaging the earnings estimates in Tables 7 and 10, the resulting coefficient (0.336) implies that exposure to this higher minimum wage through the 20-24 period reduces average earnings of 25-29 year-olds by 2.0 percent. This seems like a large effect, and it is therefore important to ask how much of it can be potentially explained by the different types of minimum wage effects suggested by the estimates reported in this paper or elsewhere in the existing literature; these are

[81] Of course the underlying story for Hispanics is potentially more complex because of the possible continuation of migration patterns beginning with migration into the United States.

limited to effects of minimum wages on current labor supply, experience, training, and schooling.[82]

The most direct effect would arise through lower current (i.e., adult) employment and hours stemming from exposure to a higher minimum wage earlier, of which there is evidence in columns (2) and (3) of Tables 7 and 10. For example, these effects may arise because the lower wage reduces labor supply. Using the average of the unconditional hours estimates in the two tables (yielding -5.734), which account for both employment and hours variation for the employed, the estimated effect on hours implies a 1.09 percent reduction in unconditional hours and hence a similar reduction in earnings.[83] Thus, the contemporaneous labor supply effect accounts for just over half of the earnings decline. Of course the source of the contemporaneous employment and hours decline cannot be determined by these data. It may in part reflect lower labor supply in the face of lower adult wages stemming from earlier exposure to a higher minimum wage, as well as other factors the accumulation of which makes those who were exposed to high minimum wages less likely to be employed or to have hours as high as other workers.

In addition to lower current employment and earnings, the estimates point to foregone labor market experience stemming from disemployment effects in earlier periods. The estimated contemporaneous effect of minimum wages on 20-24 year-olds from Table 7 implies that exposure to a higher minimum lowers unconditional hours of work for 20-24 year-olds by 0.65 percent. If each year of full-time experience is worth, say, four percent higher wages, then this implies 0.1 percent (0.04*0.0065*4) lower earnings, on average, for employed individuals, or 0.08 percentage point lower earnings unconditionally, which would account for another 4.0 percent of the earnings decline for 25-29 year-olds (0.08/2.0). Accounting for tenure effects would be expected

[82]We do not look at the implied effect of exposure to a higher minimum on wages as an "explanation" of the effect on earnings, but instead seek to understand the factors that can reduce both wages and employment or hours.

[83]Lower current hours could also help account for lower wages conditional on working.

to increase this effect, although it is difficult to say by how much since we do not know how this disemployment would have affected later tenure; arguably the effect would be relatively small because young workers change jobs frequently. Thus, foregone experience contributes a little bit, as well, to the overall earnings cost adults bear as a result of exposure to a high minimum wage as young adults.

The negative longer-run effects of minimum wages could also occur through decreased skill accumulation. There is evidence from CPS data that minimum wages reduces formal training for 20-24 year-olds (Neumark and Wascher, 2001, Table 3), with the evidence implying that a representative higher minimum (using the 7.3 percent figure from above) reduces the incidence of training by about 1.0 percentage points, or about 10 percent. With an estimated return to this training of about 18 percent, this implies an additional 0.18 percent (0.18*0.01) reduction in the average wage;[84] because this estimate comes from a sample that conditions on employment, given the employment rate for this age group this would translate into 0.14 percent reduction in average earnings, accounting for an additional 7.0 percent of the earnings decline.

Another avenue for skill reduction stemming from higher minimum wages comes through school enrollment. Here, rather than relying on past findings, we can simply adopt our regression framework to directly assess the longer-run effects of exposure to a higher minimum on schooling. As reported in Table 12, looking at both the percentage with a high school degree or higher level of educational attainment, and years of schooling,[85] the estimated effects of exposure as a 20-24 year-old (as well as a teen) on schooling of 25-29 year-olds were negative and statistically significant. Using the years of schooling estimate, the coefficient implies that the exposure we are considering reduces schooling by 0.072 years, which multiplied by a return to

[84] The training estimates used in this calculation are from Table 3 (average for column (2')), Table 2 (column (2)), and Table A1 (column (2')) of Neumark and Wascher (2001).

[85] It is less appropriate to estimate these models for younger individuals, for whom schooling may not be completed.

schooling of 0.07 implies an average 0.5 percent reduction in earnings conditional on employment or 0.4 percent unconditionally, accounting for another 20 percent of the earnings reduction.

The estimates discussed in this subsection are only intended to be suggestive. Adding them up, though, suggests that standard labor supply and human capital channels-such as lower current employment and hours, reduced training and schooling, and foregone work experience as a teenager-may be able to explain about 86 percent (1.09 + 0.08 + 0.14 + 0.4/2.0) of the longer-run effects of minimum wages that we find. In that sense, our estimates appear reasonable. Of course there may be additional influences generating the longer-run effects of minimum wages, such as the scarring effects of early non-employment that deter the formation of good work habits, a reputation as a good worker, labor market networks, etc. These types of influences can account for adverse effects on earnings that are not captured in the costs of foregone experience. Also, the returns to experience are identified largely from voluntary variation in experience, such as from when one leaves school. It is plausible, though, that spells of involuntary non-employment are more costly.[86]

[86]These findings and this conjecture are qualitatively consistent with work by Mroz and Savage (2003) indicating that-after accounting for heterogeneity that may generate a correlation between individuals' employment experiences at different ages-early spells of unemployment experienced by youths result in earnings losses that taper off only slowly over time, lowering earnings as much as 10 years later. However, they do not focus on minimum wage effects per se, and do not distinguish between voluntary and involuntary spells of unemployment. Ruhm (1991) reports evidence of long-term negative earnings effects associated with job displacement, which better captures involuntary spells of non-employment.

Conclusions

We study whether exposure to minimum wages at young ages leads to longer-run effects on labor market outcomes. Adverse longer-run effects could arise because of decreased labor market experience and accumulation of tenure, lower current labor supply, diminished training and skill formation (including schooling), and other influences, although there are also possible channels of positive longer-run effects. If minimum wages have longer-run negative effects, then an exclusive focus on short-run effects of minimum wages on youths-which characterizes nearly all of the existing research and policy debate on minimum wages-fails to capture a potentially harmful effect of minimum wages and one that may be more important from a policy perspective, both because the effects are persistent and because they fall on older individuals who are more likely to be primary breadwinners in their families.

We estimate the longer-run effects of minimum wages by using information on the minimum wage history that workers have faced since potentially entering the labor market at age 16. The evidence indicates that as individuals reach their late 20's, they earn less and may also work less the longer they were exposed to a higher minimum wage as a teen and young adult. Furthermore, the adverse longer-run effects of exposure to higher minimum wages when young are stronger for blacks, presumably reflecting in part, at least, the greater extent to which minimum wages are binding for these groups. In our view, this evidence indicates that it is important to focus on more than simply the contemporaneous effects of minimum wages on the youngest individuals, as this narrow and short-run focus may lead us to miss adverse minimum wage effects that are manifested in the longer run.

Table 1: Estimates of the Wage Elasticities of Care Supply from Previous Research

Couch et al. (1999), 1988 PSID				
Caregivers, Tobit	Wage Elasticity			
	Married couples	Single		
male	-0.22*	-0.69+		
female	-0.16	-0.58**		
Zissimopoulos (2001), 1994 HRS				
Care Recipients, Tobit		Wage Elasticity		
		Has sibling	No sibling	Spouse wage
from any child	-0.05*			
from male child		0.07	-0.07	0.07
from female child		-0.05	0.16	-0.02
Sloan et al. (2002), 1992 HRS				
Caregivers, hurdle in logs	Males+Females			
Two-part model (in logs)	Probit coefficient	OLS coefficient		
Care	0.18 (0.14)	-0.11 (0.13)		
Chores	-0.10 (0.09)	0.05 (0.06)		
Ioannides and Kann (1999), 1988 PSID				
Caregiving Households, Tobit				
	Wage Elasticity			
Husband wage	-0.02*			
Wife wage	-0.02+			
Sloan et al. (1997), 1989 NLTCS of Informal Caregivers				
Care Recipients, linear OLS corrected for selectivity				
	Mean Wage	Wage Elasticity		
	5	-0.26		
	10	-0.52		
	15	-0.78		

Notes: 1. Standard errors of the wage elasticities have not been presented in the reviewed studies and so signs near the estimates indicate the statistical significance of the wage effects, not the significance of the wage elasticities. 2. ** - significant at 1% level, * - significant at 5%, + - significant at 10%.

Table 2: Descriptive Statistics, Main HRS Sample of Working Potential Care Givers

	Males		Females	
Sample Size	1434		1358	
A. Dependent Variables				
Annual working hours	2290.15	(660.29)	1897.29	(653.51)
Annual care hours, unconditional	44.04	(162.20)	91.13	(312.69)
Annual care hours, conditional	163.18	(279.61)	250.00	(478.32)
Prevalence of caregiving	0.27		0.36	
Annual personal care hours, uncond	16.81	(105.78)	40.31	(235.42)
Annual personal care hours, cond	213.27	(317.65)	325.87	(597.28)
Annual chores hours, uncond	27.23	(91.35)	50.81	(160.23)
Annual chores care hours, cond	110.94	(157.33)	156.12	(250.05)
Net annual money transfer	-190.79	(2489.68)	-110.55	(2333.33)
B. Explanatory Variables				
Hourly Wage	20.77	(12.55)	14.59	(8.73)
Non-labor Income (capital income)	12.41	(227.12)	6.55	(17.11)
Age	57.11	(4.40)	56.21	(4.17)
Education	13.10	(3.07)	13.08	(2.52)
If non-white	0.14		0.18	
If hispanic	0.09		0.06	
If married	0.88		0.72	
Number of children < 6 years old	0.04	(0.25)	0.04	(0.23)
Number of children 6-18 years old	0.30	(0.70)	0.18	(0.53)
Number of siblings	3.92	(3.24)	3.61	(3.04)
Number of parents	1.64	(0.80)	1.50	(0.70)
Share of mothers	0.74	(0.34)	0.74	(0.35)
If single parent	0.97		0.96	
If parent with memory related disease	0.17		0.17	
Oldest parent's age	82.05	(6.48)	83.08	(6.12)
If at least one parent is poorer	0.48		0.43	
If at least one parent is richer	0.37		0.38	

Note: Numbers in the table are sample averages and numbers in parentheses are standard deviations.

Table 3: Labor Supply (Males)

	Labor Supply (1)	Informal Care Tobit (2)	Informal Care OLS (3)	Monetary Transfer (4)
Hourly Wage	68.27	-30.84	-16.13+	-13.09
	(42.45)	(25.75)	(8.65)	(112.16)
Non-labor Income	-0.05**	-0.11	-0.01	0.00
	(0.02)	(0.21)	(0.02)	(0.07)
Age	-34.61**	-4.16	-0.09	-8.62
	(4.53)	(3.40)	(1.12)	(28.94)
Education	19.60**	-4.59	-0.60	-30.17
	(7.00)	(5.23)	(1.70)	(25.68)
If non-white	63.42	-14.12	-0.90	384.29*
	(49.39)	(41.00)	(13.24)	(163.42)
If hispanic	3.27	-59.12	-19.83	211.33
	(65.53)	(54.72)	(17.16)	(139.56)
If married	101.03+	-55.19	-22.97+	-291.15**
	(58.81)	(41.11)	(13.78)	(108.25)
Children younger than 6	-13.36	-100.39+	-23.36	288.91**
	(60.80)	(59.84)	(17.42)	(106.00)
Children 6-18 years old	0.27	-13.73	-8.22	-378.82*
	(25.89)	(19.72)	(6.42)	(173.47)
Number of siblings	0.19	-9.13+	-1.34	10.67
	(5.93)	(4.74)	(1.54)	(24.23)
Number of parents	-22.67	85.26**	21.47**	-43.37
	(27.70)	(20.58)	(6.97)	(80.08)
Ratio of mothers to total	-38.63	40.28	3.65	-129.87
number of alive parents	(53.77)	(41.98)	(13.39)	(150.47)
If any single parent	-298.38*	193.24*	55.64*	370.71
	(122.16)	(83.77)	(27.62)	(673.04)
Any parent with memory disease	-78.62+	74.84*	1.05	18.36
	(45.71)	(33.65)	(0.74)	(147.70)
Oldest parent's age	3.77	6.52**	37.01**	2.13
	(2.94)	(2.30)	(11.83)	(12.77)
If at least one parent is	-72.38*	40.08	11.07	357.57**
poorer	(36.18)	(28.61)	(9.49)	(127.70)
If at least one parent is	-42.69	4.80	4.89	-412.47**
richer	(39.22)	(29.54)	(9.84)	(139.48)
Observations	1434	1434	1434	1434
Uncensored observations		387		
R-square/Chi-square	0.09	73.42**	0.04	0.04
Marginal wage effect at mean	68.27	-8.08	-16.13+	-13.09

Notes: 1. Additional covariates include region dummies. 2. Standard errors in parentheses.

Table 4: Labor Supply (Females)

	Labor Supply	Informal Care		Monetary Transfer
		Tobit	OLS	
	(1)	(2)	(3)	(4)
Hourly Wage	267.25**	-42.92	-6.88	174.47
	(44.18)	(44.10)	(18.88)	(145.46)
Non-labor Income	-1.66	-1.33	-0.54	-6.85*
	(1.17)	(1.21)	(0.51)	(3.31)
Age	-19.87**	-1.34	0.30	22.06
	(5.12)	(5.63)	(2.45)	(16.47)
Education	-5.62	-8.68	-7.68+	-46.66
	(8.89)	(9.35)	(4.07)	(31.16)
If non-white	-59.94	-27.43	-3.50	-73.78
	(45.70)	(56.44)	(24.16)	(216.67)
If hispanic	-62.60	123.17	82.51*	46.91
	(74.55)	(87.97)	(38.74)	(83.67)
If married	-196.83**	-165.80**	-47.71*	-184.21
	(43.66)	(48.08)	(21.02)	(127.89)
Children younger than 6	13.46	-113.61	-3.97	100.09
	(65.55)	(95.21)	(37.56)	(84.82)
Children 6-18 years old	-16.62	31.40	26.83	83.32
	(32.56)	(38.68)	(16.58)	(68.76)
Number of siblings	4.47	-16.75*	-5.65+	10.22
	(6.53)	(7.28)	(3.16)	(12.19)
Number of parents	52.03	129.91**	18.43	26.85
	(32.37)	(35.39)	(15.38)	(151.96)
Ratio of mothers to total	17.68	164.88**	30.18	-132.38
number of alive parents	(50.12)	(62.57)	(25.60)	(138.15)
If any single parent	236.48**	-45.06	-37.78	644.42
	(86.07)	(104.07)	(47.99)	(778.97)
Any parent with memory disease	24.47	87.62+	54.16*	130.79
	(49.16)	(51.53)	(23.26)	(184.07)
Oldest parent's age	-12.28**	13.03**	3.94*	-0.06
	(3.34)	(3.75)	(1.60)	(9.74)
If at least one parent is	-16.23	57.74	23.38	371.18**
poorer	(37.23)	(44.84)	(19.65)	(131.51)
If at least one parent is	-28.33	-38.02	1.50	-487.45**
richer	(40.85)	(46.05)	(20.05)	(133.37)
Observations	1358	1358	1358	1358
Uncensored observations		495		
R-square/Chi-square	0.11	67.12**	0.04	0.04
Marginal wage effect at mean	267.25**	-15.35	-6.88	174.47

See notes to Table 3.

Table 5: Informal Care Supply (Tobit, Males)

Annual Care Hours	(1) Tobit	(2) IV-1	(3) IV-2	(4) IV-3
Hourly Wage	-30.84	-331.05+	-259.96	-280.67+
	(25.75)	(199.09)	(190.44)	(157.07)
Non-labor Income	-0.11	-0.12	-0.12	-0.12
	(0.21)	(0.17)	(0.17)	(0.17)
Age	-4.16	-8.81+	-7.67+	-7.97+
	(3.40)	(4.71)	(4.55)	(4.26)
Education	-4.59	19.23	13.58	15.11
	(5.23)	(16.57)	(15.86)	(13.38)
If non-white	-14.12	-38.85	-33.17	-35.65
	(41.00)	(45.88)	(44.94)	(44.42)
If hispanic	-59.12	-98.33	-89.40	-91.52
	(54.72)	(62.61)	(61.40)	(59.91)
If married	-55.19	-26.91	-33.05	-30.65
	(41.11)	(46.92)	(45.93)	(45.03)
Children younger than 6	-100.39+	-104.01+	-102.94+	-101.99+
	(59.84)	(62.12)	(61.20)	(61.30)
Children 6-18 years old	-13.73	-9.82	-10.76	-10.02
	(19.72)	(20.88)	(20.46)	(20.52)
Number of siblings	-9.13+	-11.75*	-11.04*	-11.21*
	(4.74)	(5.27)	(5.14)	(5.08)
Number of parents	85.26**	92.73**	90.61**	91.23**
	(20.58)	(22.27)	(21.77)	(21.73)
Ratio of mothers to total	40.28	20.33	24.93	23.55
number of alive parents	(41.98)	(45.73)	(44.80)	(44.46)
If any single parent	193.24*	223.45*	214.68*	216.45*
	(83.77)	(90.00)	(87.92)	(87.70)
Any parent with memory disease	74.84*	92.69*	88.35*	89.79*
	(33.65)	(37.37)	(36.48)	(36.17)
Oldest parent's age	6.52**	6.93**	6.80**	6.82**
	(2.30)	(2.43)	(2.38)	(2.39)
If at least one parent is	40.08	68.05+	61.34+	62.75+
poorer	(28.61)	(35.23)	(34.32)	(32.83)
If at least one parent is	4.80	-19.10	-13.41	-15.44
richer	(29.54)	(34.77)	(33.92)	(33.03)
Observations	1434	1434	1434	1434
Uncensored observations	387	387	387	387
Chi-Square	73.42**	63.61**	64.93**	65.01**
Observed $P(y > 0)$		0.2699		
Pred. $P(y > 0 \| x)$		0.2621		
Marginal wage effect (at mean)	-8.08	-86.77	-68.14	-73.56

See notes to Table 3.

Table 6: Informal Care Supply (Least Squares, Males)

Annual Care Hours	(1) OLS	(2) IV-1	(3) IV-2	(4) IV-3
Hourly Wage	-16.13+	-59.28	-20.24	-39.43
	(8.65)	(43.12)	(56.13)	(43.95)
Non-labor Income	-0.01	-0.01**	-0.01*	-0.01**
	(0.02)	0.00	0.00	0.00
Age	-0.09	-0.78	-0.16	-0.46
	(1.12)	(1.08)	(1.23)	(1.12)
Education	-0.60	2.82	-0.28	1.25
	(1.70)	(3.58)	(4.56)	(3.54)
If non-white	-0.90	-4.43	-1.23	-2.81
	(13.24)	(11.90)	(11.59)	(11.02)
If hispanic	-19.83	-25.62*	-20.38+	-22.96*
	(17.16)	(11.19)	(12.36)	(11.41)
If married	-22.97+	-18.70	-22.57+	-20.66+
	(13.78)	(13.51)	(13.29)	(12.45)
Children younger than 6	-23.36	-23.83**	-23.40**	-23.61**
	(17.42)	(6.50)	(5.75)	(6.05)
Children 6-18 years old	-8.22	-7.56	-8.15+	-7.86+
	(6.42)	(4.60)	(4.59)	(4.58)
Number of siblings	-1.34	-1.72	-1.38	-1.55
	(1.54)	(1.22)	(1.20)	(1.21)
Number of parents	21.47**	22.52**	21.57**	22.03**
	(6.97)	(7.05)	(7.07)	(7.00)
Ratio of mothers to total	3.65	0.81	3.38	2.12
number of alive parents	(13.39)	(10.12)	(9.34)	(9.60)
If any single parent	55.64*	60.21**	56.08**	58.11**
	(27.62)	(18.60)	(18.64)	(18.44)
Any parent with memory disease	1.05	1.10+	1.05	1.08
	(0.74)	(0.67)	(0.65)	(0.66)
Oldest parent's age	37.01**	39.60*	37.26*	38.41*
	(11.83)	(18.14)	(17.35)	(17.65)
If at least one parent is	11.07	15.06	11.45	13.22
poorer	(9.49)	(9.23)	(9.91)	(8.87)
If at least one parent is	4.89	1.38	4.55	2.99
richer	(9.84)	(10.55)	(11.76)	(10.81)
Observations	1434	1434	1434	1434
R-squared/F-stat	0.04	2.47**	2.76**	2.69**
First stage F-stat		8.36	6.00	4.56
First stage partial R sq		0.0204	0.0211	0.0309
Hansen J-test from ivreg		0.8250	3.6250	6.0630

See notes to Table 3.

Table 7: Informal Care Supply (Tobit, Females)

Annual Care Hours	Tobit (1)	Tobit (2)	Tobit IV-3 (3)	Tobit IV-3 (4)
Males (N=1434)				
Log Hourly Wage	-30.84	-30.90	-280.67+	-294.71+
	(25.75)	(25.76)	(157.07)	(156.67)
Log Paid Home Care Price		-56.00		-80.76
		(178.04)		(186.14)
Females (N=1358)				
Log Hourly Wage	-42.92	-40.78	-715.35+	-518.90
	(44.10)	(44.24)	(381.05)	(380.49)
Log Paid Home Care Price		-160.94		46.10
		(284.95)		(344.25)

See notes to Table 3.

Table 8: Informal Care Supply (Least Squares, Females)

Paid Personal and Home Care	Males (1)	Females (2)
Log Paid Home Care Price	-206.38	-123.84
	(187.66)	(141.40)
Non-labor Income	0.13*	0.06
	(0.06)	(0.11)
Age	12.06**	12.58**
	(1.71)	(1.08)
Education	-14.05**	-4.42
	(3.75)	(2.78)
If non-white	24.97	73.63**
	(33.89)	(21.87)
If hispanic	8.78	30.83
	(49.77)	(33.57)
If married	-97.11**	-85.09**
	(28.36)	(21.26)
Children younger than 6	65.36	4.66
	(54.11)	(46.14)
Children 6-18 years old	-37.96	27.28
	(33.88)	(17.13)
If poor health	216.30**	252.22**
	(32.77)	(21.85)
Number of siblings	9.79	-5.64
	(8.13)	(9.88)
Number of parents	-9.74	-40.83
	(35.06)	(37.39)
Observations	8593	12024
Uncensored observations	128	375
Chi square	280.28**	761.42**
Observed $P(y > 0)$	0.0149	0.0312
Pred. $P(y > 0\|x)$	0.0045	0.0091
Marginal Effect	-0.93	-1.13
Price Elasticity	-0.5277	-0.2664

See notes to Table 3.

Table 9: Net Monetary Transfer (Males)

	Total Care Supply	Help with Basic Needs	Other Caregiving	Including Self-Employed	Excluding Respondents w/o Siblings
Males	N=1434	N=1434	N=1434	N=1765	N=1331
Basic estimates	-0.1835	-0.2124	-0.0749	-0.1831	-0.2018
IV-1	-1.9702+	-2.8654	-1.7742	-3.1101*	-2.5708+
IV-2	-1.5471	-1.5897	-1.6423	-2.5586*	-1.9981
IV-3	-1.6704+	-1.3421	-1.8146*	-2.1553*	-2.1227*
Females	N=1358	N=1358	N=1358	N=1546	N=1244
Basic estimates	-0.1696	-0.0960	-0.1569	0.0045	-0.2393
IV-1	-3.0562	0.6097	-5.7289*	-1.3782	-4.5995
IV-2	-6.1623*	-3.9501	-6.5523*	-4.9682*	-7.9939*
IV-3	-2.8259+	-1.1277	-3.2477*	-2.3638+	-3.9193*

See notes to Table 3.

Table 10: Net Monetary Transfer (Females)

	Total Care Supply	No parents coresidents	No parents in nursing home	No parents coresiding/ in nursing home
Males	N=1434	N=1372	N=1250	N=1191
Basic estimates	-0.1835	-0.0751	-0.2420	-0.1249
IV-1	-1.9702+	-1.4796	-2.5608*	-1.8222
IV-2	-1.5471	-1.4877	-2.0731+	-1.6078
IV-3	-1.6704+	-1.3261	-2.1646*	-1.6251
Females	N=1358	N=1271	N=1170	N=1086
Basic estimates	-0.1696	-0.1333	-0.1102	-0.0739
IV-1	-3.0562	-3.3526	-2.6146+	-3.0328
IV-2	-6.1623*	-6.4365	-7.0884*	-7.3205*
IV-3	-2.8259+	-3.2765	-2.7497*	-3.2347+

See notes to Table 3.

Table 11: Estimates of the Wage Elasticities

	Informal Care From 2STobit	Informal Care From 2SLS	Labor Supply	Monetary Transfer
Males (N=1434)				
Basic estimates	-0.1835	-0.3663+	0.0298	-0.0686
IV-1	-1.9702+	-1.3460	0.1783+	0.7282
IV-2	-1.5471	-0.4596	0.1823+	0.5561
IV-3	-1.6704+	-0.8953	0.1509+	0.7092
Females (N=1358)				
Basic estimates	-0.1685	-0.1770	0.1399**	1.5782
IV-1	-3.0367	-2.2334	0.3718+	-14.8005
IV-2	-6.1229*	-5.5244	0.5527*	-21.7132
IV-3	-2.8079+	-3.1479*	0.2835+	-19.8506+

Note: Signs near the estimates indicate the statistical significance of the wage effects.

Table 12: Results Controlling for the Price of Paid Home Care

Annual Care Hours	Tobit (1)	Tobit (2)	Tobit IV-3 (3)	Tobit IV-3 (4)
Males				
Log Hourly Wage	-30.84	-30.90	-280.67+	-294.71+
	(25.75)	(25.76)	(157.07)	(156.67)
Log Paid Home Care Price		-56.00		-80.76
		(178.04)		(186.14)
Females				
Log Hourly Wage	-42.92	-40.78	-715.35+	-518.90
	(44.10)	(44.24)	(381.05)	(380.49)
Log Paid Home Care Price		-160.94		46.10
		(284.95)		(344.25)

Notes: 1. Signs near the estimates indicate the statistical significance of the wage effects. 2. The result in columns (3) - (4) are from the IV Tobit regression using the most preferred IV-3 set of instruments.3.Mean wage of the Personal Care and Home Aides in the state is used as a proxy for the price of paid home care. 3. Omitted controls include all of the individual and parental characteristics, and region dummies used in the basic analysis.

Table 13: Demand for Paid Home Care: Own Price Effect

Paid Personal and Home Care	Males (1)	Females (2)
Log Paid Home Care Price	-206.38	-123.84
	(187.66)	(141.40)
Non-labor Income	0.13*	0.06
	(0.06)	(0.11)
Age	12.06**	12.58**
	(1.71)	(1.08)
Education	-14.05**	-4.42
	(3.75)	(2.78)
If non-white	24.97	73.63**
	(33.89)	(21.87)
If hispanic	8.78	30.83
	(49.77)	(33.57)
If married	-97.11**	-85.09**
	(28.36)	(21.26)
Children younger than 6	65.36	4.66
	(54.11)	(46.14)
Children 6-18 years old	-37.96	27.28
	(33.88)	(17.13)
If poor health	216.30**	252.22**
	(32.77)	(21.85)
Number of siblings	9.79	-5.64
	(8.13)	(9.88)
Number of parents	-9.74	-40.83
	(35.06)	(37.39)
Observations	8593	12024
Uncensored observations	128	375
Chi square	280.28**	761.42**
Observed $P(y > 0)$	0.0149	0.0312
Pred. $P(y > 0\|x)$	0.0045	0.0091
Marginal Effect	-0.93	-1.13
Price Elasticity	-0.5277	-0.2664

Note: Mean wage of the Personal Care and Home Aides in the state is used as a proxy for the price of paid home care.

Table 14: Extensions: Estimates of the Wage Elasticities of Care Supply

	Total Care Supply	Help with Basic Needs	Other Caregiving	Including Self-Employed	Excluding Respondents w/o Siblings
Males	N=1434	N=1434	N=1434	N=1765	N=1331
Basic estimates	-0.1835	-0.2124	-0.0749	-0.1831	-0.2018
IV-1	-1.9702+	-2.8654	-1.7742	-3.1101*	-2.5708+
IV-2	-1.5471	-1.5897	-1.6423	-2.5586*	-1.9981
IV-3	-1.6704+	-1.3421	-1.8146*	-2.1553*	-2.1227*
Females	N=1358	N=1358	N=1358	N=1546	N=1244
Basic estimates	-0.1696	-0.0960	-0.1569	0.0045	-0.2393
IV-1	-3.0562	0.6097	-5.7289*	-1.3782	-4.5995
IV-2	-6.1623*	-3.9501	-6.5523*	-4.9682*	-7.9939*
IV-3	-2.8259+	-1.1277	-3.2477*	-2.3638+	-3.9193*

See notes to Table 11.

Table 15: Extensions: Checking for the Effect of Parents' Residential Status

	Total Care Supply	No parents coresidents	No parents in nursing home	No parents coresiding/ in nursing home
Males	N=1434	N=1372	N=1250	N=1191
Basic estimates	-0.1835	-0.0751	-0.2420	-0.1249
IV-1	-1.9702+	-1.4796	-2.5608*	-1.8222
IV-2	-1.5471	-1.4877	-2.0731+	-1.6078
IV-3	-1.6704+	-1.3261	-2.1646*	-1.6251
Females	N=1358	N=1271	N=1170	N=1086
Basic estimates	-0.1696	-0.1333	-0.1102	-0.0739
IV-1	-3.0562	-3.3526	-2.6146+	-3.0328
IV-2	-6.1623*	-6.4365	-7.0884*	-7.3205*
IV-3	-2.8259+	-3.2765	-2.7497*	-3.2347+

See notes to Table 11.

Table 16: Examples of Monetary Transfer Measures Used in Empirical Research

Unit of analysis - adult child:		
1. Gross Transfer received	$y > 0$	Actual amount of transfer received for those who received any money from parents
	$y = 0$	Zero transfer for those who: – neither gave nor received any money – gave money to parents, but did not receive any money
2. Gross Transfer given	$y > 0$	Actual amount of transfer given for those who gave any money to parents
	$y = 0$	Zero transfer for those who: – neither gave nor received any money – received money from parents, but did not give any money
3. Positive Net Transfer received	$y > 0$	Transfers received minus transfers given, for those who received more than gave
	$y = 0$	Zero transfer for those who: – neither gave nor received any money – those who gave more money to parents than received from parents
4. Net Transfer received	$y > 0$	Transfers received minus transfers given, for those who received more than gave
	$y = 0$	Zero transfer for those who: – neither gave nor received any money – gave exactly the same amount of money to parents as was received from parents
	$y < 0$	Transfers given minus transfers received, for those who gave more than received
5. Net Transfer given		Is the opposite of net transfer received

Table 17: Summary of the Studies of The Wage Effect on Monetary Transfers

	Wage Effect		Wage Elasticity	
Couch et al. (1999), 1988 PSID, Children HHs, gross				
Linear-log simultaneous Tobit	Married couples	Single	Married couples	Single
male	1994**	1075+	1.17**	0.40+
female	722+	1309**	0.42+	2.44**
Zissimopoulos (2001), 1994 HRS, Parents' HHs, gross				
Log-log separate Tobit	Has sibling	No sibling	Has sibling	No sibling
from any child	0.28*		0.04*	
from male child	0.33	0.56	0.05	0.09
from female child	0.15	0.12	0.02	0.02
Sloan et al. (2002), 1992 HRS, Children, gross				
Cragg's two-part	Probit	OLS	Extensive	Intensive
from any child	0.27**	0.27**	2.60	0.27
Ioannides and Kann (1999), 1988 PSID, Children HHs, gross				
Separate Tobit				
Given:				
Husband wage	49*		0.33*	
Wife wage	63*		0.17*	
Received:				
Husband wage	-21		-0.11	
Wife wage	-10		-0.02	
Nizalova (2002), 1998 HRS, Children, net				
Linear-log separate OLS and IV				
males				
OLS	-13.09		-0.07	
IV-1	138.93		0.73	
IV-2	106.10		0.56	
IV-3	135.30		0.71	
females				
OLS	174.47		1.58	
IV-1	-1636.19		-14.80	
IV-2	-2400.39		-21.71	
IV-3	-2194.00+		-19.85+	

Table 18: Sample Description, HRS, 1992-2000

	Males		Females	
Variable	Mean	SD	Mean	SD
Number of cases	7673		7576	
Transfers:				
Recipients	5.27%		5.13%	
Net recipients	5.11%		4.94%	
Givers	13.51%		13.79%	
Net givers	13.00%		13.44%	
Net Transfer	-47.73	(2157.41)	-6.12	(1831.82)
Gross unconditional received	-253.13	(1872.14)	-205.61	(1518.83)
Gross conditional received	-4795.79	(6687.24)	-4004.47	(5457.59)
Gross unconditional given	205.40	(1057.74)	199.49	(1010.03)
Gross conditional given	1519.81	(2507.12)	1446.26	(2365.83)
Net unconditional received	-247.41	(1857.26)	-200.68	(1507.16)
Net conditional received	-4842.72	(6735.89)	-4065.03	(5511.80)
Net unconditional given	199.67	(1051.75)	194.55	(1002.97)
Net conditional given	1535.16	(2541.62)	1447.86	(2382.50)
Income:				
Capital income ($1K)	6.70	(100.13)	6.11	(20.10)
Hourly wage rate	20.31	(12.40)	14.27	(8.63)
Other characteristics:				
Age	56.72	(4.14)	54.97	(4.39)
Education	12.78	(3.19)	12.93	(2.59)
Number of children (0-5)	0.06	(0.29)	0.06	(0.30)
Number of children (6-18)	0.30	(0.68)	0.21	(0.57)
Number of siblings	3.96	(3.29)	3.68	(3.15)
If non-white	15.18%		18.45%	
If hispanic	9.19%		6.82%	
If married	91.14%		77.32%	
Parents' characteristics:				
Number of living parents	1.65	(0.79)	1.54	(0.73)
Maximum age	81.48	(6.57)	82.14	(6.20)
Percent of mothers	73.79%		74.67%	
If at least has memory related disease	5.75%		5.68%	
If at least one parent single	98.07%		97.06%	
If at least one is poorer	39.91%		37.59%	
If at least one is richer	41.31%		41.61%	

Table 19: Results from OLS Estimation Using Different Transfer Measures, Males

	(1) OLS-NGive	(2) OLS-GGive	(3) OLS-Grec
Log wage	49.83	72.17*	-22.33
	(59.94)	(30.21)	(50.70)
Non-labor income	-0.15	-0.05+	-0.10
	(0.13)	(0.03)	(0.14)
Age	4.58	2.63	1.95
	(7.85)	(2.88)	(7.20)
Education	10.67	19.18**	-8.50
	(12.05)	(6.18)	(10.15)
If married	-115.07	-109.09*	-5.99
	(78.95)	(46.78)	(61.99)
If non-white	354.22**	219.79**	134.43**
	(69.08)	(56.63)	(35.25)
If hispanic	40.04	57.15	-17.12
	(54.60)	(39.47)	(40.24)
Children (0-5)	-42.91	-52.08*	9.17
	(59.34)	(22.75)	(54.17)
Children (6-18)	-118.30+	13.03	-131.33*
	(64.56)	(27.34)	(57.08)
Number of siblings	-3.20	-6.42	3.22
	(9.81)	(4.93)	(8.22)
Number of parents	60.02	103.19**	-43.17
	(39.27)	(24.89)	(29.45)
Mothers/parents	-64.95	106.06**	-171.01**
	(70.72)	(31.96)	(62.14)
Parents' age	-6.90	-0.03	-6.87+
	(4.58)	(2.35)	(3.86)
Parents' education	-46.59**	(3.52)	-43.07**
	(11.58)	(6.57)	(9.25)
If at least one parent			
single	277.97	139.75	138.22
	(298.11)	(86.21)	(284.62)
has memory disease	-43.55	-15.31	-28.24
	(115.75)	(47.99)	(102.48)
is poorer	237.70**	168.17**	69.53
	(56.84)	(35.05)	(43.15)
is richer	-396.21**	-85.49**	-310.72**
	(58.95)	(28.69)	(50.21)
Observations	7673	7673	7673
R-squared	0.03	0.03	0.03

Notes: 1. For the details on the construction of the sample see Nizalova (2006). 2. Additional covariates include year dummies. Standard errors are cluster robust. 3. OLS-GRec refers to gross in-transfers HRS respondents receive from their elderly parents and is multiplied by (-1) to allow for easier comparison with the net transfers.

Table 20: Results from OLS Estimation Using Different Transfer Measures, Females

	(1) OLS-NGive	(2) OLS-GGive	(3) OLS-GRec
Log wage	83.43	79.90*	3.53
	(61.23)	(38.20)	(46.58)
Non-labor income	-1.21	0.39	-1.60
	(1.42)	(0.36)	(1.36)
Age	2.48	-1.35	3.83
	(7.97)	(3.85)	(6.87)
Education	-14.59	10.20	-24.7826**
	(12.13)	(9.13)	(7.79)
If married	32.33	-12.22	44.55
	(52.32)	(24.83)	(45.13)
If non-white	155.71**	88.02*	67.6917+
	(57.68)	(43.58)	(34.54)
If hispanic	129.30	217.13**	-87.83
	(92.09)	(65.93)	(60.85)
Children (0-5)	-67.16	-62.84**	-4.32
	(43.94)	(23.69)	(36.93)
Children (6-18)	24.29	47.78	-23.49
	(46.43)	(34.72)	(30.36)
Number of siblings	-0.48	-4.51	4.03
	(7.80)	(4.92)	(5.85)
Number of parents	116.60*	134.74**	-18.13
	(45.65)	(31.41)	(31.53)
Mothers/parents	-38.76	121.52**	-160.2807**
	(65.37)	(29.33)	(57.47)
Parents' age	-5.79	0.07	-5.86
	(4.71)	(2.87)	(3.64)
Parents' education	-23.12*	2.12	-25.2452**
	(10.12)	(5.85)	(7.95)
If at least one parent			
single	237.74+	136.22*	101.52
	(139.05)	(59.33)	(123.91)
has memory disease	17.87	46.12	-28.26
	(100.03)	(55.13)	(79.96)
is poorer	203.67**	122.17**	81.4974**
	(46.05)	(33.81)	(29.50)
is richer	-373.73**	-104.77**	-268.9546**
	(56.14)	(28.95)	(46.66)
Observations	7576	7576	7576
R-squared	0.03	0.03	0.02

See notes to Table 19.

Table 21: Estimates From The Three-part Model, Males 1992-2000

	(1)	(2)	(3)	(4)	(5)
	Multinomial Probit - Marginal Effects			OLS	
	Receiver	None	Giver	Net Transfer	
Predicted P(outcome)	0.0545	0.8216	0.1238	Positive	Negative
Log wage	-0.0085*	-0.0180+	0.0265**	337.48+	-841.80
	(0.0045)	(0.0102)	(0.0094)	(189.30)	(572.28)
Non-labor income	-0.0019**	-0.0006	0.0024*	-0.53	-44.38**
	(0.0006)	(0.0013)	(0.0012)	(1.04)	(14.71)
Age	0.00001	0.00003	-0.00003	-15.55	-144.33
	(0.0000)	(0.0000)	(0.0000)	(21.24)	(125.89)
Education	0.0021+	-0.0072**	0.0051**	61.48+	-82.71
	(0.0011)	(0.0023)	(0.0021)	(31.41)	(151.20)
If married	-0.0066	0.0581*	-0.0515**	-288.11	-339.51
	(0.0092)	(0.0203)	(0.0191)	(277.22)	(1108.89)
If non-white	-0.0192**	-0.0860**	0.1051**	363.86+	2,594.55**
	(0.0053)	(0.0188)	(0.0185)	(217.14)	(988.36)
If hispanic	-0.0069	-0.0842**	0.0912**	-31.60	2,633.76**
	(0.0096)	(0.0268)	(0.0262)	(190.55)	(990.25)
Children (0-5)	0.0039	-0.0129	0.0090	-308.41**	574.07
	(0.0075)	(0.0151)	(0.0137)	(112.15)	(689.91)
Children (6-18)	0.0088*	-0.0128+	0.0040	36.54	-571.30
	(0.0030)	(0.0074)	(0.0070)	(140.41)	(448.34)
Number of siblings	-0.0027**	0.0023	0.0004	-45.55	-123.16
	(0.0009)	(0.0017)	(0.0015)	(29.70)	(139.42)
Number of parents	0.0062*	-0.0566**	0.0504**	147.24	-362.02
	(0.0033)	(0.0075)	(0.0070)	(143.21)	(444.31)
Mothers/parents	0.0096	-0.0765**	0.0669**	225.53	-2,052.47*
	(0.0078)	(0.0176)	(0.0166)	(272.55)	(913.33)
Parents' age	0.0003	0.0001	-0.0004	7.33	-97.68
	(0.0004)	(0.0009)	(0.0008)	(14.10)	(75.32)
Parents' education	0.0054**	-0.0045*	-0.0009	-3.86	-251.14+
	(0.0009)	(0.0020)	(0.0019)	(40.04)	(130.68)
If at least one parent					
single	0.0090	-0.0553*	0.0463*	-198.44	-153.08
	(0.0122)	(0.0274)	(0.0240)	(510.32)	(1669.72)
has memory disease	0.0011	0.0002	-0.0013	193.61	373.52
	(0.0087)	(0.0192)	(0.0178)	(307.31)	(1355.91)
is poorer	-0.0023	-0.0980**	0.1003**	89.84	905.59
	(0.0049)	(0.0113)	(0.0107)	(205.26)	(732.64)
is richer	0.0486**	0.0097	-0.0583**	86.78	-708.71
	(0.0062)	(0.0110)	(0.0096)	(208.12)	(635.07)
Observations				998	392
R-squared				0.05	0.12

Notes: 1. Columns (2)-(4) provide dy/dx. 2. dy/dx for dummy variable is an effect of discrete change from 0 to 1. 3. See Notes to Table 19.

Table 22: Estimates From The Three-part Model, Females 1992-2000

	(1)	(2)	(3)	(4)	(5)
	Multinomial Probit - Marginal Effects			OLS	
	Receiver	None	Giver	Net Transfer	
Predicted P(outcome)	0.0450	0.8121	0.1428	Positive	Negative
Log wage	-0.0084	-0.0310**	0.0394**	162.90	-423.62
	(0.0052)	(0.0113)	(0.0104)	(232.83)	(605.24)
Non-labor income	0.0001	-0.0005*	0.0004*	-1.99	-18.03
	(0.0001)	(0.0002)	(0.0002)	(2.66)	(20.87)
Age	-0.0014*	0.0029*	-0.0015	3.91	-64.40
	(0.0007)	(0.0015)	(0.0013)	(25.51)	(105.82)
Education	0.0030*	-0.0068*	0.0038	25.04	-323.26*
	(0.0013)	(0.0029)	(0.0027)	(38.10)	(153.19)
If married	-0.0314**	0.0288*	0.0026	-106.17	-1,376.18*
	(0.0090)	(0.0138)	(0.0116)	(184.68)	(622.02)
If non-white	-0.0131+	-0.0646**	0.0777**	-80.36	1,275.94+
	(0.0071)	(0.0171)	(0.0162)	(186.86)	(685.67)
If hispanic	-0.0003	-0.1391**	0.1395**	216.09	110.19
	(0.0114)	(0.0314)	(0.0314)	(232.77)	(1406.34)
Children (0-5)	-0.0026	-0.002	0.0046	-399.15**	346.30
	(0.0095)	(0.0150)	(0.0126)	(131.82)	(827.29)
Children (6-18)	0.0049	-0.0085	0.0036	252.82	-2.52
	(0.0037)	(0.0090)	(0.0083)	(161.07)	(670.61)
Number of siblings	-0.0036**	0.001	0.0025	-62.18*	-166.64
	(0.0011)	(0.0019)	(0.0017)	(28.12)	(147.06)
Number of parents	0.0077+	-0.0459**	0.0382**	574.17**	-66.96
	(0.0041)	(0.0083)	(0.0076)	(172.41)	(543.02)
Mothers/parents	0.0098	-0.0979**	0.0881**	347.27	-1,706.08*
	(0.0098)	(0.0184)	(0.0167)	(225.41)	(840.28)
Parents' age	0.0003	-0.0009	0.0006	-9.00	-35.53
	(0.0005)	(0.0009)	(0.0008)	(18.86)	(67.46)
Parents' education	0.0043**	-0.0028	-0.0015	28.35	-80.71
	(0.0010)	(0.0021)	(0.0020)	(29.15)	(122.08)
If at least one parent					
single	0.0001	0.0056	-0.0057	894.76*	642.92
	(0.0112)	(0.0314)	(0.0302)	(358.66)	(1315.58)
has memory disease	0.0064	-0.0139	0.0075	515.71	-13.09
	(0.0106)	(0.0209)	(0.0195)	(331.81)	(1148.97)
is poorer	-0.0123*	-0.0777**	0.0896**	-166.43	865.57
	(0.0056)	(0.0120)	(0.0111)	(198.26)	(649.77)
is richer	0.0417**	0.0231*	-0.0647**	-87.13	-966.02+
	(0.0067)	(0.0115)	(0.0099)	(203.60)	(527.86)
Observations				1018	374
R-squared				0.05	0.11

See Notes to Table 19

Table 23: OLS vs. LAD Estimates at the Intensive Margin, Male Net Givers

	OLS	OLS-99%	LAD
	(1)	(2)	(3)
Log wage	337.48+	152.80	161.63**
	(189.30)	(108.52)	(39.04)
Non-labor income	-0.53	-0.44	2.19**
	(1.04)	(0.65)	(0.64)
Age	-15.55	-1.35	1.90
	(21.24)	(15.64)	(5.92)
Education	61.48+	21.42	6.40
	(31.41)	(19.99)	(7.59)
If married	-288.11	-339.04	-221.55**
	(277.22)	(223.48)	(72.34)
If non-white	363.86+	336.17*	143.12**
	(217.14)	(143.50)	(49.30)
If hispanic	-31.60	14.48	130.87+
	(190.55)	(153.81)	(67.36)
Children (0-5)	-308.41**	-235.90*	-120.99+
	(112.15)	(100.17)	(63.62)
Children (6-18)	36.54	11.65	59.16*
	(140.41)	(72.23)	(27.12)
Number of siblings	-45.55	-26.43	-8.99
	(29.70)	(20.68)	(6.53)
Number of parents	147.24	112.98	104.92**
	(143.21)	(96.63)	(30.72)
Mothers/parents	225.53	120.62	183.44*
	(272.55)	(215.41)	(75.55)
Parents' age	7.33	-5.90	6.36+
	(14.10)	(10.93)	(3.30)
Parents' education	-3.86	25.70	10.57
	(40.04)	(22.43)	(7.44)
If at least one parent			
single	-198.44	-241.79	-166.39
	(510.32)	(449.11)	(154.41)
has memory disease	193.61	22.29	174.47*
	(307.31)	(191.38)	(88.76)
is poorer	89.84	6.58	-4.34
	(205.26)	(123.00)	(45.39)
is richer	86.78	-57.83	-65.83
	(208.12)	(122.32)	(49.70)
Observations	998	985	998
R-squared	0.05	0.05	

See Notes to Table 21.

Table 24: OLS vs. LAD Estimates at the Intensive Margin, Male Net Recipients

	OLS	OLS-99%	LAD
	(4)	(5)	(6)
Log wage	-841.80	-660.63	-280.65
	(572.28)	(527.41)	(394.06)
Non-labor income	-44.38**	-48.84**	-71.55**
	(14.71)	(14.69)	(10.42)
Age	-144.33	-65.82	-79.11
	(125.89)	(98.77)	(68.66)
Education	-82.71	-79.55	-135.88
	(151.20)	(116.41)	(93.36)
If married	-339.51	-260.77	-794.35
	(1108.89)	(1078.26)	(772.33)
If non-white	2,594.55**	2,074.65*	1,569.72+
	(988.36)	(941.86)	(858.34)
If hispanic	2,633.76**	2,379.42**	884.08
	(990.25)	(837.43)	(1184.69)
Children (0-5)	574.07	290.22	-134.31
	(689.91)	(581.70)	(773.13)
Children (6-18)	-571.30	-478.69	-189.85
	(448.34)	(399.60)	(264.15)
Number of siblings	-123.16	-30.22	-5.72
	(139.42)	(112.31)	(87.15)
Number of parents	-362.02	-165.44	-53.96
	(444.31)	(411.76)	(334.20)
Mothers/parents	-2,052.47*	-1,532.96*	-200.69
	(913.33)	(728.76)	(704.32)
Parents' age	-97.68	-113.06+	-59.96
	(75.32)	(63.88)	(42.15)
Parents' education	-251.14+	-181.54+	(119.44)
	(130.68)	(104.60)	(92.05)
If at least one parent			
single	-153.08	862.34	114.28
	(1669.72)	(1694.71)	(1299.04)
has memory disease	373.52	-313.06	-196.71
	(1355.91)	(1201.86)	(871.68)
is poorer	905.59	692.87	500.69
	(732.64)	(616.82)	(522.11)
is richer	-708.71	-852.73	-582.12
	(635.07)	(565.50)	(510.76)
Observations	392	388	392
R-squared	0.12	0.15	

See Notes to Table 21.

Table 25: OLS vs. LAD Estimates at the Intensive Margin, Female Net Givers

	OLS (1)	OLS-99% (2)	LAD (3)
Log wage	162.90	212.61+	177.01**
	(232.83)	(121.56)	(64.14)
Non-labor income	-1.99	-0.33	1.75
	(2.66)	(2.18)	(1.52)
Age	3.91	-13.02	-4.56
	(25.51)	(17.35)	(9.25)
Education	25.04	-6.09	-14.17
	(38.10)	(27.28)	(13.11)
If married	-106.17	-220.80	-22.04
	(184.68)	(166.28)	(89.15)
If non-white	-80.36	-58.29	-2.06
	(186.86)	(138.90)	(78.18)
If hispanic	216.09	197.63	282.48**
	(232.77)	(168.72)	(100.85)
Children (0-5)	-399.15**	-265.94**	-122.85
	(131.82)	(90.08)	(90.91)
Children (6-18)	252.82	108.18	54.66
	(161.07)	(76.78)	(47.39)
Number of siblings	-62.18*	-36.57*	-15.31
	(28.12)	(17.56)	(10.21)
Number of parents	574.17**	290.92**	149.11**
	(172.41)	(95.69)	(52.11)
Mothers/parents	347.27	179.46	100.28
	(225.41)	(168.59)	(115.99)
Parents' age	-9.00	-8.08	4.58
	(18.86)	(12.56)	(5.63)
Parents' education	28.35	19.36	21.10*
	(29.15)	(17.27)	(10.58)
If at least one parent			
single	894.76*	606.41*	261.85
	(358.66)	(297.05)	(191.90)
has memory disease	515.71	364.88	138.92
	(331.81)	(261.85)	(142.76)
is poorer	-166.43	-152.50	-32.41
	(198.26)	(129.08)	(69.29)
is richer	-87.13	-132.95	-107.80
	(203.60)	(142.35)	(78.87)
Observations	1018	1006	1018
R-squared	0.05	0.04	

See Notes to Table 21.

Table 26: OLS vs. LAD Estimates at the Intensive Margin, Female Net Recipients

	OLS (4)	OLS-99% (5)	LAD (6)
Log wage	-423.62	-243.44	-86.23
	(605.24)	(576.59)	(407.49)
Non-labor income	-18.03	-24.71	-17.40
	(20.87)	(19.03)	(11.14)
Age	-64.40	-117.40	6.02
	(105.82)	(92.10)	(61.83)
Education	-323.26*	-388.63**	-213.66+
	(153.19)	(140.51)	(116.82)
If married	-1,376.18*	-980.41+	-760.94
	(622.02)	(578.01)	(513.44)
If non-white	1,275.94+	1,181.51+	781.68
	(685.67)	(639.86)	(685.59)
If hispanic	110.19	-169.61	-436.91
	(1406.34)	(1335.87)	(1111.35)
Children (0-5)	346.30	487.42	-829.73
	(827.29)	(746.07)	(982.74)
Children (6-18)	-2.52	173.37	111.04
	(670.61)	(572.56)	(420.28)
Number of siblings	-166.64	-60.88	-10.15
	(147.06)	(113.35)	(87.12)
Number of parents	-66.96	-323.29	431.12
	(543.02)	(497.39)	(362.22)
Mothers/parents	-1,706.08*	-877.15	-588.51
	(840.28)	(649.29)	(642.22)
Parents' age	-35.53	5.60	-1.77
	(67.46)	(55.45)	(42.65)
Parents' education	(80.71)	13.47	29.70
	(122.08)	(107.54)	(79.20)
If at least one parent			
single	642.92	275.17	1377.84
	(1315.58)	(1230.23)	(950.44)
has memory disease	-13.09	-586.52	-615.31
	(1148.97)	(1068.01)	(929.99)
is poorer	865.57	508.32	-174.14
	(649.77)	(570.41)	(563.80)
is richer	-966.02+	-788.90	-777.08
	(527.86)	(497.88)	(495.50)
Observations	374	371	374
R-squared	0.11	0.11	

See Notes to Table 21.

Table 27: Estimates of Marginal Wage Effects and Wage Elasticities

Estimated	Males 3-part model	Males OLS	Females 3-part model	Females OLS
E	**2.3443**	**1.0440**	**19.6663**	**13.6232**
		(1.4926)		**(57.8050)**
$P(y < 0 \| x)$	0.0545		0.0450	
$P(y = 0 \| x)$	0.8216		0.8121	
$P(y > 0 \| x)$	0.1238		0.1428	
$E(y \| x, y < 0)$	-4842.72		-4004.47	
$E(y \| x)$	-47.73		-6.12	
$E(y \| x, y > 0)$	1535.16		1447.86	
Enegext	-0.2656*		-0.2365+	
	(0.1405)		(0.1462)	
Eneg	0.1738		0.1042	
	(0.1176)		(0.1484)	
Ezeroext	-0.0210+		-0.0364**	
	(0.0119)		(0.0133)	
Eposext	0.2410**		0.3477**	
	(0.0848)		(0.0913)	
Epos	0.2198+		0.1125	
	(0.1207)		(0.1604)	

Table 28: Decomposition of Marginal Wage Effects and Wage Elasticities

	Males		Females	
Effect of 1% increase in wage	Absolute Change	Elasticity Components	Absolute Change	Elasticity Components
At the negative extensive margin	0.7014	1.4696	0.4266	6.9708
At the negative intensive margin	-0.4591	-0.9618	-0.1880	-3.0716
At the positive extensive margin	0.4581	0.9598	0.7190	11.7481
At the positive intensive margin	0.4178	0.8754	0.2326	3.8014
Total Effect	1.1183	2.3429	1.1903	19.4487

Table 29: Ages, Years, and Cohorts Used in Analysis

		Cohort:																										
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29
Year:	1979	22	21	20	19	18	17	16																						
	1980	23	22	21	20	19	18	17	16																					
	1981	24	23	22	21	20	19	18	17	16																				
	1982	25	24	23	22	21	20	19	18	17	16																			
	1983	26	25	24	23	22	21	20	19	18	17	16																		
	1984	27	26	25	24	23	22	21	20	19	18	17	16																	
	1985	28	27	26	25	24	23	22	21	20	19	18	17	16																
	1986	29	28	27	26	25	24	23	22	21	20	19	18	17	16															
	1987		29	28	27	26	25	24	23	22	21	20	19	18	17	16														
	1988			29	28	27	26	25	24	23	22	21	20	19	18	17	16													
	1989				29	28	27	26	25	24	23	22	21	20	19	18	17	16												
	1990					29	28	27	26	25	24	23	22	21	20	19	18	17	16											
	1991						29	28	27	26	25	24	23	22	21	20	19	18	17	16										
	1992							29	28	27	26	25	24	23	22	21	20	19	18	17	16									
	1993								29	28	27	26	25	24	23	22	21	20	19	18	17	16								
	1994									29	28	27	26	25	24	23	22	21	20	19	18	17	16							
	1995										29	28	27	26	25	24	23	22	21	20	19	18	17	16						
	1996											29	28	27	26	25	24	23	22	21	20	19	18	17	16					
	1997												29	28	27	26	25	24	23	22	21	20	19	18	17	16				
	1998													29	28	27	26	25	24	23	22	21	20	19	18	17	16			
	1999														29	28	27	26	25	24	23	22	21	20	19	18	17	16		
	2000															29	28	27	26	25	24	23	22	21	20	19	18	17	16	
	2001																29	28	27	26	25	24	23	22	21	20	19	18	17	16

Minimum wage data are used beginning as 1973, but monthly CPS ORG files are available beginning only in 1979. The analysis focuses on minimum wages from age 16 up to the present age (through age 29). Consequently, the youngest cohort we can study for

Table 30: Minimum Wages During Analysis Period

	Federal	Ak	CA	CT	DE	DC	HI	ID	IA	ME	MA	MN	NH	NJ	NY	OR	PA	RI	VT	WA
73	1.60	2.10		1.85		2.16				1.80	1.85			1.75	1.85					
74	2.00	2.50		2.01		2.19														
75	2.10	2.60		2.11		2.45								2.20						
76	2.30	2.80		2.31		2.55	2.40													
77	2.30	2.80	2.50	2.31		2.76	2.40							2.50						
78	2.65	3.15		2.66		2.79														
79	2.90	3.40		2.91		2.95														
80	3.10	3.60		3.12		3.14														
81	3.35	3.85		3.37		3.48														
82	3.35	3.85		3.37		3.62														
83	3.35	3.85		3.37		3.82														
84	3.35	3.85		3.37		3.82														
85	3.35	3.85		3.37		3.85				3.45										
86	3.35	3.85		3.37		3.86				3.55										
87	3.35	3.85		3.37		4.16				3.65	3.55		3.45					3.55	3.45	
88	3.35	3.85		3.75		4.33	3.85			3.65	3.65	3.55	3.55					3.65	3.55	
89	3.35	3.85	4.25	4.25		4.33	3.85			3.75	3.75	3.85	3.65				3.70	4.00	3.65	3.85
90	3.80	4.30	4.25	4.25		4.38	3.85		3.85	3.85		3.95				4.25		4.25	3.85	4.25
91	4.25	4.75		4.27		4.51										4.75		4.45		
92	4.25	4.75		4.27		4.55	4.75		4.65					5.05		4.75		4.45		
93	4.25	4.75		4.27		4.55	5.25		4.65					5.05		4.75		4.45		
94	4.25	4.75		4.27		5.25	5.25		4.65					5.05		4.75		4.45		4.90
95	4.25	4.75		4.27		5.25	5.25		4.65					5.05		4.75		4.45	4.50	4.90
96	4.25	4.75		4.27	4.65	5.25	5.25		4.65		4.75			5.05		4.75		4.45	4.75	4.90
97	4.75	5.25	5.00	4.77	5.00	5.75	5.25				5.25			5.05		5.50		5.15	5.00	4.90
98	5.15	5.65	5.75	5.18		6.15	5.25				5.25					6.00			5.25	
99	5.15	5.65	5.75	5.65	5.65	6.15	5.25				5.25					6.50			5.25	5.70
00	5.15	5.65	5.75	6.15	5.65	6.15	5.25				6.00					6.50		5.65	5.75	6.50
01	5.15	5.65	6.25	6.40	6.15	6.15	5.25				6.75					6.50		6.15	5.75	6.72

Table 31: Examples of Construction of Average Effective State Minimum Wage, Oregon

	1989	1990	1991
State minimum wage	–	4.25	4.75
Federal minimum wage	3.35	3.8	4.25
Average effective state minimum wage, all ages	*16-24*: 3.35 *25*: (1/10)3.10 + (9/10)3.35 *29*: (2/14)2.30 + (1/14)2.65 + (1/14)2.90 + (1/14)3.10 + (9/14)3.35	*16*: 4.25 *17*: (1/2)3.35 + (1/2)4.25 *18*: (2/3)3.35 + (1/3)4.25 *29*: (1/14)2.30 + (1/14)2.65 + (1/14)2.90 + (1/14)3.10 + (8/14)3.35 + (1/14)4.25	*16*: 4.75 *17*: (1/2)4.25 + (1/2)4.75 *18*: (1/3)3.35 + (1/3)4.25 + (1/3)4.75 *29*: (1/14)2.65 + (1/14)2.90 + (1/14)3.10 + (7/14)3.35 + (1/14)4.25 + (1/14)4.75
Average effective state minimum wage exposed to at ages 16-19	*20*: 3.35 *29*: (1/2)2.30 + (1/4)2.65 + (1/4)2.90	*20*: 3.35 *29*: (1/4)2.30 + (1/4)2.65 + (1/4)2.90 + (1/4)3.10	*20*: (1/4)3.80 + (3/4)3.35 *29*: (1/4)2.65 + (1/4)2.90 + (1/4)3.10 + (1/4)3.35

See Table 2 for the federal minimum wage series extending back to 1973.

Table 32: Summary Statistics

	16-19 year-olds			20-24 year-olds			25-29 year-olds		
	Whole sample	Current state $MW >$ federal	Current federal $MW >=$ state MW	Whole sample	Current state $MW >$ federal	Current federal $MW >=$ state MW	Whole sample	Current state $MW >$ federal	Current federal $MW >=$ state MW
Observations	4692	692	4000	5712	856	4856	4590	789	3801
Log of effective state	1.688	1.746	1.678	1.683	1.744	1.673	1.655	1.734	1.639
minimum wage, current	(0.092)	(0.087)	(0.089)	(0.088)	(0.085)	(0.084)	(0.068)	(0.077)	(0.052)
Average effective log of state	1.694	1.74	1.685	1.704	1.733	1.699	1.697	1.732	1.689
minimum wage since age 16	(0.092)	(0.087)	(0.091)	(0.087)	(0.083)	(0.088)	(0.068)	(0.068)	(0.066)
Employment (%)	46.45	44.96	46.7	70.43	69.68	70.56	78.84	79.04	78.8
	(14.59)	(15.52)	(14.41)	(8.91)	(10.42)	(8.62)	(6.50)	(6.82)	(6.43)
Wage ($2001)	6.54	7.1	6.44	9.15	9.96	9	12.34	13.72	12.06
unconditional	(0.96)	(1.16)	(0.88)	(1.49)	(1.74)	(1.40)	(1.79)	(1.91)	(1.62)
Weekly hours of work,	11.88	11.2	11.99	25.72	25.31	25.8	31.68	31.61	31.69
	(5.78)	(5.69)	(5.78	(4.47)	(5.05)	(4.36)	(3.14)	(3.27)	(3.11)
Weekly earnings,	85.43	86.87	85.18	247.63	265.22	244.53	404.65	448.62	395.53
unconditional	(49.34)	(52.47)	(48.79)	(70.02)	(80.37)	(67.57)	(74.09)	(79.91)	(69.43)
Percentage at or	12.71	10.57	13.08	7.14	6.46	7.26	3.32	3.23	3.34
below minimum wage	(6.79)	(6.41)	(6.78)	(4.47)	(4.34)	(4.49)	(2.58)	(2.72)	(2.55)

Each observation is the mean for the cells defined by state, year, and age (by single year), using the monthly CPS ORG files from 1979-2001. Means of these observations are reported, with standard errors of these means reported in parentheses. "Average effective state minimum wage since age 16" is calculated inclusive of age 16. In computing mean weekly earnings, observations on individuals employed but not reporting a wage were weighted by P(employed)/P(employed and wage reported). Without this weighting, mean earnings would be biased toward zero because data on wages are missing for the employed but not the non-employed. Minimum wages, wages, and earnings are converted to 2001 dollars based on the Consumer Price Index research series using current methods (CPI-U-RS); see http://www.bls.gov/cpi/cpiurstx.htm. Individual observations are weighted using CPS earnings weights.

Table 33: Estimated Effects of Current Log of State Minimum

	(1) Log(wage)	(2) Percent employed	(3) Hours	(4) Log (weekly earnings)
16-19				
Effective log state	0.2216**	-9.4008+	-2.2123	-0.0607
minimum wage, current	(0.0693)	(5.1854)	(1.4551)	(0.2390)
R2	0.78	0.86	0.91	0.91
20-24				
Effective log state	0.0102	-2.4059	-0.4954	0.0128
minimum wage, current	(0.0590)	(2.9786)	(1.4671)	(0.1004)
R2	0.8	0.64	0.74	0.82
25-29				
Effective log state	-0.0048	2.2494	1.0103	0.0258
minimum wage, current	(0.0554)	(2.8554)	(1.4064)	(0.0910)
R2	0.77	0.48	0.52	0.71

All estimates are from linear regressions with standard errors reported in parentheses. More details on the variables are given in Table 4. Standard errors are "clustered" by state, and hence are robust to arbitrary heteroscedasticity across states and arbitrary correlations across observations (distinguished by year or age) within states. A plus sign (+) indicates that estimate is statistically significant at the 10-percent level, a single asterisk (*) indicates that estimate is statistically significant at the five-percent level, and a double asterisk (**) indicates significance at the one-percent level. All regressions contain controls for age (single-year age dummy variables), year, and state. State-age-year observations are weighted by the number of observations in the cell, multiplied by the average CPS earnings weight of individuals in the state-year-age cell to correct for oversampling of individuals in small states.

Table 34: Estimated Effects of Current Log of State Minimum

	(1) Log(wage)	(2) Percent employed	(3) Hours	(4) Log (weekly earnings)
16-19				
Average effective				
log state minimum	0.1914**	-11.8109*	-2.4776*	-0.2783
wage since age 16	(0.0602)	(4.7417)	(1.2525)	(0.2364)
R2	0.78	0.86	0.91	0.91
20-24				
Average effective				
log state minimum	0.1944**	-11.3323**	-5.9440**	-0.0828
wage since age 16	(0.0666)	(4.3730)	(1.9027)	(0.1121)
R2	0.81	0.64	0.74	0.82
25-29				
Average effective				
log state minimum	-0.3192**	-19.2979**	-9.8015**	-0.5928**
wage since age 16	(0.1021)	(5.0667)	(2.5440)	(0.1440)
R2	0.77	0.48	0.53	0.71

See notes to Table 33.

Table 35: Estimated Effects of Average Effective Log State Minimum Wage by Age of Exposure

	(1) Log(wage)	(2) Percent employed	(3) Hours	(4) Log (weekly earnings)
20-24				
Average effective state	0.1067**	-3.6759	-2.1364+	-0.017
minimum wage, 16-19	-0.0331	-2.5398	-1.1383	-0.068
Average effective state	0.1033*	-3.6115	-2.7972+	0.0131
minimum wage, 20-24	-0.0519	-3.1615	-1.5123	-0.0933
R2	0.81	0.64	0.74	0.82
Observations	5712	5712	5712	5712
25-29				
Average effective state	-0.2150**	-5.7487*	-2.7991*	-0.3024**
minimum wage, 16-19	-0.0485	-2.7085	-1.3059	-0.0682
Average effective state	-0.1894**	-11.0518**	-6.6298**	-0.3807**
minimum wage, 20-24	-0.0456	-2.6619	-1.3428	-0.0726
Average effective state	0.0351	-1.649	-1.0418	0.001
minimum wage, 25-29	-0.045	-2.238	-1.1492	-0.0692
R2	0.77	0.48	0.53	0.71
Observations	4590	4590	4590	4590

See notes to Table 33.

Table 36: Summary Statistics for Whites and Blacks

	16-19 year-olds		20-24 year-olds		25-29 year-olds	
	Whites	**Blacks**	**Whites**	**Blacks**	**Whites**	**Blacks**
Observations	4691	3897	5712	4788	4590	3845
Log of effective state	1.688	1.689	1.683	1.684	1.655	1.655
minimum wage, current	(0.092)	(0.093)	(0.088)	(0.089)	(0.068)	(0.068)
Average effective log of state	1.694	1.694	1.703	1.705	1.697	1.698
minimum wage since age 16	(0.092)	(0.093)	(0.087)	(0.088)	(0.068)	(0.069)
Employment (%)	49.85	29.05	73.02	56.43	80.58	68.87
	(14.63)	(25.38)	(8.85)	(26.93)	(6.81)	(24.51)
Wage ($2001)	6.56	6.48	9.26	8.5	12.62	10.76
unconditional	(0.99)	(1.99)	(1.57)	(2.75)	(1.96)	(3.08)
Observations, wages	4677	3211	5712	4374	4590	3620
Weekly hours of work,	12.81	8.91	26.8	21.84	32.58	28.36
	(6.04)	(7.79)	(4.56)	(9.51)	(3.39)	(8.89)
Observations, hours	4677	3208	5712	4370	4590	3612
Weekly earnings,	92.17	62.92	260.9	194.26	425.72	314.31
unconditional	(52.11)	(69.01)	(74.18)	(112.73)	(86.45)	(142.31)
Observations, earnings	4677	3208	5712	4370	4590	3612
Percentage at or	13.44	8.88	7.12	7.29	3.18	3.84
below minimum wage	(7.69)	(14.86)	(4.82)	(14.34)	(2.65)	(10.59)

See notes to Table 32 for details. Samples are sometimes smaller because of absence of white or black respondents in a cell.

Table 37: Estimated Effects of Current State Minimum and Average State Minimum Wage by Age of Exposure, Whites

	(1) Log(wage)	(2) Percent employed	(3) Hours	(4) Log (weekly earnings)
20-24				
Average effective state	0.1171**	-2.7625	-1.4824	0.0175
minimum wage, 16-19	(0.0373)	(2.5750)	(1.2198)	(0.0717)
Average effective state	0.1115*	-0.6500	-1.4948	0.0797
minimum wage, 20-24	(0.0553)	(3.3735)	(1.6421)	(0.0923)
R2	0.79	0.56	0.7	0.8
25-29				
Average effective state	-0.2178**	-0.3870	-0.3206	-0.2376**
minimum wage, 16-19	(0.0508)	(2.7329)	(1.3556)	(0.0700)
Average effective state	-0.1872**	-7.0702*	-4.8671**	-0.3267**
minimum wage, 20-24	(0.0493)	(2.7535)	(1.4209)	(0.0758)
Average effective state	-0.0010	-0.9717	-0.5264	-0.0190
minimum wage, 25-29	(0.0524)	(2.3156)	(1.1771)	(0.0749)
R2	0.74	0.46	0.5	0.68

See notes to Table 35 and 36.

Table 38: Estimated Effects of Current State Minimum and Average State Minimum Wage by Age of Exposure, Blacks

	(1) Log(wage)	(2) Percent employed	(3) Hours	(4) Log (weekly earnings)
20-24				
Average effective state	0.1067**	-3.6759	-2.1364+	-0.0170
minimum wage, 16-19	(0.0331)	(2.5398)	(1.1383)	(0.0680)
Average effective state	0.1033*	-3.6115	-2.7972+	0.0131
minimum wage, 20-24	(0.0519)	(3.1615)	(1.5123)	(0.0933)
R2	0.81	0.64	0.74	0.82
25-29				
Average effective state	-0.2150**	-5.7487*	-2.7991*	-0.3024**
minimum wage, 16-19	(0.0485)	(2.7085)	(1.3059)	(0.0682)
Average effective state	-0.1894**	-11.0518**	-6.6298**	-0.3807**
minimum wage, 20-24	(0.0456)	(2.6619)	(1.3428)	(0.0726)
Average effective state	0.0351	-1.6490	-1.0418	0.0010
minimum wage, 25-29	(0.0450)	(2.2380)	(1.1492)	(0.0692)
R2	0.77	0.48	0.53	0.71

See notes to Table s35 and 36.

Table 39: Estimated Effects of Years of Exposure to Higher State Minimum Weighted by State Minimum Wage Gap, by Age of Exposure, Including Unemployment Rate Exposure Controls

	(1) Log(wage)	(2) Percent employed	(3) Hours	(4) Log (weekly earnings)
25-29				
Weighted exposure, 16-19	-0.0829+	-1.0426	-0.4219	-0.1012*
	(0.0463)	(2.8212)	(1.3118)	(0.0617)
Weighted exposure, 20-24	-0.1468**	-7.6701**	-4.8380**	-0.2908**
	(0.0459)	(2.7586)	(1.3528)	(0.0720)
Weighted exposure, 25-29	-0.0340	-4.2832+	-2.3793*	-0.1072
	(0.0428)	(2.2731)	(1.1284)	(0.0660)
Average UR, 16-19	-0.0061**	0.0904	0.0590+	-0.0049**
	(0.0012)	(0.0734)	(0.0348)	(0.0017)
Average UR, 20-24	-0.0161**	-0.4409**	-0.2170**	-0.0226**
	(0.0015)	(0.0760)	(0.0354)	(0.0019)
R2	0.79	0.49	0.54	0.73
Observations	4590	4590	4590	4590

See notes to Table 35.

Table 40: Relationship Between Wage- and Skill-Related Variables and Change in Minimum Wage for Movers, 25-29 Year-Olds

	Change in log of effective minimum wage			
	1990 PUMS		2000 PUMS	
	(1)	(2)	(3)	(4)
	20-24	**25-29**	**20-24**	**25-29**
Education less than high school	-0.0024**	-0.0023**	-0.0024**	-0.0043**
	(0.0004)	(0.0004)	(0.0008)	(0.0008)
Female	-0.0009**	-0.0011**	0.0004	-0.0003
	(0.0003)	(0.0002)	(0.0005)	(0.0004)
Non-white	0.0013**	0.0016**	-0.0027**	-0.0020**
	(0.0004)	(0.0004)	(0.0006)	(0.0005)
Hispanic	0.0096**	0.0080**	0.0048**	0.0020**
	(0.0006)	(0.0005)	(0.0008)	(0.0008)
Observations	100,454	149,668	98,560	134,483
R2	0.0003	0.0019	0.0005	0.0004
Share of movers in age group	0.1283	0.1572	0.1303	0.1613
Change in log effective	-0.0336	-0.0341	0.0900	0.0913
minimum wage, mean	(0.0448)	(0.0448)	(0.0751)	(0.0754)

Sample used in estimation excludes individuals who lived abroad 5 years ago. A single asterisk (*) indicates that estimate is statistically significant at the five-percent level, and a double asterisk (**) indicates significance at the one-percent level. The five-percent PUMS samples are used.

Table 41: Estimated Effects on Schooling of Average State Minimum Wage by Age of Exposure

	(1) Percentage with high school degree or higher education	(2) Years of schooling
25-29		
Average effective state	-7.0313**	-0.8145**
minimum wage, 16-19	(2.2168)	(0.2093)
Average effective state	-7.4778**	-1.1940**
minimum wage, 20-24	(2.4633)	(0.2385)
Average effective state	0.7907	-0.0359
minimum wage, 25-29	(2.3264)	(0.2185)
R2	0.61	0.79

For 1979-1991 high school degree is based on years of schooling, and for 1992-2001 on whether a high school diploma (or equivalent) was earned (Jaeger, 1997). (Before 1997, it was not possible in the CPS to distinguish high school graduates from those with a GED; see Clark and Jaeger, 2002.)

Appendix A

Kuhn-Tucker Conditions and Some Intermediate Results.

Solving the R's optimization problem described earlier and allowing for corner solutions, gives the following four first-order conditions, first two of which are equalities and the last two are Kuhn-Tucker conditions allowing for the corner solutions for the market-purchased time and caregiving time:

$$U_X^G w - U_l^G = 0 \tag{32}$$

$$-U_X^G + U_{U^R}^G U_X^R = 0 \tag{33}$$

$$-U_X^R p_t + U_Z^R Z_{tm}^R < 0 \quad and \quad t_m = 0 \tag{34}$$

or

$$-U_X^R p_t + U_Z^R Z_{tm}^R = 0 \quad and \quad t_m > 0$$

$$-U_l^G + U_{U^R}^G U_Z^R Z_{tg}^R < 0 \quad and \quad t_g = 0 \tag{35}$$

or

$$-U_l^G + U_{U^R}^G U_Z^R Z_{tg}^R = 0 \quad and \quad t_g > 0$$

$$U_l^G \theta - U_{U^R}^G U_l^R < 0 \quad and \quad t_r = 0 \tag{36}$$

or

$$U_l^G \theta - U_{U^R}^G U_l^R = 0 \quad and \quad t_r > 0$$

Analyzing these expressions produces the following conditions:

1. $t_m = 0, t_g = 0$ if and only if

$$\frac{U_Z^R \gamma}{U_X^R} < p_t \tag{37}$$

and

$$\frac{U_Z^R Z_{tg}^R}{U_X^G} < w \tag{38}$$

2. $t_m > 0, t_g = 0$ if and only if

$$Z_{tg}^R|_{tg=0} < \frac{w}{p_t} Z_{tm}^R \tag{39}$$

3. $t_m = 0, t_g > 0$ if and only if

$$Z_{tg}^R > \frac{w}{p_t} Z_{tm}^R \quad \forall\ t_g \tag{40}$$

4. $t_m > 0, t_g > 0$ if and only if

$$Z_{tg}^R = \frac{w}{p_t} Z_{tm}^R \tag{41}$$

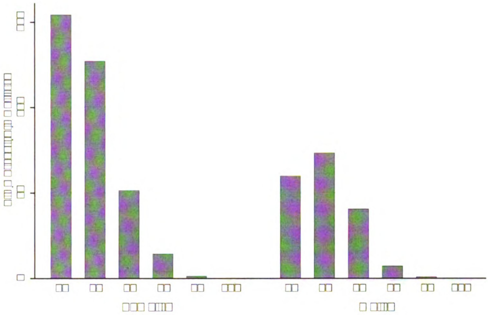

Figure A1. Time Transfer to Adult Children By Age and Gender

Table A1. Sample Selection Criteria

	Males		Females	
Initial Sample Interviewed in 1998	9669		13212	
If non-missing state of residence	8704	90.02%	12139	91.88%
If Age Eligible	6566	67.91%	8151	61.69%
Potential Caregivers	3305	34.18%	3318	25.11%
If not retired in 1998	2425	25.08%	2748	20.80%
If working and 0 <wage reported<100	1817	18.79%	1617	12.24%
If 0 <annual working hours<5200	1773	18.34%	1568	11.87%
If non-missing respondent's characteristics	1769	18.30%	1554	11.76%
If parents' info is non-missing	1765	18.25%	1546	11.70%
If non-self employed	1434	14.83%	1358	10.28%

Table A2. Descriptive Statistics (including those not at risk of caregiving)

	Males		Females	
Sample Size	2242		2505	
A. Dependent Variables				
Annual working hours	2201.52	(707.95)	1810.07	(674.90)
B. Explanatory Variables				
Hourly Wage	19.33	(12.09)	13.69	(8.45)
Other income	23.97	(183.89)	24.08	(40.53)
Non-labor Income	9.86	(181.90)	5.97	(16.92)
Age	58.63	(5.29)	58.16	(5.19)
Education	12.84	(3.14)	12.80	(2.62)
If non-white	0.16		0.19	
If hispanic	0.09		0.06	
If married	0.83		0.62	
Children younger than 6	0.04	(0.25)	0.04	(0.22)
Children 6-18 years old	0.25	(0.64)	0.18	(0.56)

Note: Numbers in the table are sample averages and numbers in parentheses are standard deviations.

Table A3. CPS Descriptive Statistics, 1992-2001

	Males		Females	
Age 18+				
Sample Size	316263		295786	
Working hours	2001.26	(732.13)	1658.75	(738.27)
Hourly Wage	19.11	(13.52)	13.18	(9.66)
Other income	26.56	(35.73)	37.65	(44.92)
Age	38.93	(12.99)	38.83	(12.91)
Education	12.95	(2.82)	13.11	(2.47)
If non-white	0.13		0.15	
If hispanic	0.15		0.13	
If married	0.64		0.60	
Children <6	0.26	(0.59)	0.23	(0.54)
Children 6-18	0.53	(0.91)	0.56	(0.91)
Age 18-49				
Sample Size	264668		248000	
Working hours	1986.19	(739.87)	1642.95	(742.72)
Hourly Wage	18.31	(13.04)	12.97	(9.55)
Other income	25.91	(36.17)	36.86	(43.82)
Age	35.55	(11.35)	35.48	(11.26)
Education	12.95	(2.73)	13.17	(2.42)
If non-white	0.13		0.15	
If hispanic	0.16		0.13	
If married	0.60		0.58	
Children <6	0.30	(0.63)	0.28	(0.58)
Children 6-18	0.59	(0.95)	0.65	(0.95)
Age 50+				
Sample Size	51595		47786	
Working hours	2078.56	(685.84)	1740.75	(709.08)
Hourly Wage	23.22	(15.07)	14.27	(10.14)
Other income	29.91	(33.24)	41.74	(50.03)
Age	56.26	(3.85)	56.17	(3.84)
Education	12.95	(3.22)	12.85	(2.72)
If non-white	0.11		0.13	
If hispanic	0.10		0.09	
If married	0.84		0.67	
Children <6	0.02	(0.16)	0.01	(0.12)
Children 6-18	0.21	(0.57)	0.09	(0.36)

See Notes to Table A2.

Table A4. CPS Descriptive Statistics, 1998

	Males		Females	
Age 18+				
Sample Size	29733		27995	
Working hours	2022.88	(725.29)	1673.84	(733.77)
Hourly Wage	19.14	(13.72)	13.28	(9.83)
Other income	27.56	(40.49)	38.96	(50.98)
Age	39.12	(12.88)	39.13	(12.93)
Education	12.98	(2.80)	13.16	(2.49)
If non-white	0.13		0.15	
If hispanic	0.16		0.13	
If married	0.64		0.59	
Children <6	0.25	(0.59)	0.23	(0.55)
Children 6-18	0.53	(0.91)	0.57	(0.92)
Age 18-49				
Sample Size	24785		23316	
Working hours	2008.37	(733.75)	1655.80	(738.13)
Hourly Wage	18.30	(13.14)	13.04	(9.72)
Other income	26.81	(41.00)	37.85	(49.47)
Age	35.72	(11.26)	35.73	(11.33)
Education	12.97	(2.71)	13.19	(2.45)
If non-white	0.13		0.15	
If hispanic	0.17		0.14	
If married	0.60		0.58	
Children <6	0.30	(0.63)	0.28	(0.59)
Children 6-18	0.60	(0.95)	0.67	(0.97)
Age 50+				
Sample Size	4948		4679	
Working hours	2095.58	(676.76)	1763.75	(704.86)
Hourly Wage	23.35	(15.66)	14.47	(10.25)
Other income	31.33	(37.62)	44.45	(57.63)
Age	56.12	(3.83)	56.08	(3.81)
Education	13.03	(3.18)	12.98	(2.71)
If non-white	0.11		0.13	
If hispanic	0.11		0.09	
If married	0.84		0.67	
Children <6	0.02	(0.16)	0.01	(0.11)
Children 6-18	0.20	(0.57)	0.09	(0.37)

See Notes to Table A2.

Table A5. Estimates of Wage Elasticity of Labor Supply, All Working Males

	Wage Elasticity	1st F-stat	Partial R-sq	Hansen J-stat
A. HRS, all working individuals				
1998	0.0481**		N=2242	
IV-1	0.1361	10.19	0.0153	3.86
IV-2	0.1668+	6.85	0.0154	3.26
IV-3	0.1697*	5.66	0.0246	9.58
IV-4	0.3559**	13.97	0.0382	31.70**
B. CPS: 50+ years old				
1991-2001	0.1863**		N=316263	
IV-1	0.1680**	131.64	0.0017	153.03**
IV-2	0.1510**	95.09	0.0018	188.71**
IV-3	0.1943**	67.9	0.0024	368.55**
IV-4	0.6007**	4168.5	0.0927	866.17**
1998	0.1802**		N=29733	
IV-1	0.1848**	17.38	0.0025	5.338
IV-2	0.2010**	13.55	0.0028	7.808
IV-3	0.1503**	8.67	0.0033	26.90**
IV-4	0.6248**	373.81	0.0891	97.76**
C. CPS: 18+ years old				
1991-2001 CPS	0.1863**		N=316263	
IV-1	0.1943**	67.9	0.0024	368.55**
IV-2	0.1680**	131.64	0.0017	153.03**
IV-3	0.1510**	95.09	0.0018	188.71**
IV-4	0.6007**	4168.5	0.0927	866.17**
1998-CPS	0.1802**		N=29733	
IV-1	0.6248**	373.81	0.0891	97.76**
IV-2	0.1848**	17.38	0.0025	5.338
IV-3	0.2010**	13.55	0.0028	7.808
IV-4	0.1503**	8.67	0.0033	26.90**
D. CPS: 18-49 years old				
1991-2001 CPS	0.1934**		N=264668	
IV-1	0.1645**	116.78	0.0018	129.79**
IV-2	0.1609**	88.96	0.002	159.55**
IV-3	0.2079**	64.42	0.0027	325.01**
IV-4	0.6203**	3761.51	0.1073	945.52**
1998-CPS	0.1926**		N=24785	
IV-1	0.1916**	14.34	0.0024	6.95+
IV-2	0.2262**	10.34	0.0025	10.86+
IV-3	0.1770**	8.18	0.0037	26.63**
IV-4	0.6512**	339.79	0.1041	105.61**

Notes: 1. Additional covariates include: other income, age, education, indicators for marital status, non-white, and hispanics, number of children less than 6 years old, number of children 6 to 18 years, and region dummies. 2. Statistical significance of the Hansen statistics indicates that the test for overidentification fails at 1, 5 or 10% significance level. Likewise, the absence of the sign near the statistics indicates that the test for overidentification passes.

Table A6. Estimates of Wage Elasticity of Labor Supply, All Working Females

	Wage Elasticity	1st F-stat	Partial R-sq	Hansen J-stat
A. HRS, all working individuals				
1998	0.1402**		N=2505	
IV-1	0.4143+	3.90	0.0053	2.62
IV-2	0.5230*	2.41	0.0047	2.35
IV-3	0.2503	2.59	0.0093	11.22
IV-4	0.0647	14.63	0.0422	26.57**
B. CPS: 50+ years old				
1991-2001	0.2458**		N=295786	
IV-1	0.2701**	371.63	0.0049	99.04**
IV-2	0.2657**	224.98	0.0044	150.47**
IV-3	0.3142**	137.13	0.005	275.69**
IV-4	0.7144**	3874.39	0.0775	1530.99**
1998	0.2484**		N=27995	
IV-1	0.3516**	34.82	0.0051	3.999
IV-2	0.3506**	22.58	0.0048	5.183
IV-3	0.3692**	14.98	0.0061	26.50**
IV-4	0.6949**	351.59	0.0761	140.05**
C. CPS: 18+ years old				
1991-2001 CPS	0.2458**		N=295786	
IV-1	0.3142**	137.13	0.005	275.69**
IV-2	0.2701**	371.63	0.0049	99.04**
IV-3	0.2657**	224.98	0.0044	150.47**
IV-4	0.7144**	3874.39	0.0775	1530.99**
1998-CPS	0.2484**		N=27995	
IV-1	0.6949**	351.59	0.0761	140.05**
IV-2	0.3516**	34.82	0.0051	3.999
IV-3	0.3506**	22.58	0.0048	5.183
IV-4	0.3692**	14.98	0.0061	26.50**
D. CPS: 18-49 years old				
1991-2001 CPS	0.2509**		N=248000	
IV-1	0.2909**	304.56	0.0048	82.84**
IV-2	0.2897**	185.28	0.0043	129.17**
IV-3	0.3310**	113.8	0.0049	216.28**
IV-4	0.7421**	3392.48	0.0852	1269.02**
1998-CPS	0.2521**		N=23316	
IV-1	0.3711**	28.67	0.005	2.884
IV-2	0.3929**	18.7	0.0048	3.006
IV-3	0.3830**	12.61	0.0061	19.10*
IV-4	0.7196**	307.84	0.0836	133.68**

See Notes to Table A5.

Table A7. Sensitivity Analysis

	Total Care Supply	Lower Limit 50	Wage ¡=50
Males	N=1434	N=1434	N=1387
Tobit	-0.1835	-0.2145+	-0.1768
IV-1	-1.9702+	-2.0782+	-1.7289
IV-2	-1.5471	-1.6591+	-1.3900
IV-3	-1.6704+	-1.9111*	-1.6165
Females	N=1358	N=1358	N=1350
Tobit	-0.1696	-0.1020	-0.1626
IV-1	-3.0562	-2.6283	-2.7930
IV-2	-6.1623*	-5.1429*	-6.1502*
IV-3	-2.8259+	-3.1622*	-2.3363

See Notes to 11.

Appendix B

Assuming that the net transfers are nothing else but the combination of gross transfers in different directions and there is zero probability of having one individual to be a giver and a recipient, the following exercise can be carried out. Suppose the data can be divided into three subsamples: one for positive values of net transfers, one for zero net transfers, and one for negative, as shown on Figure B1.

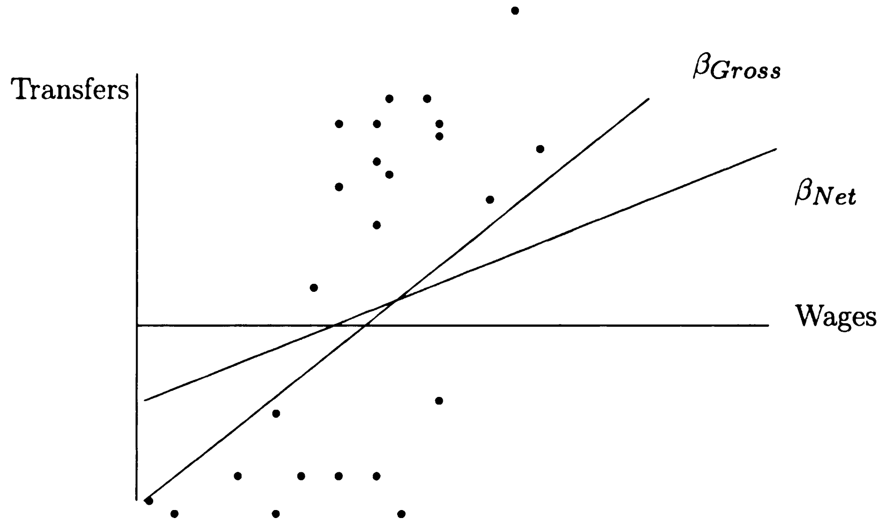

Figure B1. Relationship Between β_{Net} and β_{Gross}

Suppose that $y - positive$ represents observations with positive transfers and $y - negative$ represents observations corresponding to negative transfers, and β_{Net} and β_{Gross} are the estimates of the effect of wages on net and gross transfers respectively. $y - gross$ represents observations on the dependent variable with all the negative values replaced by zeros. Then, the following formula would provide least squares estimate of the coefficients on net transfers:

$$
\begin{aligned}
\beta_{Net} &= \frac{\sum(x-\overline{x})(y-\overline{y})}{\sum(x-\overline{x})^2} = \\
&= \frac{\sum(x-\overline{x})(y-\overline{y-gross}+\overline{y-gross}-\overline{y})}{\sum(x-\overline{x})^2} = \\
&= \frac{\sum_{y-positive}(x-\overline{x})(y-\overline{y-gross})}{\sum(x-\overline{x})^2} + \\
&+ \frac{\sum_{y-negative}(x-\overline{x})(y-\overline{y-gross})}{\sum(x-\overline{x})^2}
\end{aligned} \tag{42}
$$

$$
\begin{aligned}
\beta_{Gross} &= \frac{\sum(x-\overline{x})(y-\overline{y-gross})}{\sum(x-\overline{x})^2} = \\
&= \frac{\sum_{y-positive}(x-\overline{x})(y-\overline{y-gross})}{\sum(x-\overline{x})^2} + \\
&+ \frac{\sum_{y-negative}(x-\overline{x})(0-\overline{y-gross})}{\sum(x-\overline{x})^2}
\end{aligned} \tag{43}
$$

So,

$$
\beta_{Net} = \beta_{Gross} + \frac{\sum_{y-negative}(x-\overline{x})(y-\overline{y-gross})}{\sum(x-\overline{x})^2} \tag{44}
$$

which means that the coefficient on net transfers differs from the coefficient on gross transfers by the term the sign of which is defined by the coefficient from the model estimated using only observations with negative dependent variable. Summarizing:

$$
\begin{aligned}
\beta_{Net} &> \beta_{Gross} \quad if \quad \beta_{Neg} > 0 \\
\beta_{Net} &= \beta_{Gross} \quad if \quad \beta_{Neg} = 0 \\
\beta_{Net} &< \beta_{Gross} \quad if \quad \beta_{Neg} < 0
\end{aligned} \tag{45}
$$

Given that

$$\beta_{Pos} = \frac{\frac{1}{n_{positive}}\sum_{y-positive}(x-\overline{x})(y-\overline{y-gross})}{\frac{1}{n_{positive}}\sum_{y-positive}(x-\overline{x})^2} \tag{46}$$

$$\beta_{Neg} = \frac{\frac{1}{n_{negative}}\sum_{y-negative}(x-\overline{x})y}{\frac{1}{n_{negative}}\sum_{y-positive}(x-\overline{x})^2} \tag{47}$$

β_{Gross} and β_{Net} can be represented as follows:

$$\begin{aligned}\beta_{Gross} &= \beta_{Pos}\frac{n_{positive}}{n}\frac{Var(x|y>0)}{Var(x)} \\ &= \beta_{Pos}P(y>0)\frac{Var(x|y>0)}{Var(x)}\end{aligned} \tag{48}$$

$$\begin{aligned}\beta_{Net} = \beta_{Pos}\frac{n_{positive}}{n}\frac{Var(x|y>0)}{Var(x)} + \beta_{Neg}\frac{n_{negative}}{n}\frac{Var(x|y<0)}{Var(x)} = \\ = \beta_{Pos}P(y>0)\frac{Var(x|y>0)}{Var(x)} + \beta_{Neg}P(y<0)\frac{Var(x|y<0)}{Var(x)}\end{aligned} \tag{49}$$

Appendix C

Similar to Cragg (1971) the expected net monetary transfer conditional on explanatory variables can be formulated as follows:

Given that

$$E(y|x) = P(y < 0|x) * E(y|x, y < 0) + P(y = 0|x) * 0 + P(y > 0|x) * E(y|x, y > 0) \quad (50)$$

Differentiating this expression with respect to x_j produces the following expression for calculation of the marginal effects:

$$\begin{aligned}\frac{\partial E(y|x)}{\partial x_j} &= \frac{\partial P(y<0|x)}{\partial x_j} * E(y|x, y<0) + P(y<0|x) * \frac{\partial E(y|x, y<0)}{\partial x_j} + \\ &+ \frac{\partial P(y>0|x)}{\partial x_j} * E(y|x, y>0) + P(y>0|x)\frac{\partial E(y|x, y>0)}{\partial x_j}\end{aligned} \quad (51)$$

Alternatively,

$$\begin{aligned}\frac{\partial E(y|x)}{\partial x_j} &= \frac{\partial P(y<0|x)}{\partial x_j} * E(y|x, y<0) + P(y<0|x) * \beta_{Neg} + \\ &+ \frac{\partial P(y>0|x)}{\partial x_j} * E(y|x, y>0) + P(y>0|x) * \beta_{Pos}\end{aligned} \quad (52)$$

As is evident from the formula, the overall effect of changes in an explanatory variable can be represented as a summation of the effects on the probability of observing a certain transfer status as well as changes in the amount of transfers for those who remain in the givers' or recipients' categories.

To derive the formulas for the wage elasticities, it is useful to recall that $x_j = log(wage)$ and thus $\epsilon_{yx} = \frac{\partial E(y|x)}{\partial x_j} * \frac{1}{E(y|x)}$. So the four wage elasticities that will be useful in calculating the wage elasticity of net monetary transfers are the following:

$$\epsilon_{\ NegExt} = \frac{\partial P(y<0|x)}{\partial x_j} \frac{1}{P(y<0|x)} \quad (53)$$

$$\epsilon_{PosExt} = \frac{\partial P(y > 0|x)}{\partial x_j} \frac{1}{P(y > 0|x)} \tag{54}$$

$$\epsilon_{Neg} = \beta_{Neg} \frac{1}{E(y|x, y < 0)} \tag{55}$$

$$\epsilon_{Pos} = \beta_{Pos} \frac{1}{E(y|x, y > 0)}, \tag{56}$$

where β_{Neg} and β_{Pos} represent the coefficients from the estimating the wage effects for the population of net recipients and net givers respectively.

Combining this elasticities leads to the following expression:

$$\begin{aligned} \epsilon_{Net} = (\epsilon_{NegExt} + \epsilon_{Neg}) * \frac{P(y < 0|x) * E(y|x, y < 0)}{E(y|x)} + \\ +(\epsilon_{PosExt} + \epsilon_{Pos}) * \frac{P(y > 0|x) * E(y|x, y > 0)}{E(y|x)}, \end{aligned} \tag{57}$$

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