# Commitment in the Household: Evidence from the Effect of Inheritances on the Labor Supply of Older Married Couples

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Abstract

We study the effect of receiving an inheritance on the labor force participation (LFP) of both the recipient and the recipient's spouse. An inheritance is not subject to laws governing division of marital property at divorce, because it is not acquired with income earned during marriage. Hence it plays the role of a "distribution factor" in the intrahousehold allocation of resources, increasing bargaining power of the recipient. Controlling for inheritance expectations, we interpret the actual receipt of an inheritance as a shock to wealth. Our results indicate that receiving an inheritance reduces LFP of the recipient by 2 to 4 percentage points, comparable in magnitude to the effect of a decline in health. However, an inheritance has little or no effect on LFP of the spouse. These estimates are inconsistent with a dynamic collective model of the household in which spouses have the ability to commit to an *ex ante* efficient allocation. The results are consistent with a model of limited commitment. We discuss the implications for reform of Social Security spouse and survivor benefits.

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## 1. Introduction

Cooperative bargaining models of intrahousehold resource allocation have been applied with increasing frequency to analyze and interpret intertemporal behavior of households in an environment of uncertainty. A key issue in this setting is whether household members are able to fully commit to a resource allocation plan (a "contract") agreed upon at the time the household is formed. If spouses can commit to a state-contingent resource allocation plan, then their relative bargaining power at the time of marriage determines the effects of subsequent income and other shocks on intrahousehold allocations. Such shocks would have wealth and/or substitution effects on household resource allocation decisions, but they would not cause renegotiation of the original contract.[1]

Commitment is an important issue because, as Mazzocco (2007) points out, it determines the impact of public policies that shift control of resources within the household. If households are able to commit to an *ex ante* efficient resource allocation plan, then policies that intentionally or unintentionally change control of resources within the household will have limited impact on intrahousehold resource allocation.[2] However, Voena (in press) argues that unilateral divorce laws, which are ubiquitous in the US today, limit the ability of spouses to commit. In this legal environment, a shock that increases the relative value of the outside alternative for one spouse may result in a binding participation constraint, causing a shift in bargaining power within the household. In a cooperative bargaining framework this will cause renegotiation of the contract,

[1] See Marcet and Marimon (2011) for a general discussion of contracting problems in which agents are subject to intertemporal participation or other constraints that affect the set of feasible contracts. Of course, a contract can always be renegotiated by mutual consent, regardless of commitment ability.

[2] Such policies also operate via the budget constraint, so they will have wealth and/or substitution effects. And they will affect the initial distribution of bargaining power in households formed after implementation of the new policy.

leading to an *ex post* efficient outcome, given the new distribution of bargaining power. The new outcome could involve divorce, if that is efficient, or a reallocation of decision power toward the spouse whose participation constraint binds. But the inability to commit to an efficient resource allocation plan will lead to an *ex ante* inefficient outcome. For example, specialization of one spouse in home production activities and the other in the labor market may be optimal, but if the spouse who specializes in the market cannot commit to remaining in the household when his earnings are high, the optimal degree of specialization will not occur.[3]

Previous empirical studies of intertemporal household behavior in the cooperative bargaining framework have either assumed that spouses have full commitment ability (Casanova, 2010; van der Klaauw and Wolpin, 2008) and imposed the assumption in a structural estimation approach, or have tested for full commitment by analyzing the implications for consumption or time allocation Euler equations (Lich-Tyler, undated; Lise and Yamada, 2014; Mazzocco, 2007).[4] The drawback of the first approach is clear: if full commitment is not feasible, the model is misspecified. A drawback of the second approach is that Euler equation methods are not well-suited to analyze labor supply. Labor supply decisions are often discrete, especially at older ages,

[3] The legal environment governing household dissolution and property division for cohabiting couples is very different than for married couples. Hence we do not analyze or discuss cohabiting couples, although many of the same issues are relevant.

[4] An exception is Lundberg, Startz, and Stillman (2003), who analyze the change in household consumption expenditure following retirement of the husband, and interpret the results in terms of an intertemporal bargaining model without commitment. Our approach is similar, as it develops a test based on a model and imposes minimal assumptions in the estimation. Mazzocco, Ruiz, and Yamaguchi (2007) estimate a dynamic collective labor supply model without commitment for young couples. Gemici (2011) estimates a dynamic cooperative Nash bargaining model of family labor supply and migration. She assumes that utility is transferable, leading to an efficient outcome despite lack of commitment ability. Several papers have used a non-cooperative bargaining approach to modeling retirement behavior of couples: e.g. Gallipoli and Turner (2013) and Gustman and Steinmeier (2009). By construction, there is no commitment ability in such models.

where the most common pattern of retirement is abrupt and complete withdrawal from the labor force.

Our paper introduces a new approach to empirical analysis and testing of commitment in married-couple households. We estimate the impact of receiving an inheritance on the labor force participation (LFP) decisions of older individuals and their spouses. Inheritances provide a useful new source of identification for studying commitment, because they are not subject to marital property law in the US. In most US states these laws specify that earnings during marriage and the assets acquired with those earnings are community property, divided equally or equitably between the spouses in the event of divorce, regardless of which spouse formally holds title to the asset (Mazzocco, 2007; Voena, in press). For example, an employer-provided pension account held by one spouse is considered community property in the event of a divorce if the job was held during the marriage. In contrast, inheritances belong exclusively to the recipient since they were not acquired with earnings during marriage. Inheritances unambiguously increase the value of the outside option of the recipient but not of the spouse.

We use inheritances to test for commitment in a discrete choice labor supply framework. Our approach is similar to Mazzocco (2007), but our test is for labor supply, imposes weaker assumptions, and uses a new source of identification.[5] Under the null hypothesis of full commitment ability, the effect on the husband's LFP of an unexpected inheritance received by him should be equal to the effect on his LFP of an unexpected inheritance received by his wife, and conversely for the wife's LFP. Under full commitment, decision power at the time of marriage determines the allocation of resources in the couple's state-contingent contract. For

[5] The assumptions of the Euler equation approach include intertemporal separability of preferences and the absence of liquidity constraints (Mazzocco, 2007).

example, if both spouses perceive a high probability that the wife will inherit a large sum in the future, her decision power at the time of the marriage will be relatively high. The actual receipt of an inheritance will affect LFP of the spouses via wealth effects as determined by their initial decision power, regardless of which spouse is the recipient. A pattern in which a husband's inheritance affects only his LFP and a wife's inheritance affects only her LFP is inconsistent with full commitment, but is consistent with a limited commitment model in which contracts are renegotiated when a participation constraint binds. We develop a simple model in the next section to illustrate this claim.

Our empirical analysis uses novel longitudinal data from the Health and Retirement Study (HRS) on inheritances *and* inheritance expectations of both spouses in married-couple households. Controlling for inheritance expectations, we interpret inheritance receipt as a shock. This is a rare example of a measureable household resource shock that unambiguously accrues to a specific household member. It is important to distinguish between expected and unexpected inheritances and, conditional on anticipating an inheritance, the expected and actual inheritance amount, because only the unexpected part is informative about commitment. An inheritance that is anticipated at the beginning of marriage might affect the timing of retirement, but this would be the case regardless of the ability to commit.

We find that receiving an inheritance has a negative effect on the probability of LFP by the recipient, and virtually zero impact on LFP of the spouse, controlling for inheritance expectations, lagged LFP, lagged inheritances, household wealth, and many other determinants of labor supply. The estimates of the own-inheritance effects for husbands and wives are similar in magnitude. The precision of the estimates is somewhat low as a result of the relative

infrequency of inheritances, but in some specifications we can reject the null hypothesis of full commitment. The results are quite robust to alternative definitions of employment, alternative regression specifications, and alternative estimation approaches.

This finding confirms results from previous studies that have analyzed the impact of changes in control over resources within the household resulting from exogenous policy changes, but our context is quite different. Previous studies have focused mainly on spending on children as a function of who controls income entering the household.[6] Our study is one of the first to focus on the impact of control over household resources on LFP.[7] Lise and Yamada (2014) study commitment in a model of time allocation, using deviations of wage growth from the path anticipated at the time of marriage as a measure of resource shocks. To implement this approach, they specify a wage forecasting model that is assumed to be used by individuals. The advantage of our approach is that we do not have to make assumptions about how expectations are formed. We contribute to the literature on commitment by using a new source of identifying information and studying retirement, a major life decision. In the concluding section we discuss reform of Social Security spouse and survivor benefits as an important example of a policy change the effects of which depend on commitment ability.

[6] Lundberg, Pollak, and Wales (1996) study the effect on household expenditure patterns of a change in policy in the UK that shifted control of a state-provided child allowance to mothers. Bobonis (2009) analyzes the impact on child outcomes of Progresa, a conditional cash transfer program in Mexico that provided subsidies directly to mothers. Duflo (2003) analyzes the impact on child health of a large increase in public pension receipt by female-headed black households in South Africa. Duflo and Udry (2004) analyze the impact of rainfall shocks on the within-household allocation of expenditure in Cote d'Ivoire, where crops customarily raised by men and women differ in their sensitivity to rainfall patterns. See also Lundberg, Startz, and Stillman (2003), discussed previously.

[7] Chiappori, Fortin, and Lacroix (2002) use a static framework to analyze the effects of various "distribution factors" on hours of work in two-earner households, but they do not study the participation decision. A number of studies treat the ratio of the spouse's wage rates as a distribution factor, but the wage ratio is unlikely to be exogenous.

A recent paper by Brown, Coile, and Weisbenner (2010) exploits the HRS survey data on anticipated and actual receipt of inheritances to construct a measure of unanticipated inheritances which does not rely on strong assumptions about expectations. They find that receipt of an unanticipated inheritance leads to an increase in labor force exit at older ages.[8] They focused on the effect of *household-level* receipt of an inheritance. In a typical specification, the dependent variable in their analysis is an indicator of labor force exit by an individual in a given period, and the key explanatory variable is an indicator for whether *someone* in the individual's household received an inheritance since the previous interview. Their estimate represents the average effect of inheritance receipt on household labor supply. We extend their analysis to estimate both the own and cross-spouse effects of inheritance shocks, disaggregated by the identity of the recipient. We focus on married couples, and analyze men and women separately in order to determine whether there are systematic differences in behavior in response to inheritance shocks.

In the next section, we sketch a very simple cooperative model of intertemporal household behavior designed to formalize our claim that inheritance shocks can be used to test for commitment. Section 3 describes the data and section 4 presents and discusses the results. Section 5 concludes.

## 2. Model

[8] Two earlier papers (Holtz-Eakin, Joulfaian, and Rosen 1993; Joulfaian and Wilhelm 1994) examine the effect of inheritances on labor supply, but they do not focus on retirement. Joulfaian and Wilhelm use data on inheritance expectations as well as inheritance receipt from the Panel Study of Income Dynamics.

We develop a stylized model to motivate our test of commitment.[9] Consider a two-person household and a two-period horizon. We condition on formation of the household at the beginning of the first period, implying that the lifetime expected value of the match exceeds the value of the outside option for both spouses. Spouse $i$ has a period utility function defined over consumption and hours of work: $u_i(c_{it}, h_{it})$. For simplicity, there are no household public goods. We focus on the hours of work choice, but the extension to the discrete work decision is straightforward. We assume cooperative behavior that leads to a Pareto efficient outcome (see Browning, Chiappori and Weiss, 2014, for a survey of this literature). This implies a formulation in which the spouses choose consumption and hours of work each period to maximize a weighted sum of the spouses' expected present discounted value of lifetime utility:

$$\max_{\{c_{it},\, h_{it}\}_{i=1,2;\; t=1,2}} \quad \mu_1 E\sum_t u_1(c_{1t}, h_{1t}) + \mu_2 E\sum_t u_2(c_{2t}, h_{2t})$$

subject to constraints specified below. $\mu_i$ is the *ex ante* bargaining power or Pareto weight of person $i$ at the time the match is formed, which is a function of distribution factors to be specified below, and $E$ is the expectations operator. For simplicity we have assumed no discounting, and we will also assume that the interest rate is zero.

Resources are derived from a household-level endowment $A_0$, earnings $w_{it}h_{it}$, and spouse-specific inheritances $I_i$. The household faces no liquidity constraint, but must be solvent at the end of period 2. Assume for simplicity that wage rates are constant over time. Inheritances are random variables realized at the beginning of period 2, before period-2 choices are made. The

[9] For clarity and simplicity, henceforth we use the term commitment to refer to full commitment ability (enforceable *ex ante* efficient contracts). Inability to fully commit encompasses limited commitment, where contracts are renegotiated only when a participation constraint binds, and no commitment, with contracts renegotiated every period. Our test cannot distinguish between limited and no commitment, so we lump them together and refer to them jointly as inability to commit, as in Lise and Yamada (2014).

joint probability density function (pdf) of inheritances is $f(I_1, I_2)$. Inheritances are the only source of uncertainty in the model. The budget constraint in the first period is

$$c_{11} + c_{21} = A_0 + w_1 h_{11} + w_2 h_{21} - A_1,$$

where $A_1$ is savings, and the state-contingent budget constraint in period 2 is

$$c_{12} + c_{22} = A_1 + w_1 h_{21} + w_2 h_{22} + I_1^* + I_2^*,$$

where $I_i^*$ is the realization of the random variable $I_i$. These constraints are based on the assumption that resources are pooled within the household, a key element of cooperative bargaining models.

Following Chiappori et al. (2002), define a distribution factor as a variable that affects the intrahousehold decision process but does not influence preferences or the couple's joint budget constraint. The key distribution factor in this model is $f$. The greater the likelihood that spouse $i$ will receive an inheritance, as measured by the joint pdf, the greater is her *ex ante* bargaining power at the time the marriage is formed.[10] The assumption of commitment means that the Pareto weights $\mu_i(f)$ are constant: the couple commits to an allocation plan, and the realization of the inheritance outcome does not cause renegotiation. With this assumption, the model is complete and can be solved recursively. The household's problem in period 2 is

$$\max_{\{c_{i2}, h_{i2}\}_{i=1,2}} \mu_1(f)u_1(c_{12}, h_{12}) + \mu_2(f)u_2(c_{22}, h_{22})$$

subject to the period 2 budget constraint.

[10]To illustrate this point more transparently, suppose that the inheritance probability distribution takes the following very simple form: with probability $\pi_i$, spouse $i$ receives an inheritance of amount $I$, and with probability $1 - \pi_1 - \pi_2$ neither spouse receives an inheritance. In this setup, at most one spouse receives an inheritance, and the amount of the inheritance is the same regardless of which spouse receives it. The Pareto weights then can be written as $\mu_i(\pi_i)$, with $\mu_i$ increasing in $\pi_i$, illustrating the point that a greater likelihood of receiving an inheritance increases bargaining power. Note that inheritance *realizations* are not distribution factors in the commitment model because they are unknown at the time the marriage is formed.

The solution can be written in the form of state-contingent value functions $V_{i2}(\Phi, I_1^*, I_2^*)$, where $\Phi$ is the vector of state variables known at the end of period 1: $\Phi = \{A_1, w_1, w_2\}$. The key empirical implication of commitment derives from the fact that inheritance *realizations* enter the problem only through the period-2 budget constraint, where they appear additively. The *ex ante* probability distribution of inheritances affects bargaining power, but under commitment the realizations do not. The realizations have wealth effects only. Hence in the case of commitment we can rewrite $V_{i2}(\Phi, I_1^*, I_2^*)$ as $V_{i2}^*(\Phi, I_1^* + I_2^*)$.

We can write a regression function for period-2 hours of work for spouse *i* based on this model, omitting a household subscript:

$$h_{i2} = \beta_{i1} w_1 + \beta_{i2} w_2 + \alpha_{i1} I_1^* + \alpha_{i2} I_2^* + \gamma_i A_1 + g_i(f(I_1, I_2)) + \varepsilon_{i2}$$

where $g_i$ is a function of *ex ante* inheritance expectations. It is crucial to control for inheritance expectations, since they will naturally co-vary with inheritance realizations.[11] The testable implications of commitment are $\alpha_{i1} = \alpha_{i2}$, $i = 1,2$: inheritance shocks affect labor supply, but the identity of the recipient of the inheritance does not matter.[12]

If commitment is not possible, there are participation constraints in period 2: $u_i(c_{i2}, h_{i2}) \geq u_{i2}^*(I_i^*)$, $i = 1, 2$, where $u_{i2}^*$ is the level of utility associated with the outside option of spouse *i*. We show the dependence of the utility of the outside option on the inheritance realization to

[11] In the empirical analysis we estimate this specification as well as a more restrictive specification that combines inheritance expectations and realizations into a single unanticipated inheritance variable, $I_i^u = I_i - \mathrm{E}(I_i)$.

[12] This result holds in more general models as well. For example, consider a strategic bequest model in which spouse 1 may be able to increase the expected value of her inheritance by providing services to the benefactor, such as personal care. If this imposes a cost on spouse 1, for example by reducing her available time to allocate between leisure and employment, then her bargaining power at the beginning of the union would be higher than in the absence of such a consideration. But realization of the inheritance would not alter bargaining power. Similar logic applies if a specific bequeathable good such as a parent's home has sentimental value to one spouse but not to the other. If the recipient plans to keep the parent's home indefinitely after inheriting it, this will reduce the impact of the inheritance on labor supply of both spouses.

emphasize the point that receiving an inheritance increases the value of the outside option. The key consideration in the absence of commitment is whether one of the spouses receives an inheritance shock large enough to cause a participation constraint to bind. If neither spouse experiences this event, the solution is identical to the commitment case. If both spouses receive such an inheritance, then both spouses prefer the outside option. If the outside option is divorce, the marriage ends and we don't observe the household in the data in period 2. Thus we focus here on the case in which one and only one spouse receives such an inheritance.[13] Let $\lambda_i$ be the multiplier on the participation constraint for spouse *i*. Following Mazzocco (2007) and Marcet and Marimon (2011), the optimization problem in period 2 in the absence of commitment ability can be written as

$$\max_{\{c_{i2},\, h_{i2}\}_{i=1,2}} \; \mu_1 u_1(c_{12}, h_{12}) + \lambda_1(u_1(c_{12}, h_{12}) - u_{12}^*) + \mu_2 u_2(c_{22}, h_{22}) + \lambda_2(u_2(c_{22}, h_{22}) - u_{22}^*),$$

subject to the budget constraint. If a participation constraint is not binding ($u_i(c_{i2}, h_{i2}) - u_{i2}^* > 0$), then $\lambda_i = 0$.

A binding participation constraint causes the period-2 Pareto weight to differ from the period 1 value. If person 1 has a binding participation constraint, then the first line of the maximand above can be written $M_1 u_1(c_{12}, h_{12}) - \lambda_1 u_{12}^*$, where $M_1 = \mu_1 + \lambda_1$. If spouse 1 receives an inheritance shock large enough to cause her participation constraint to bind, the original "contract" is renegotiated so that her bargaining weight increases by enough to make her indifferent between remaining in the marriage and choosing the outside option. In the no-

[13] This restriction is not imposed in the empirical analysis. If the outside option is to remain married but interact non-cooperatively, we would expect the solution to be similar to this case.

commitment case, receiving an inheritance shock that is large enough to cause a participation constraint to bind causes a shift in resources toward the recipient, resulting in what is effectively a wealth effect, since the inheritance does not alter any relative prices. If leisure is a normal good, we expect this to cause a decrease in hours worked (and participation). Things are more complicated for the non-recipient because there are offsetting effects: (1) his bargaining power declines, so he loses some control over resources, and (2) household wealth increases, so he gains a share of the additional resources available to the household, thanks to resource pooling.[14] The model does not predict which effect dominates. If the latter effect dominates and leisure is highly weighted in person 2's preferences, the decline in his hours of work could be larger than the decline in person 1's hours. Thus, the inability to commit implies that in the empirical model written above, in general $\alpha_{i1} \neq \alpha_{i2}$.[15] We might expect the "own inheritance" effects to be larger than "spouse inheritance" effects: $|\alpha_{11}| > |\alpha_{12}|$ and $|\alpha_{22}| > |\alpha_{21}|$, but this is not a prediction of the theory. So a test of commitment is a test of $\alpha_{i1} = \alpha_{i2}$ versus the alternative $\alpha_{i1} \neq \alpha_{i2}$, $i = 1, 2$.

We have assumed egoistic preferences (no externalities in utility), but the result generalizes to any form of non-separable preferences, such as caring preferences and leisure complementarity. The equalities implied by commitment will hold with non-separable preferences, because under commitment inheritances have only a wealth effect. Thus regardless

[14] The inheritance realization is an argument of the Pareto weighting functions, but also enters the period-2 budget constraint. This appears to violate the condition for a variable to be a distribution factor. However, the formulation of the model as described here is equivalent to a formulation in which each spouse has a separate savings account in addition to the couple's joint account, and inheritances are deposited in the individual account of the recipient rather than the joint account. In this formulation, the inheritance realization does not enter the *joint* budget set, which is the condition for a variable to be a distribution factor (Chiappori et al., 2002). Separate accounts are irrelevant in the commitment case. In order keep the no-commitment analysis comparable to the commitment analysis, we use the joint-account formulation. See Mazzocco (2007) and Voena (in press) for discussion of joint versus individual accounts in household bargaining models with limited commitment.

[15] It would be interesting to decompose the estimated total effect of an inheritance into the bargaining power and wealth effects, but unfortunately this requires knowledge of initial wealth ($A_0$), which we do not have.

of the form of preferences the restriction $V_{i2}(\Phi, I_1^*, I_2^*) = V_{i2}^*(\Phi, I_1^* + I_2^*)$ holds under commitment, because this is determined purely by the budget constraint. And this restriction generates the testable implications of commitment.

## 3. Data

We use data from the Health and Retirement Study (HRS), a national biennial panel study of older individuals and their spouses.[16] The HRS contains an abundance of information on demographic characteristics, health, labor supply, income, and wealth. Our sample includes data from the original HRS cohort born from 1931 to 1941 and interviewed beginning in 1992, the "War Baby" cohort born from 1942 to 1947 and interviewed beginning in 1998, and the Early Baby Boom cohort born from 1948 to 1953 and interviewed beginning in 2004.[17] We examine labor supply behavior in survey years 1996 through 2008.[18] The final analysis sample has 42,962 person-wave (21,481 couple-wave) observations on 5,937 married-couple households in which both spouses are between the ages of 45 and 70.

[16] Specifically, we make use of the RAND HRS data file (version L), a user-friendly cleaned and processed subset of the HRS data. For certain variables not included in this data file (e.g. inheritance receipts and source of inheritances), we use the RAND enhanced Fat Files datasets. See the RAND and HRS websites for more detail.
[17] To ensure stability of households across survey waves, we keep only couples whose marriage was in progress at the previous survey wave. We drop a very small number of observations with census region missing or equal to 11 ("Not US/inc US Terr"), and a small number of same-sex couples. Finally, as discussed in further detail below, we also drop a small number of couple-wave observations with extremely high inheritance amounts.
[18] Data on inheritance expectations are available in the HRS in years 1994 through 2006. We control for lagged inheritance expectations, so we estimate models for survey years 1996 through 2008.

Our primary measure of labor supply is LFP status at the survey date. We also examine other outcomes such as indicators for currently working for pay and full-time year-round employment, and weekly and annual hours worked.[19]

The key explanatory variable is receipt of an inheritance since the previous interview (interviews are two years apart on average). The HRS survey asks one member of the household, designated the financial respondent, to answer questions about all inheritances received by the household. If the household received an inheritance from a parent or sibling of the financial respondent then we assign the inheritance to the financial respondent. If the financial respondent reports that the household received an inheritance from a parent of his or her spouse, then we assign the inheritance to the financial respondent's spouse. Other responses to the question on the source of an inheritance do not provide enough information to permit the inheritance to be assigned to a particular spouse.[20] Specifically, if the inheritance is received from an "other relative", "other individual", "ex-spouse/partner", or the source is missing or unknown, then we do not know whether the inheritance accrued to the husband, wife, or another household member.

Table 1 summarizes the incidence and distribution of inheritance receipts among households in our sample. The first panel shows that 16 percent of couples received at least one inheritance during the 12 year period of observation, and 5 percent received an inheritance

[19] Specifically, the respondent is categorized as a labor force participant (LFP = 1) if he or she has full or part time employment, is unemployed, or is partially retired. The respondent is categorized as not in the labor force (LFP = 0) if he or she is retired, disabled, or "not in LF". Our measure of employment status is based on the variable RxLBRF in the RAND HRS data set, and the alternative measures of hours worked are based on the variables RxJHOURS and RxJWEEKS.

[20] Respondents can report up to three inheritances in a given wave. We use the sum of inheritances received from each source.

between a given pair of interviews. Husbands and wives are about equally likely to be the recipient, and in 19 percent of cases (.010/.052) the inheritance recipient cannot be determined.

The second panel in Table 1 shows that the distribution of inheritances amounts is quite skewed. Among the 1,062 couple-wave observations where at least one inheritance was received, the mean inheritance amount is $74,300 while the median is $37,600. Inheritances at the upper tail of the distribution are quite large – the 95$^{th}$ percentile inheritance is over $275,000, while the 99$^{th}$ percentile is over $500,000. In fact, reported inheritances are even more skewed than these statistics indicate, because the sample excludes 33 couple-wave observations in which an inheritance of $617,550 or more was received. These large outliers had an inordinate influence on the results. This is discussed in more detail in the Appendix, and estimates that incorporate these outliers are reported as well.

As discussed earlier, in order to interpret inheritances as shocks, we must control for inheritance expectations. An innovative feature of the HRS is that survey respondents are asked a number of questions about their expectations of future events, including inheritances. The expectations are based on a series of questions asked of *each respondent* (financial and non-financial). Respondents are first asked to rate their chances of receiving an inheritance within the next 10 years, from 0 to 100 percent. Respondents who report a positive probability are asked how large the inheritance is expected to be.[21] Panels (c) and (d) of Table 1 summarize inheritance expectations. Over all person-wave observations in our sample, 36 percent of husbands and 42 percent of wives report a positive probability of receiving an inheritance, and

[21] Respondents who do not report a specific value are asked a series of questions that bracket (i.e. assign a lower and upper bound to) the expected inheritance amount. We set the expected inheritance amount equal to the midpoint of the bracket for these respondents.

conditional on being positive the mean probabilities are 0.55 and 0.59, respectively. Conditional on expecting an inheritance, for both husbands and wives the median expected amount is roughly $35,000, and the mean expected amount is roughly $100,000. In 15 percent of cases the husband does not answer any of the inheritance expectation questions, and in an additional 3 percent of cases the husband reports a probability of inheritance receipt but does not report an expected inheritance amount; we keep these cases in the sample and include missing-data indicators in the model. We do the same for wives. We also note that inheritance expectations are correlated with inheritance realizations, as previously reported by Brown et al. (2010). For example, among respondents who previously reported a positive probability of receiving an inheritance, 25 percent are observed to actually receive an inheritance in our sample, compared with 7 percent among respondents who reported a probability of zero.

Another key control variable is household wealth at the date of the previous interview.[22] It is worth emphasizing that inheritances received since the previous interview are *not* included in lagged net worth. Social Security and employer pensions are important sources of wealth that are of particular relevance for older workers. Unlike net worth, claims on Social Security and pensions are illiquid and cannot necessarily be treated as equivalent to other assets. Nevertheless, we follow the conventional approach in the literature, forming measures of the expected present discounted value of future Social Security and pension benefits. Specifically, we compute the expected present discounted value (EPDV) of Social Security benefits (Social Security Wealth, or SSW) under several alternative assumptions about labor force exit and claiming: (1) exit in the

[22] We use the variable HxATOTA from the RAND HRS dataset, which measures total household net worth. This variable is built up from responses to questions about many types of assets, and incorporates extensive imputations based on partial (bracketed) responses. Note that wealth is measured at the household level in the HRS; the survey does not attempt to identify individual versus joint ownership of each asset.

current period and never return to work, and claim the benefit at 62, (2) exit in the current period and successfully apply for Social Security Disability benefits, (3) work until age 62, exit the labor force at 62, and claim the Old Age and Social Insurance (OASI) benefit at 62 and never return to work, (4) same as (3) but at 65, (5) same as (3) but at 70.[23] These are used in alternative specifications to determine whether the results are sensitive to the specific assumptions. Earnings data from Social Security Earnings Records are available for many HRS respondents and are used to compute benefits under a set of assumptions about future earnings. We also use these data to construct a measure of the EPDV of remaining lifetime earnings, included as a control variable. Details of the calculations are described in the Appendix.

For workers covered by a Defined Benefit (DB) pension, we use summary plan descriptions and pension calculator software provided by the HRS to calculate the expected present discounted value of benefits available under scenarios (1) and (3)-(5) described above for Social Security. The only difference is that we use respondent-reported earnings from the pension-providing employer instead of the Social Security earnings record. For workers covered by Defined Contribution plans, we used the account balances reported by respondents. We also try using the present discounted value of the DC account balance computed using the pension calculator software under the same assumptions as for DB cases. These calculations are also described in the Appendix.

[23] In the scenarios involving continued work beyond the current period, we assume annual earnings are equal to the average of the most recent five years of earnings up to the current period. For these and the additional variables described in this paragraph, we assume standard life table mortality, a 3% real rate of interest, and zero real wage growth. The main specification includes SSW under scenarios (1) and (2), and the increment to SSW from scenario (3) relative to scenario (1).

We control for a large number of other variables that may affect LFP and could in principle be correlated with inheritance shocks. These include lagged LFP of the individual and the spouse, lagged self-employment status of both spouses, and whether the employer provides health insurance coverage, both with and without retiree benefits. Other controls include categorical indicators for educational attainment, ethnicity (Hispanic), race (black; other non-white), geographic location (census division), year fixed effects, age (cubic plus dummies for 62-64 and 65+), health status, recent changes in health status , and whether the respondent's parents (or spouse's parents) died since the last survey wave. The latter variable could directly affect the respondent's preferences for work in the current period, and is of course associated with the likelihood of inheritance receipt.[24] Descriptive statistics are provided in the Appendix.

In order to test whether this specification is rich enough to adequately control for unobserved factors that could be correlated with inheritance receipt, even after controlling for expectations, we estimated several regression models using this specification to explain outcomes that were determined *before* receipt of an inheritance. If we have adequately controlled for expectations and other factors, inheritance receipt should be uncorrelated with predetermined outcomes. The predetermined outcomes we examined included lagged inheritance receipt, twice-lagged expectations, twice-lagged wealth, and twice-lagged LFP, for the individual and the spouse. We found that this "placebo test" failed for several predetermined variables, indicating

[24] Theory implies that the wage rates of the individual and spouse should be included in the specification. However, we do not observe a wage rate for non-workers so wage rates are omitted. We estimated several alternative specifications incorporating the wage rate, using a variety of approaches to address the problem of missing wages for non-workers. The estimates from these specifications are virtually identical to those reported below.

that self-reported expectations are not sufficient to eliminate unobserved heterogeneity. As a result, we added the variables described above to the regression specification.[25]

## 4. Results

### *A. Specification*

We estimate models of the form

$$y_{it} = \beta_0 + \beta_1 I_{it} + \beta_2 I_{jt} + \beta_3 I_{ut} + \beta_4 E_{it-1} + \beta_5 E_{jt-1} + \beta_6 X_{it-1} + \beta_7 X_{jt-1} + \beta_8 Z_i + \varepsilon_{it}.$$

$y_{it}$ is a binary indicator of LFP of member $i$ in a couple (the couple subscript is omitted). $I_{it}$ is either an indicator of inheritance receipt by individual $i$ since the previous interview, or a measure of the amount inherited, in alternative specifications. $I_{jt}$ is the indicator or amount inherited by $i$'s spouse since the previous interview, and $I_{ut}$ is the indicator or amount inherited by the couple from other sources where the recipient cannot be determined (unknown). $E_{it-1}$ and $E_{jt-1}$ are the inheritance expectations of the individual and spouse as of the previous interview. $X_{it-1}$ and $X_{jt-1}$ are vectors of time-varying spouse-specific variables, including two lags each of own and spouse LFP, inheritance receipt, inheritance expectations, and household wealth, and $Z$ is a vector of fixed household characteristics. Controlling for inheritance expectations as of the previous interview (and the other control variables), we interpret $\beta_1$, and $\beta_2$ as the effects of inheritance shocks.

The main estimates are from linear probability models. Estimates are presented separately for husbands and wives.[26] We include all couples regardless of labor force participation in the

[25] Another approach to testing for unobserved heterogeneity is to estimate a fixed effects specification. This is feasible because our main specification pools labor force participants and non-participants, so the effects are identified by both exit and entry. About one third of the sample ever changes labor force status during the period of observation. Fixed effects estimates are very similar to the OLS estimates reported in Table 2 except in one case.

previous wave. This makes it possible to capture effects of inheritances on reentry to the labor force as well as exit. It is well known that retirement patterns can be complex, with repeated exit and entry, so looking only at exit could miss part of the impact of an inheritance shock. We also present results that split the sample according to lagged LFP.

*B. Main Results*

Table 2 presents selected results from estimates of the model. Each column shows results for one linear regression, estimated by OLS. Coefficient estimates on the variables other than those shown in the table are reported for the specifications in the first two columns in Appendix D. The standard errors are clustered by household. The first column shows that controlling for inheritance expectations at the previous interview, receipt of an inheritance by a married man causes a 3.0 percentage point decline in LFP, a 5% effect relative to the sample mean LFP rate of 0.61 for men. Receipt of an inheritance by the wife causes a decrease of 0.8 percentage points in *his* LFP. Surprisingly, receipt by the household of an inheritance for which the recipient within the household is unknown causes a decline of 6.6 percentage points in his LFP. The effects of inheritance receipt for women shown in the second column are very similar in sign and magnitude to the effects for men, except in the case of an unknown recipient, which has a very small effect on LFP of wives. The last two columns of the table show results for a specification that uses the dollar amount inherited (in units of $100,000), with zero for non-recipients. The estimates of own and spouse inheritance effects are similar in sign, magnitude, and precision to those in the first two columns.

[26] Marginal effects from probits are very similar to the linear regression estimates.

Inheritance expectations turn out to have little association with LFP, despite the fact that they are positively correlated with inheritance realizations, as noted earlier. The null hypothesis that the coefficients on the four own inheritance expectations variables shown in Table 2 are jointly equal to zero cannot be rejected at the 10 percent level of significance in any of the regression models. There are several statistically significant coefficient estimates for the wife's expectations in the husband's LFP equations, but the implied magnitudes are very small.

One way to illustrate the magnitude of the inheritance receipt effects is to compare them to the estimated effects of other events. For example, Table D in the Appendix shows that the impact on LFP of a deterioration in health since the previous interview is -0.045 for men and -0.035 for women. Thus the magnitude of the own-inheritance effect is about two thirds the size of the effect of a deterioration in health for men, and is the same size for women. This suggests a relatively large impact of inheritance receipt in view of the importance of health declines for LFP.

Our estimates are roughly in line with earlier studies examining the effects of inheritances on labor supply, despite differences in sample composition and time period. Brown et al. (2010) report effects of about 0.02 on the probability of exit from the labor force, and about 0.04 per $100,000 received (Table 2), based on a sample of older individuals and married couples in the HRS.[27] The estimates of Holtz-Eakin et al. (1993) imply that a $100,000 inheritance would have caused LFP to fall by .039 among prime age men and women during the 1980s. Joulfaian and Wilhelm also analyze labor supply of prime age men and women during the 1980s, and find relatively smaller effects: a $100,000 inheritance would have caused a

[27] Brown et al. (2010) also report estimates using a long-difference sample, with one observation per household summarizing labor force exits between 1994 and 2002. These estimates are not directly comparable to ours.

reduction in annual hours of work of 24 for women, and less for men. We can also compare our estimates of the inheritance effects to estimates of the effects of lottery winnings on labor supply, an arguably similar type of shock to wealth. Using a measure of labor force participation defined by an annual earnings threshold of about $4,000, Cesarini et al. (2013) estimate that winning a $100,000 lottery prize would cause LFP to fall by .011 among a sample of Swedish men and women ages 21 to 64. Imbens et al. (2001) estimate a marginal propensity to earn from lottery winnings of -.167 for 55-65 year old winners in the 1980s. If the change in earnings was due entirely to changes in LFP, holding hours worked constant, this implies that winning a $100,000 lottery would cause LFP to decline by .052.[28] Overall, our results are well within the range of estimates from the previous literature.

The p-value for the test of commitment is shown at the bottom of the columns in Table 2. The test is for the equality of the coefficients on own and spouse inheritance. We show results for both one and two tailed tests, although as discussed above the theory does not predict the sign of the difference in coefficients if full commitment does not hold. Nevertheless, the most plausible alternative to equality is that the own effect exceeds the spouse effect in absolute value (which is the case in all results reported in Table 2). The null hypothesis of commitment is not

[28] Imbens et al. estimate a model of annual earnings of the form $y = \beta_0 + \beta_1 L/20$, where $L$ is the total payout, which is spread over 20 years. Their estimate of $\beta_1$ is -.0167 for ages 55-65. Let $E(y) = E(wHI)$, where w is the hourly wage, H is annual hours worked, E is the expectations operator, and I is a dummy for LFP. If the lottery only affects I, then $dy/dL = \beta_1/20 = wHdE(I)/dL$, so $dE(I)d/L = \beta_1/(20wH) = -.000000521$, given the (overall sample) mean value of wH of $16,000. Multiply this by $100,000 to get the effect of winning a $100,000 lottery on LFP: -.0521. Cesarini et al. estimate a regression of the form $I = \beta_0 + \beta_1 L$, with $L$ measured in units of 100,000 Swedish Krona. The coefficient estimate is -.0017 (Table 6). 100,000 Krona is equivalent to $16,000, so the effect of winning a $100,000 lottery is (100/16)*(-.0017) = -.011. Holtz-Eakin et al. estimate a logit model of transitions from employment to non-employment for a sample of unmarried inheritance recipients, using a quadratic in the inheritance amount. From their Table 3 (column 3), we computed the marginal effect of a $100,000 inheritance as -.039, evaluated at $L$ = $164,000 and mean LFP = 0.82. The median inheritance in their sample was $82,000 in 1982 dollars (we computed this as a weighted average of medians reported by group in their Table 1). We doubled this to account for inflation since 1982. Joulfaian and Wilhelm computed the estimates cited in the text based on their Table 5 for women and Table 3 for men.

rejected at the 10% level in any of the results using a two-tailed test, and is rejected at the 10% level for women using a one-tailed test. As noted above, while inheritances are not rare in this population, they are not very common at a two-year frequency (5%), so lack of sufficient statistical power could be one reason for the less-than-decisive results. Below, we explore ways to increase the precision of the estimates.

*C. Additional Results*

Table 3 presents results from estimates that condition on LFP = 1 at the previous interview in the upper panel, and LFP = 0 in the lower panel. The estimates are surprisingly similar for LF exit and entry, with only one exception (men, lagged LFP=0, continuous). Results of tests of commitment are mixed; we reject the null hypothesis at the 5% level in the binary specification for men with lagged LFP=0, and at the 10% level in the binary specification for women with lagged LFP = 1.

We next utilize more flexible specifications that allow nonlinear effects of the amount inherited. The upper panel of Table 4 reports results from a specification that includes indicators for whether the inheritance amount is above or below the median (conditional on receiving an inheritance; not receiving is the omitted category), and in the lower panel the results are from a specification with indicators for the quartile of the inheritance distribution. In three of the four sets of results shown, the effects are very similar for inheritances of different magnitudes, suggesting a nonlinear effect. The exception is the median specification for men, where the effect of receiving an above-median inheritance is much larger than the effect of receiving a below median inheritance, although equality cannot be rejected statistically. The cross-spouse

inheritance effects show no evidence against a nonlinear specification, but are too imprecisely estimated to be useful.

The main specification used in this paper does not impose a tight relationship between the effect of expectations and actual inheritance receipt. An alternative approach is to construct a measure of unanticipated inheritances, $I_{it} - E_{it-1}$, as the main explanatory variable. This imposes more structure but is perhaps easier to interpret. The specification in this case is

$$y_{it} = \beta_0 + \beta_1(I_{it} - E_{it-1}) + \beta_2(I_{jt} - E_{jt-1}) + \beta_3 I_{ut} + \beta_3 X_{it-1} + \beta_4 X_{jt-1} + \beta_5 Z_i + \varepsilon_{it}.$$

In order to construct such a measure we have to convert expectations, which are measured over a ten-year horizon, to the same horizon as actual receipt, which is measured since the previous interview, two years on average. If we assume that the respondent's subjective probability of receiving an inheritance is the same in each of the five two year periods covered by her response to the ten year expectation question, the probability of receiving an inheritance in the next two years is $q = 1 - (1-p)^{1/5}$, where $p$ is the ten year probability. For the case in which inheritance receipt is measured by a binary indicator, we use $I_{it} - q_{it-1}$ as the "surprise." Conditional on a non-zero probability of receipt, the amount expected is independent of the horizon.

A benefit of this approach is that it allows us to examine the possibility of asymmetry in response to positive and negative shocks. It seems intuitive that the negative "surprise" of not receiving an inheritance in a given period, given a positive subjective probability, may evoke a smaller change in labor supply than would the positive surprise of receiving one, conditional on a subjective probability less than one. Thus in this specification we allow the effects of positive and negative shocks to differ.

Table 5 reports results from a specification in which the inheritance variables appear in the form of amount received minus amount expected, or actual receipt [0, 1] minus subjective probability of receipt. The upper panel shows results that do not distinguish positive and negative shocks, as a baseline. The effects of own inheritance receipt relative to expectations are small for the binary specification, but similar to the results in Table 2 for the continuous specification. The results in the lower panel show that the effect on LFP of a positive inheritance surprise is larger in absolute value than the effect of a negative inheritance surprise in three out of four cases. Furthermore, in three of the four cases the effects of a positive surprise for the spouse's inheritance are positive, so the differences between own and spouse effects are relatively large in these cases. The null hypothesis of full commitment is rejected for positive surprises at the 5% level in two cases for a two-tailed test, and in three cases at the 10% level for a one-tailed test. In contrast, the null hypothesis is not rejected in any of the cases for negative surprises.[29]

An interesting question is whether there are observable subgroups of couples for which commitment is *a priori* more plausible. We examine this issue using three alternative approaches. First, we use self-reported information about the marriage that may help distinguish between couples more and less likely to be able to agree to self-enforcing contracts. Specifically, we identify three relevant measures in the HRS data: whether the spouses agree on who has the most say in major decisions, whether they both say that the time they spend with

[29] The results in Table 5 use the same sample as in the previous tables, which include cases with missing inheritance expectations. In previous tables, missing values were replaced by zeros, and dummies for such cases were included. In the amount-minus-expected specification, which combines actual and expected inheritances, this may not be an innocuous approach. We re-estimated the models in Table 5 using smaller samples that dropped cases with missing expectations. The results were similar to those reported in Table 5.

their spouse is "extremely enjoyable", and whether they both say that they like to spend their free time "together".[30] It seems plausible that couples who agree on these statements are more likely to behave as if they have signed a binding contract at the beginning of marriage. However, these measures were collected many years after the beginning of the marriage for most couples, so they may not convey much information about commitment at the time of the marriage. In the second and third alternatives, we allow the effect of the inheritance to vary by whether or the household has any living children, and by the length of the marriage. We hypothesize that couples with children and couples who have been married longer are more likely to exhibit behavior consistent with commitment, compared to childless and more recently married couples.

The results in panel A of Table 6 show limited support for differences in commitment according to whether the couple agrees in their responses to the three questions. (In this and most of the remaining discussion we focus on results from the binary specification). Among men in couples who disagree with the three questions, the null hypothesis of commitment can be rejected at the 10% level, while the null cannot be rejected among men in couples who agree (in fact the point estimate on own inheritance is smaller in magnitude than on spouse's inheritance, although both are imprecisely estimated). However, among women the difference in the own and spouse inheritance effects is very similar for couples who agree and disagree in their responses to the statements. The results in panel B show that the difference between own and spouse inheritance effects is smaller for women (but not men) in couples with children, but the results for women without children are rather perverse, so this finding is not very convincing. And in panel C the differences in own and spouse inheritance effects are larger for men in longer

[30] These measures were previously used by Friedberg and Webb (2006) and Maestas (2001) in analysis of retirement decisions.

marriages compared to those that have been married fewer than ten years, but again this is due in part to perverse results. The only clear support for the proposition is for women, where there is a small difference between own and spouse inheritance effects for longer-married couples, and a large difference for shorter marriages. However, the differences in differences are themselves not significantly different from zero.

The results discussed so far are short run effects that occur within two years of receiving an inheritance. Our model is dynamic, so we can trace out longer run impacts as well, accounting for effects transmitted via once-lagged and twice-lagged own and spouse LFP, as well as effects arising from once-lagged and twice-lagged inheritance receipt (see Appendix Table D for coefficient estimates on these variables). The cumulative effects operate through both the husband and wife equations as a consequence of the cross-spouse effects. Table 7 shows cumulative effects of inheritance shocks through three periods (six years) based on the estimates in Table 2 and Appendix Table D. The own-inheritance effects are two to three times larger after three periods compared to the one-period effects reported in Table 2, and spouse effects are larger as well. In two of the four models (women continuous, men binary), the own effects increase by more than do the spouse effects, leading to greater precision in the test for full commitment. However, in the binary specification for women, the spouse effect increases by more than does the own effect, so the one-sided test that rejects commitment at the 10% level in the short run (see Table 2) no longer rejects in the longer run.

*D. Robustness Analysis*

In the remainder of this section we discuss the robustness of our results. First, we return to the issue of how to deal with inheritances for which the recipient within the household cannot

be identified. Table 8 shows results from five different approaches to dealing with such cases. Panel A repeats results from Table 2, where inheritances from an unknown source enter separately in the specification. As noted above, receipt of an inheritance whose owner within the household is unknown has a large effect on LFP of men, more than twice as large as the own-inheritance effect. This suggests that men may have more control of inheritances with an unknown recipient. The financial respondent is male in roughly 60% of observations, which could lead to better reporting of inheritances from relatives of the male. This suggests a specification in which inheritances with an unknown recipient are pooled with inheritances received by men. This would also imply that, for women, inheritances received by the spouse should be pooled with those with an unknown recipient. Results for this specification are presented in Panel B of Table 8. The results are similar to those in panel A, but the estimates are more precise, rejecting the null hypothesis of full commitment at the 10% level using a one-tailed test in all four cases, but not rejecting in any of the four cases at the 5% level.

Another approach to dealing with inheritances for which the recipient is unknown is to drop those cases. Panel C reports results using this approach. The results are very similar to those in the first panel, with a slight loss of precision.

Panel D reports results in which only observations with an inheritance received by the financial respondent are used (along with non-recipients, as usual). This is based on the point made by Laitner and Sonnega (2010) that there appears to be significant underreporting of inheritances for non-financial respondents. The results for men are quite similar to those in Panel A, although quite a bit less precise, but the coefficient estimates for women are smaller and in one case of a different sign. This is not surprising given that the majority of financial respondents

are men. Many inheritances received by women omitted in this approach. Laitner and Sonnega (2010) also provide evidence from the HRS that inheritance reporting is generally more accurate when the inheritance is received by the financial respondent. This suggests that "unknown" sources of inheritance are more likely to belong to the non-financial respondent. We follow this approach in Panel E, and again, estimates are similar to the preceding panels. Overall, the results are not very sensitive to the treatment of unknown inheritance recipients.[31]

Table 9 reports results using alternative dependent variables and sample selection rules. The first panel shows results for alternative binary measures of LFP, for which the dependent variable is equal to one for currently working for pay**,** working year-round, working full time, and working year-round full time, respectively. The effects of own inheritances are robust to these alternative measures of employment, with a tendency toward larger effects for men than in models of LFP. Spouse inheritance effects are all small. The next set of results is for weekly and annual hours of work. The own inheritance effects are all negative and significantly different from zero, and spouse effects are much smaller in absolute value and not significantly different from zero.

The second panel presents results from regressions using samples of men and women with a relatively strong attachment to the labor force: work experience of at least 10 years, job tenure of at least two years (if employed), and out of the labor force for less than five years (if not employed). The own inheritance effects are 20-33% smaller than those reported in Table 2.

[31] Gouskova (2013) provides evidence of another source of measurement error: "forward telescoping" in inheritance reporting, a type of memory error when events are remembered as occurring more recently than they actually did. As a result, respondents might report the same inheritance more than once if an inheritance that was received before the previous interview is erroneously recalled as having occurred after the interview. When we estimate our models using corrected measures of inheritances as suggested by Gouskova (2013), the results (not shown) are nearly identical to the main results presented in Table 2.

Overall, Tables 8 and 9 show that our results are quite robust to alternative measures and samples.

## 5. Conclusions

This paper provides new evidence on the ability of spouses to commit to a fully efficient allocation of resources within the household. The analysis exploits unique data from the HRS on inheritance expectations and the identity of the recipients of actual inheritances received by married couples. Controlling for inheritance expectations, we interpret the impact of inheritance receipt as a shock to wealth. The addition to wealth is under the control of the recipient and is not subject to marital property law, so it can be interpreted as a distribution factor: a variable that affects bargaining power within the household but has no direct effect on preferences or the joint budget constraint. The results are inconsistent with the hypothesis that households are able to commit to an *ex ante* efficient contract at the beginning of marriage. Commitment ability has been tested and rejected in other contexts, as discussed in the introduction, so our evidence is consistent with previous evidence on this issue.

The finding that commitment is infeasible, at least for older households, has implications for Social Security policy, specifically for proposed reforms to spouse and survivor benefits. Under current policy a spouse (typically the wife) who has worked only intermittently may be eligible for a spouse benefit equal to 50% of her husband's benefit, if the spouse benefit exceeds the benefit to which she is entitled based on her own earnings history. Upon the death of her spouse, a woman whose retired-worker benefit is less than the benefit of her deceased spouse is eligible for a survivor benefit equal to 100% of his benefit. Martin (2012) argues that this policy

results in many inequities and unintended consequences, and is inconsistent with the community property or equitable distribution approach to marital assets, including employer-provided pensions. One proposal to reform the system would be to combine the earnings received by spouses during marriage and divide them equally between the spouses for purposes of determining Social Security benefits. Because spouses are unable to commit to a long term contract, one consequence of such a reform would be to increase the decision power of low-earning wives.[32]

A natural extension of the analysis is to exploit inheritance shocks for structural estimation and identification of an intertemporal collective model of the household. Voena (in press) solves and estimates a model without commitment, using divorce and marital property law changes as distribution factors for identification. The advantages of using inheritances as distribution factors are that there is household-level variation rather than only cross-state and time variation for divorce and marital property law, and data on inheritance expectations can be used in a natural way in estimation (see van der Klaauw and Wolpin, 2008, for an example of structural estimation using expectations data).

[32] Mazzocco (2007) argues that the inability of households to commit to a long term contract makes it possible for public programs to redistribute decision power within the household.

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# Appendix

## *A. Distribution of Inheritances and Outlier Values*

Appendix Table A summarizes the distribution of inheritance receipts in our data, *including* the 33 couple-wave observations with an inheritance of $617,550 or more that were trimmed from our main analysis sample. Inheritance receipts are highly skewed, as the 95$^{th}$ percentile value ($436,000) is an order of magnitude higher than the median value ($40,000), and the 99$^{th}$ percentile value ($1.24m) is 30 times larger than the median. The skewness of the inheritance distribution is generally similar across inheritances assigned to the husband, wife, and unknown.

The extremely large inheritances at the upper tail of the distribution could be correct, since the distribution of wealth in the U.S. is highly skewed. Nonetheless, these observations may have a large influence on our estimates. This turned out to be the case, as illustrated in Appendix Table B. The table shows estimates of the effects of inheritances in the regression specification presented in Table 2 for alternative samples where inheritances above the 95$^{th}$ to the 99$^{th}$ percentile value are dropped. The results show significant sensitivity of the estimated effect of the amount inherited, but appear to stabilize around around the 97$^{th}$ percentile, $622,000.

A natural approach to dealing with outliers in inheritances would be to use the logarithm of inheritance as the explanatory variable. However, in roughly 95 percent of observations the inheritance amount is zero, so much of the variation comes from the extensive margin of receipt.

Instead, we drop the 33 couple-wave observations with inheritances above $617,550 (the 97th percentile) from the sample.[33]

*B. Social Security Wealth*

We use administrative Social Security data on annual covered earnings of HRS respondents from 1951 to 2003 for the original HRS cohort, and from 1951 to 1999 for the War Baby and Early Boomer cohorts. In addition, we use annual earnings for the previous calendar year reported by HRS respondents in even-numbered survey years. These earnings, capped at the maximum taxable earnings, are used to extend the earnings series through 2007 and to fill in missing odd years from 1991 to 2003. We assign the average of earnings in adjacent years to fill in earnings for years with missing data. If the last observation on earnings prior to the assumed date of labor force exit in a given scenario (see below for the scenarios) is missing, we assign the value from the preceding year. Pre-1951 earnings were set equal to 1951 earnings. Labor force entry was assumed to occur at age $a = \max\{e + 6, 16\}$, where $e$ is years of schooling completed.

The earnings data are used as input to the Social Security Administration's *anypia* program to calculate Social Security retirement and disability benefits under a variety of scenarios.[34] For each survey wave observation on a given individual (even numbered years from 1996 to 2008) the scenarios include the following: (1) Stop working in the current year, never return to work, and claim at the first date of eligibility (the current year, if already at least 62). (2) Work until age 62, claim at age 62. (3) Work until age 65, claim at 65. (4) Work until 70, claim at 70. (5) Stop working in the current year, apply for SSDI benefits, and begin receiving

[33] We note that the results for the binary inheritance indicators (not shown) are much less sensitive to the inclusion of the outlier observations, as one would expect. However, for consistency throughout the paper we use the same (trimmed) analysis sample for both the binary inheritance indicators and continuous inheritance amounts.
[34] We use the batch version of the calculator, anypiab, available at http://www.ssa.gov/oact/anypia/anypiab.html.

SSDI benefits in the following year. For scenarios (2)-(4) we do not use observed earnings for the years between the survey year of interest and the assumed year of claiming, because these reflect actual work behavior, and our scenarios assume constant earnings until retirement. We impute earnings for the years between the survey year of interest and the assumed year of claiming using the average of the five most recent years of earnings observed prior to the survey year.

The annual benefits calculated by the anypia program under each scenario are used to compute Social Security Wealth, the expected present discounted value of remaining lifetime benefits, discounted at a 3% rate back to the survey year, using life table mortality schedules to discount for survival risk. We assume that benefits remain constant in real terms after claiming. We use benefits for both the husband and wife to calculate SSW, because benefits are determined at the household level. For each study year observation, we compute the spouse benefit corresponding to the individual's retired worker benefit, and assign to the spouse the larger of the spouse benefit and her (or his, if the wife's retired worker benefit is larger) own retired worker benefit. We make the following assumptions about when the spouse claims his or her benefit in a given scenario:

- If the spouse's benefit is based on her own earnings (because her retired worker benefit exceeds her spouse benefit) the spouse claims at the same age as the respondent in the scenario: 62, 65, or 70 (which will be in a different year unless the spouses are the same age).

- If the spouse's benefit is based on her husband's earnings record, then she claims at 62 if she is young than her spouse, and she claims when he turns 62 if she is older than her spouse.

We account for joint survival probabilities (both survive to the next year, the husband survives and the wife dies, the husband dies and the wife survives, and both die), and assign a survivor benefit for the cases in which one spouse is assumed to die and the other is assumed to live.

*C. Defined Benefit Pension Wealth*

We use employer-reported data on the Defined Benefit pension plans held by HRS respondents. The HRS provides these data along with software that can be used to calculate benefits under alternative scenarios regarding earnings, inflation, interest rates, and the date of claiming. We use a single self-reported annual earnings observation in this case, along with an assumed growth rate of zero, instead of the earnings series described above, for two reasons. First, Social Security earnings are capped at the maximum taxable level, and the cap is binding in many cases. Second, the self-reported earnings variable is built into the calculator, and it is very cumbersome to use the pension calculator with a user-provided earnings series. The pension calculator is used to compute the present discounted value of the annuity to which an individual would be entitled, using an assumed real interest rate of 3% and life table mortality schedules, for each of the first four employment-claiming scenarios described above. A two thirds joint and survivor annuity is assumed, and mortality risk of the spouse is incorporated in the present value calculations.

*D. Defined Contribution Balances*

We use respondent-reported DC balances because relatively few DC plans held by respondents are included in the pension data base. Balances are summed for all plans held by a respondent at a given survey wave. We also used DC balances computed by the pension calculator, as an alternative to respondent-reported balances, with similar results.

*E. Lifetime Earnings*

We use average capped earnings in the five years prior to the survey year as our measure of average lifetime earnings. As noted above, we assume constant real earnings from a given survey year until the assumed year of retirement, and we use average earnings in the five years prior to the survey year to project earnings forward. We could use observed capped earnings up to the survey year, but this will be equal to zero if the individual actually did not work in one of those years.

SSW, DB Pension Wealth, and DC Balances are deflated to 2005 dollars using the CPI-U.

**Table 1: Inheritance Receipts and Expectations**

| Variable | N | Mean | p50 | p90 | p95 | p99 |
|---|---|---|---|---|---|---|
| *(a) Inheritance Receipt Indicators* | | | | | | |
| Received over period of observation | 5,937 | 0.155 | | | | |
| Received since previous survey | 21,481 | 0.052 | | | | |
| Received since previous survey - Husband | 21,481 | 0.023 | | | | |
| Received since previous survey - Wife | 21,481 | 0.021 | | | | |
| Received since previous survey - Unknown source | 21,481 | 0.010 | | | | |
| Missing: received since previous survey | 21,481 | 0.006 | | | | |
| Missing: amount received | 21,481 | 0.002 | | | | |
| *(b) Conditional Inheritance Amounts (1000s)* | | | | | | |
| Received since previous survey | 1,062 | 74.3 | 37.6 | 182.5 | 278.7 | 516.9 |
| Received since previous survey - Husband | 456 | 74.3 | 36.5 | 188.4 | 271.3 | 530.7 |
| Received since previous survey - Wife | 424 | 81.4 | 45.6 | 187.6 | 330.8 | 551.4 |
| Received since previous survey - Unknown source | 201 | 52.2 | 25.6 | 125.0 | 207.9 | 325.7 |
| *(c) Inheritance Expectations (lagged 1 period): Husband* | | | | | | |
| Probability of Receiving an Inheritance > 0 | 21,481 | 0.355 | | | | |
| Conditional Inheritance Probability (%) | 7,621 | 55.3 | 50.0 | 100.0 | 100.0 | 100.0 |
| Conditional Expected Inheritance (1000s) | 6,905 | 103.6 | 35.9 | 206.8 | 395.3 | 1033.9 |
| Missing: Probability of Receiving | 21,481 | 0.151 | | | | |
| Missing: Exp Inheritance Amount | 21,481 | 0.031 | | | | |
| *(d) Inheritance Expectations (lagged 1 period): Wife* | | | | | | |
| Probability of Receiving an Inheritance > 0 | 21,481 | 0.421 | | | | |
| Conditional Inheritance Probability (%) | 9,038 | 59.1 | 50.0 | 100.0 | 100.0 | 100.0 |
| Conditional Expected Inheritance (1000s) | 8,080 | 95.3 | 34.0 | 197.7 | 311.2 | 968.8 |
| Missing: Probability of Receiving | 21,481 | 0.048 | | | | |
| Missing: Exp Inheritance Amount | 21,481 | 0.042 | | | | |

Notes: The sample has 42,962 person-wave (or 21,481 household-wave) observations on 5,937 married-couple households. Dollar amounts are deflated by the Consumer price Index to the year 2005.

**Table 2: OLS Estimates of the Effects of Inheritances on Labor Force Participation**

| | Binary indicator of inheritance receipt | | Amount Inherited | |
|---|---|---|---|---|
| Selected Covariates | Men | Women | Men | Women |
| *Inheritance recipient* | | | | |
| Self | -0.030* | -0.035* | -0.019 | -0.032** |
| | (0.017) | (0.018) | (0.016) | (0.014) |
| Spouse | -0.008 | -0.005 | -0.009 | -0.002 |
| | (0.017) | (0.014) | (0.011) | (0.013) |
| Unknown | -0.066*** | -0.008 | -0.050* | -0.013 |
| | (0.025) | (0.021) | (0.027) | (0.027) |
| *Own Inheritance expectations* | | | | |
| Percent chance | 0.00018** | -0.00001 | 0.00016** | -0.00002 |
| | (0.00008) | (0.00007) | (0.00008) | (0.00008) |
| % chance missing | 0.007 | -0.014 | 0.007 | -0.014 |
| | (0.008) | (0.011) | (0.008) | (0.011) |
| Amount expected | -0.000 | -0.001 | -0.000 | -0.001 |
| | (0.001) | (0.002) | (0.001) | (0.002) |
| Amount missing | -0.012 | 0.006 | -0.011 | 0.006 |
| | (0.013) | (0.011) | (0.013) | (0.011) |
| *Spouse Inheritance expectations* | | | | |
| Percent chance | -0.011* | 0.006 | -0.011** | 0.006 |
| | (0.006) | (0.006) | (0.006) | (0.006) |
| % chance missing | -0.030*** | -0.001 | -0.030*** | -0.001 |
| | (0.011) | (0.008) | (0.011) | (0.008) |
| Amount expected | 0.002 | -0.000 | 0.002 | -0.000 |
| | (0.001) | (0.000) | (0.001) | (0.000) |
| Amount missing | 0.006 | -0.007 | 0.006 | -0.007 |
| | (0.011) | (0.013) | (0.011) | (0.013) |
| *Labor Force Status* | | | | |
| Own LFP (one lag) | 0.566*** | 0.568*** | 0.566*** | 0.568*** |
| | (0.009) | (0.009) | (0.009) | (0.009) |
| Own LFP (two lags) | 0.124*** | 0.129*** | 0.124*** | 0.130*** |
| | (0.008) | (0.008) | (0.008) | (0.008) |
| Spouse LFP (one lag) | 0.044*** | 0.043*** | 0.044*** | 0.043*** |
| | (0.007) | (0.007) | (0.007) | (0.007) |
| Spouse LFP (two lags) | -0.012* | -0.009 | -0.012* | -0.008 |
| | (0.007) | (0.007) | (0.007) | (0.007) |
| R squared | 0.552 | 0.575 | 0.552 | 0.575 |
| P value, test of commitment | | | | |
| One-sided | 0.200 | 0.084 | 0.298 | 0.061 |
| Two-sided | 0.399 | 0.169 | 0.596 | 0.122 |

Notes: The sample size is 21,481 household-wave observations. Each column shows results from a different regression. Inheritance amounts are measured in units of $100,000 (deflated to 2005 dollars). Ordinary Least Squares (OLS) estimates are reported, with standard errors clustered at the household level. See Appendix Table D for parameter estimates on the

other variables included in the models using the binary inheritance indicator. The alternative hypothesis for the one-sided test of commitment is that the coefficient estimate on inheritances received by self is greater than by spouse in absolute value, and for the two-sided test is that the coefficient estimates on self and spouse inheritances are not equal. One, two, and three asterisks indicate that the coefficient estimate is significantly different from zero at the 10%, 5%, and 1% level, respectively.

**Table 3: Alternative Estimates, Sample Stratified by Lagged LFP**

| | Binary indicator of inheritance receipt | | Amount Inherited | |
|---|---|---|---|---|
| Selected Covariates | Men | Women | Men | Women |
| *(a) Alternative Sample: Lagged LFP = 1* | | | | |
| Self | -0.028 | -0.042* | -0.025 | -0.037* |
| | (0.021) | (0.025) | (0.018) | (0.020) |
| Spouse | -0.027 | 0.008 | -0.013 | -0.006 |
| | (0.019) | (0.020) | (0.012) | (0.017) |
| P value, test of commitment | | | | |
| One-sided | 0.496 | 0.052 | 0.294 | 0.120 |
| Two-sided | 0.993 | 0.104 | 0.587 | 0.239 |
| *(b) Alternative Sample: Lagged LFP = 0* | | | | |
| Self | -0.043* | -0.022 | 0.002 | -0.023*** |
| | (0.025) | (0.022) | (0.030) | (0.009) |
| Spouse | 0.039 | -0.030 | 0.014 | 0.004 |
| | (0.034) | (0.020) | (0.024) | (0.022) |
| P value, test of commitment | | | | |
| One-sided | 0.022 | 0.610 | 0.381 | 0.116 |
| Two-sided | 0.044 | 0.781 | 0.762 | 0.232 |

Notes: Sample sizes for the sample with Lagged LFP = 1 are 14,550 for men and 12,514 for women, and for Lagged LFP = 0 are 6,931 for men and 8,967 for women. Estimates for inheritance receipt from unknown source are not shown. See Table 2 for additional notes.

**Table 4: Estimates Allowing Nonlinear Inheritance Effects**

| Sample | Men | | Women | |
|---|---|---|---|---|
| Recipient | Self | Spouse | Self | Spouse |
| *(a) Indicators for inheritance amount below or above median* | | | | |
| Below median | -0.014 | 0.029 | -0.036 | -0.015 |
| | (0.021) | (0.025) | (0.027) | (0.019) |
| Above median | -0.049* | -0.029 | -0.034 | -0.008 |
| | (0.026) | (0.023) | (0.022) | (0.020) |
| *(b) Indicators for quartile of inheritance amount distribution* | | | | |
| Quartile 1 (lowest) | -0.066** | 0.049 | -0.026 | -0.034 |
| | (0.033) | (0.032) | (0.038) | (0.028) |
| Quartile 2 | 0.026 | 0.004 | -0.047 | 0.002 |
| | (0.025) | (0.039) | (0.038) | (0.024) |
| Quartile 3 | -0.026 | -0.034 | -0.036 | 0.008 |
| | (0.032) | (0.034) | (0.029) | (0.028) |
| Quartile 4 (highest) | -0.069* | -0.023 | -0.032 | -0.025 |
| | (0.038) | (0.030) | (0.033) | (0.032) |

Notes: The first two columns report results from three different regressions for men, and the last two columns report results for three different regressions for women. The omitted category in the first two panels is non-recipients. See Tables 2 and 3 for additional notes.

**Table 5: Estimates of the Effect of Inheritance Receipt Relative to Expectations**

| | Binary indicator of inheritance receipt | | Amount Inherited | |
|---|---|---|---|---|
| | Men | Women | Men | Women |
| *(a) Inheritance Relative to Expectations* | | | | |
| Self | 0.001 | -0.007 | -0.019 | -0.032** |
| | (0.012) | (0.012) | (0.016) | (0.014) |
| Spouse | 0.005 | 0.012 | -0.009 | -0.005 |
| | (0.009) | (0.009) | (0.011) | (0.013) |
| P value, test of commitment | | | | |
| One-sided | 0.379 | 0.097 | 0.308 | 0.079 |
| Two-sided | 0.757 | 0.194 | 0.616 | 0.158 |
| *(b) Inheritance Relative to Expectations, by Sign* | | | | |
| Self - Positive | -0.016 | -0.043* | -0.004 | -0.038* |
| | (0.018) | (0.022) | (0.014) | (0.021) |
| Spouse - Positive | 0.033 | 0.012 | -0.002 | 0.027 |
| | (0.025) | (0.020) | (0.017) | (0.018) |
| Self - Negative | 0.017** | 0.007 | -0.040 | -0.024 |
| | (0.008) | (0.008) | (0.033) | (0.024) |
| Spouse - Negative | 0.040* | 0.015 | -0.019 | -0.048** |
| | (0.024) | (0.020) | (0.019) | (0.020) |
| P value, test of commitment | | | | |
| Positive: One-sided | 0.057 | 0.032 | 0.469 | 0.010 |
| Two-sided | 0.114 | 0.065 | 0.937 | 0.020 |
| Negative: One-sided | 0.185 | 0.364 | 0.293 | 0.779 |
| Two-sided | 0.370 | 0.728 | 0.586 | 0.442 |

Notes: The first two columns report results from two different regressions for men, and the last two columns report results from two different regressions for women. See Tables 2 and 3 for additional notes, and see text for detail on measures of inheritances relative to expectations.

**Table 6: Estimates Allowing Effects to Vary by Characteristics of the Household, Binary Indicator of Inheritance Receipt**

| | Men | | Women | |
|---|---|---|---|---|
| | Self | Spouse | Self | Spouse |
| *(a) Subjective Commitment Indicator* | | | | |
| Agreement | -0.050 | -0.101 | -0.056 | -0.026 |
| | (0.058) | (0.072) | (0.052) | (0.040) |
| Disagreement | -0.047** | -0.007 | -0.058*** | -0.022 |
| | (0.020) | (0.021) | (0.021) | (0.016) |
| *(b) Couple has Living Children* | | | | |
| Has Children | -0.028* | -0.010 | -0.039** | -0.005 |
| | (0.017) | (0.018) | (0.018) | (0.014) |
| No Children | -0.127 | 0.050* | 0.163*** | -0.079 |
| | (0.093) | (0.028) | (0.058) | (0.074) |
| *(c) Length of Marriage* | | | | |
| Married < 10 years | 0.050 | 0.052 | -0.163* | 0.036 |
| | (0.049) | (0.091) | (0.089) | (0.048) |
| Married >= 10 years | -0.036** | -0.011 | -0.028 | -0.009 |
| | (0.017) | (0.017) | (0.018) | (0.014) |

Notes: The first two columns report results from three different regressions for men, and the last two columns report results from three different regressions for women. The estimates in panel A are from a specification where the binary indicator of inheritance receipt is interacted with a subjective commitment indicator set to "agreement" if the husband and wife both agree on who has the most say in major decisions, both say that the time they spend with their spouse is "extremely enjoyable", and both say that they like to spend their free time "together". Otherwise the indicator is set to "disagreement". The estimates in panels B and C are from specifications where the inheritance indicator is interacted with indicators for living children and length of marriage, respectively. See Tables 2 and 3 for additional notes.

**Table 7: Cumulative Inheritance Effects on Labor Force Participation, Three Periods (Six Years)**

| | Binary indicator of inheritance receipt | | Amount Inherited | |
|---|---|---|---|---|
| Selected Covariates | Men | Women | Men | Women |
| Self | -0.099** | -0.064 | -0.047 | -0.085*** |
| | (0.041) | (0.044) | (0.034) | (0.030) |
| Spouse | -0.026 | -0.068* | -0.029 | -0.016 |
| | (0.044) | (0.039) | (0.029) | (0.027) |
| P value, test of commitment | | | | |
| One-sided | 0.103 | 0.532 | 0.344 | 0.044 |
| Two-sided | 0.206 | 0.937 | 0.688 | 0.087 |

Notes: The cumulative effect of inheritances on labor force participation is computed using the estimates in Table 2 and in Appendix Table D. See Table 2 for additional notes.

**Table 8: Estimated Effects, Alternative Treatment of Inheritances from Unknown Source**

| | Binary indicator of inheritance receipt | | Amount Inherited | |
|---|---|---|---|---|
| Selected Covariates | Men | Women | Men | Women |
| *(a) Unknown enters separately* | | | | |
| Self | -0.030* | -0.035* | -0.019 | -0.032** |
| | (0.017) | (0.018) | (0.016) | (0.014) |
| Spouse | -0.008 | -0.005 | -0.009 | -0.002 |
| | (0.017) | (0.014) | (0.011) | (0.013) |
| Unknown | -0.066*** | -0.008 | -0.050* | -0.013 |
| | (0.025) | (0.021) | (0.027) | (0.027) |
| P value, test of commitment | | | | |
| One-sided | 0.200 | 0.084 | 0.298 | 0.061 |
| Two-sided | 0.399 | 0.169 | 0.596 | 0.122 |
| *(b) Combine Unknown with Male* | | | | |
| Self | -0.042*** | -0.035** | -0.025* | -0.032** |
| | (0.014) | (0.018) | (0.014) | (0.015) |
| Spouse | -0.010 | -0.008 | -0.008 | -0.004 |
| | (0.017) | (0.012) | (0.011) | (0.012) |
| P value, test of commitment | | | | |
| One-sided | 0.072 | 0.094 | 0.177 | 0.064 |
| Two-sided | 0.144 | 0.188 | 0.355 | 0.128 |
| *(c) Drop Unknown* | | | | |
| Self | -0.029* | -0.036** | -0.024 | -0.032** |
| | (0.017) | (0.018) | (0.017) | (0.014) |
| Spouse | -0.010 | -0.009 | -0.009 | -0.006 |
| | (0.017) | (0.014) | -0.011 | (0.014) |
| P value, test of commitment | | | | |
| One-sided | 0.216 | 0.110 | 0.235 | 0.093 |
| Two-sided | 0.433 | 0.220 | 0.470 | 0.186 |
| *(d) Drop if Inheritance not Received from Parent of Financial Respondent* | | | | |
| Self | -0.030 | -0.007 | -0.021 | 0.011 |
| | (0.022) | (0.028) | (0.019) | -0.013 |
| Spouse | 0.018 | -0.014 | -0.020 | -0.011 |
| | (0.029) | (0.018) | (0.021) | (0.016) |
| P value, test of commitment | | | | |
| One-sided | 0.085 | 0.576 | 0.484 | 0.848 |
| Two-sided | 0.170 | 0.847 | 0.968 | 0.303 |

**Table 8 (continued)**

| | Binary indicator of inheritance receipt | | Amount Inherited | |
|---|---|---|---|---|
| Selected Covariates | Men | Women | Men | Women |
| *(e) Combine Unknown with Non-Financial Respondent* | | | | |
| Self | -0.039** | -0.024* | -0.026 | -0.029** |
| | (0.016) | (0.015) | (0.016) | (0.013) |
| Spouse | -0.017 | -0.011 | -0.010 | -0.003 |
| | (0.015) | (0.014) | (0.010) | (0.013) |
| P value, test of commitment | | | | |
| One-sided | 0.156 | 0.258 | 0.200 | 0.069 |
| Two-sided | 0.310 | 0.516 | 0.400 | 0.138 |

Notes: Sample sizes in panels C and D are 21,280 and 20,855 household-wave observations, respectively. See Tables 2 and 3 for additional notes.

**Table 9: Estimated Effects of Binary Indicator of Inheritance Receipt, Alternative Specifications**

| | Men | | Women | |
|---|---|---|---|---|
| | Self | Spouse | Self | Spouse |
| *(a) Alternative measures of Labor Supply* | | | | |
| Currently working for pay | -0.028* | 0.001 | -0.032* | -0.017 |
| | (0.017) | (0.017) | (0.018) | (0.015) |
| Year round | -0.029 | 0.002 | -0.043** | -0.026 |
| | (0.019) | (0.018) | (0.019) | (0.018) |
| Full time | -0.038* | -0.003 | -0.039** | -0.007 |
| | (0.020) | (0.019) | (0.020) | (0.017) |
| YRFT | -0.040** | 0.008 | -0.039** | -0.019 |
| | (0.020) | (0.019) | (0.020) | (0.018) |
| Weekly Hours of Work | -1.085 | -0.377 | -2.049*** | -0.497 |
| | (0.813) | (0.801) | (0.680) | (0.589) |
| Annual Hours of Work | -59.388 | -14.097 | -84.023** | -20.046 |
| | (42.090) | (41.487) | (34.299) | (28.778) |
| *(b) Alternative Sample* | | | | |
| Strong LF Attachment | -0.020 | -0.019 | -0.028 | 0.002 |
| | (0.020) | (0.018) | (0.022) | (0.019) |

Notes: Sample sizes for the sample with Strong LF attachment are 15,188 for men and 13,133 for women. The inheritance receipt variables are binary indicators. See Tables 2 and 3 for additional notes.

**Appendix Table A: Distribution of Inheritances by Source, Untrimmed Sample**

| Variable | N | mean | p50 | p95 | p96 | p97 | p98 | p99 | p99 | max |
|---|---|---|---|---|---|---|---|---|---|---|
| All | 1,095 | 108.2 | 40.0 | 435.9 | 516.9 | 622.4 | 1000.0 | 1244.7 | 1244.7 | 5861.3 |
| Source = Husband | 471 | 116.4 | 38.8 | 479.3 | 530.7 | 678.1 | 1061.4 | 1281.5 | 1281.5 | 5861.3 |
| Source = Wife | 437 | 110.2 | 47.8 | 479.3 | 551.4 | 617.5 | 1000.0 | 1244.7 | 1244.7 | 1317.8 |
| Source = Unknown | 206 | 75.5 | 25.8 | 260.5 | 310.2 | 413.6 | 649.9 | 1033.9 | 1033.9 | 1281.5 |

Note: Sample size is 21,514 household-year observations.

**Appendix Table B: Estimated Effect of Amount Inherited on LFP, Alternative Samples**

| Sample | | Men | | | Women | | |
|---|---|---|---|---|---|---|---|
| Cutoff | Nbr obs | Self | Spouse | Unknown | Self | Spouse | Unknown |
| None | 21514 | 0.007 | -0.013* | -0.0245** | -0.016* | 0.002 | -0.004 |
| | | (0.006) | (0.007) | (0.010) | (0.008) | (0.006) | (0.008) |
| 99th Percentile | 21502 | -0.003 | -0.023*** | -0.023 | -0.029*** | 0.000 | -0.015 |
| | | (0.007) | (0.009) | (0.019) | (0.009) | (0.008) | (0.016) |
| 98th Percentile | 21491 | -0.011 | -0.022* | -0.051* | -0.033*** | -0.008 | -0.013 |
| | | (0.012) | -(0.051) | (0.027) | (0.012) | -(0.013) | (0.027) |
| 97th Percentile | 21480 | -0.019 | -0.016 | -0.061* | -0.031*** | -0.002 | 0.017 |
| | | (0.016) | -(0.061) | (0.034) | (0.013) | (0.017) | (0.021) |
| 96th Percentile | 21468 | -0.017 | -0.007 | -0.07* | -0.023 | -0.007 | 0.013 |
| | | (0.017) | -(0.070) | (0.036) | (0.015) | (0.013) | (0.022) |
| 95th Percentile | 21458 | -0.028 | -0.015 | -0.089** | -0.011 | -0.003 | 0.011 |
| | | (0.017) | -(0.089) | (0.040) | (0.015) | (0.011) | (0.026) |

Notes: The first column of each row indicates the respective cutoff value for inheritance receipt; observations with inheritances over this value are trimmed from the sample.

## Appendix Table C: Descriptive Statistics

| | Male | | Female | |
|---|---|---|---|---|
| Variable | Mean | Std Dev | Mean | Std Dev |
| *Dependent Variable* | | | | |
| Labor Force Participation | 0.609 | (0.488) | 0.530 | (0.499) |
| *Wealth and Pensions* | | | | |
| Net worth | 4.833 | (16.451) | 4.833 | (16.451) |
| DC account balance missing | 0.086 | (0.281) | 0.070 | (0.255) |
| DB pension wealth missing | 0.122 | (0.327) | 0.084 | (0.278) |
| DB pension wealth for exit at survey date | 0.952 | (2.294) | 0.473 | (1.751) |
| Gain in DB pension wealth for exit at 65 | -0.095 | (0.785) | -0.032 | (0.604) |
| DC account balance | 0.235 | (3.399) | 0.060 | (0.365) |
| SS wealth for exit and claiming at survey date | 1.299 | (0.925) | 0.962 | (0.697) |
| Gain in SS wealth for exit and claiming at 65 | 0.266 | (0.258) | 0.248 | (0.226) |
| SS wealth for entry to SSDI at survey date | 1.309 | (1.277) | 1.261 | (1.216) |
| PDV of lifetime earnings at 65 | 9.098 | (7.257) | 3.997 | (4.816) |
| *Other Control Variables* | | | | |
| Education = High School | 0.339 | (0.473) | 0.401 | (0.490) |
| Education = Some College | 0.207 | (0.405) | 0.237 | (0.425) |
| Education = College Graduate | 0.256 | (0.437) | 0.190 | (0.392) |
| Hispanic | 0.086 | (0.281) | 0.086 | (0.280) |
| Black | 0.105 | (0.307) | 0.103 | (0.303) |
| Other race | 0.043 | (0.202) | 0.041 | (0.199) |
| Parent died since last interview | 0.074 | (0.262) | 0.079 | (0.270) |
| Employer retiree health insurance indicator | 0.235 | (0.424) | 0.153 | (0.360) |
| Retiree health insurance missing | 0.294 | (0.456) | 0.556 | (0.497) |
| Employer-provided health insurance indicator | 0.499 | (0.500) | 0.317 | (0.465) |
| Employer health insurance missing | 0.020 | (0.140) | 0.023 | (0.150) |
| Health good | 0.319 | (0.466) | 0.298 | (0.457) |
| Health fair poor | 0.223 | (0.417) | 0.201 | (0.401) |
| Health improved since last interview | 0.102 | (0.303) | 0.113 | (0.317) |
| Health worsened since last interview | 0.205 | (0.404) | 0.201 | (0.401) |
| Self employed | 0.167 | (0.373) | 0.090 | (0.286) |
| Age | 62.0 | (5.0) | 58.9 | (5.5) |
| Age = 62-64 | 0.195 | (0.396) | 0.173 | (0.378) |
| Age = 65+ | 0.353 | (0.478) | 0.173 | (0.379) |
| Probability live to age 75 | 44.1 | (37.8) | 58.9 | (34.0) |

Notes: Total sample size is 42,962 person-wave observations (21,481 observations for each gender). See Table 1 for descriptive statistics on the inheritance variables. All dollar amounts are in units of $100,000, deflated to year 2005.

## Appendix Table D: Other coefficient estimates for binary inheritance specification in Table 2

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| Inheritance Receipt Self (1 lag) | -0.022 | (0.016) | | 0.007 | (0.017) | |
| Inheritance Receipt Spouse (1 lag) | -0.006 | (0.018) | | -0.045 | (0.017) | *** |
| Inheritance Receipt Unknown (1 lag) | -0.005 | (0.018) | | -0.012 | (0.017) | |
| Inheritance Receipt Self (2 lag) | -0.005 | (0.015) | | -0.026 | (0.017) | |
| Inheritance Receipt Spouse (2 lag) | 0.007 | (0.017) | | 0.017 | (0.014) | |
| Inheritance Receipt Unknown (2 lag) | -0.002 | (0.021) | | 0.002 | (0.019) | |
| Net worth (1 lag) | 0.000 | (0.000) | | 0.000 | (0.000) | * |
| Net worth (2 lag) | 0.000 | (0.000) | | 0.000 | (0.000) | |
| DB pension wealth missing | 0.021 | (0.011) | * | 0.016 | (0.012) | |
| DB pension wealth of spouse missing | 0.000 | (0.012) | | 0.002 | (0.010) | |
| DC account balance missing | -0.029 | (0.010) | *** | -0.021 | (0.011) | * |
| DC account balance of spouse missing | -0.012 | (0.011) | | -0.021 | (0.010) | ** |
| SS wealth of spouse missing | -0.029 | (0.010) | *** | -0.021 | (0.011) | * |
| DB pension wealth for exit at survey date | -0.003 | (0.002) | ** | -0.001 | (0.002) | |
| Gain in DB pension wealth for exit at 65 | -0.003 | (0.004) | | 0.003 | (0.005) | |
| DC account balance | -0.001 | (0.000) | ** | -0.020 | (0.007) | *** |
| SS wealth for exit and claiming at survey date | -0.002 | (0.008) | | 0.001 | (0.009) | |
| Gain in SS wealth for exit and claiming at 65 | 0.029 | (0.014) | ** | 0.016 | (0.016) | |
| SS wealth for entry to SSDI at survey date | 0.005 | (0.003) | * | -0.001 | (0.003) | |
| PDV of lifetime earnings at 65 | 0.000 | (0.001) | | 0.002 | (0.001) | *** |
| DC pension indicator | 0.043 | (0.007) | *** | 0.031 | (0.008) | *** |
| DB pension indicator | -0.052 | (0.009) | *** | -0.019 | (0.010) | ** |
| DB pension coverage indicator spouse | -0.009 | (0.009) | | -0.017 | (0.008) | ** |
| DC pension coverage indicator spouse | 0.013 | (0.008) | | 0.007 | (0.007) | |
| DB pension wealth of spouse for exit at survey date | -0.003 | (0.002) | ** | -0.001 | (0.002) | |
| Gain in DB pension wealth of spouse for exit at 65 | -0.003 | (0.004) | | 0.003 | (0.005) | |
| DC account balance of spouse | -0.001 | (0.000) | ** | -0.020 | (0.007) | *** |
| SS wealth of spouse | -0.002 | (0.008) | | 0.001 | (0.009) | |
| Gain in SS wealth of spouse for exit and claiming at 65 | 0.007 | (0.015) | | -0.010 | (0.013) | |
| SS wealth for entry of spouse to SSDI at survey date | -0.001 | (0.004) | | 0.003 | (0.003) | |
| PDV of spouse's lifetime earnings at 65 | -0.001 | (0.001) | | -0.001 | (0.001) | |
| Employer retiree health insurance indicator | -0.049 | (0.008) | *** | -0.053 | (0.008) | *** |
| Retiree health insurance missing | -0.030 | (0.009) | *** | 0.003 | (0.012) | |
| Employer-provided health insurance indicator | 0.008 | (0.007) | | 0.066 | (0.011) | *** |
| Employer health insurance missing | 0.011 | (0.018) | | 0.016 | (0.019) | |
| Employer-provided health insurance indicator for spouse | -0.003 | (0.006) | | -0.009 | (0.006) | |
| Employer health insurance missing for spouse | 0.003 | (0.016) | | -0.024 | (0.018) | |
| Education = High School | 0.007 | (0.007) | | 0.013 | (0.007) | * |
| Education = Some College | 0.020 | (0.008) | *** | 0.016 | (0.008) | ** |
| Education = College Graduate | 0.039 | (0.008) | *** | 0.023 | (0.009) | *** |
| Hispanic | 0.019 | (0.009) | ** | -0.001 | (0.009) | |
| Black | -0.003 | (0.008) | | 0.011 | (0.008) | |
| Other race | 0.012 | (0.011) | | -0.007 | (0.011) | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Parent died since last interview | -0.011 | (0.009) | | -0.006 | (0.009) | |
| Health good | -0.015 | (0.005) | *** | -0.013 | (0.005) | *** |
| Health fair poor | -0.099 | (0.007) | *** | -0.084 | (0.007) | *** |
| Health improved since last interview | -0.015 | (0.008) | ** | -0.022 | (0.007) | *** |
| Health worsened since last interview | -0.045 | (0.006) | *** | -0.035 | (0.006) | *** |
| Self employed | 0.049 | (0.008) | *** | -0.010 | (0.011) | |
| Self Employed, spouse | 0.003 | (0.008) | | 0.013 | (0.008) | * |
| Age | -0.006 | (0.001) | *** | -0.008 | (0.001) | *** |
| Age = 62-64 | -0.089 | (0.009) | *** | -0.059 | (0.008) | *** |
| Age = 65+ | -0.099 | (0.014) | *** | -0.025 | (0.016) | |
| Probability live to age 75 | 0.000 | (0.000) | | 0.000 | (0.000) | |
| Probability live to age 75, Spouse | 0.000 | (0.000) | | 0.000 | (0.000) | |
| R squared (sample size) | 0.328 | (21,480) | | 0.326 | (21,480) | |

Notes: The specification also includes Census Region dummies and survey year dummies. All dollar amounts are in units of $100,000 (deflated to 2005). See Tables 2 and 3 for additional notes.