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THE WEALTH EFFECTS OF INCOME INSURANCE

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Precautionary savings models suggest that wealth should rise with income risk. Risk is reduced by means-tested transfers, however, which implies that transfer programs should discourage private wealth accumulation. We offer a comprehensive empirical assessment based on variation across states in the generosity of a number of programs, specifically unemployment insurance and means-tested transfers (Aid to Families with Dependent Children and Food Stamps). We use monthly data on married couples from the Survey of Income and Program Participation (SIPP) to regress wealth on income, income risk, and various measures of transfer generosity. The results support the precautionary savings model and reveal moderate negative wealth effects of both unemployment insurance and means-tested transfers, with an elasticity of about −0.18.

I. INTRODUCTION

Research on savings behavior has highlighted the importance of precautionary motives for individual saving. Simulation methods have suggested that a significant fraction of total wealth stocks may derive from the precautionary motive (Caballero, 1991), and there is substantial empirical support for this view (Carroll, 1992; Carroll and Samwick, 1997, 1998). One policy consequence is that social programs may have a savings disincentive effect, which would operate on two levels. First, many social programs have an asset test, and this would directly discourage asset accumulation among those at or near eligibility for the program (for evidence, see Hubbard, Skinner, and Zeldes, 1995). Second, independent of any asset test, social programs act as a form of income insurance (Bird, 1995), which may partly satisfy the precautionary savings motive. Thus, the mere existence of social programs may discourage savings among the population as a whole. A growing body of empirical evidence seems to suggest a savings disincentive effect of specific programs (unemployment insurance: Engen and Gruber, 1995; aid to unwed mothers: Powers, 1998; means-tested elderly assistance: Neumark and Powers, 1996). Ziliak (1998) uses the Panel Study of Income Dynamics to show that a combined package of several U.S. social programs seems to discourage savings across the income distribution.[1]

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[1]Ziliak (1998) does not examine the effects of workers compensation programs, nor do we. They are another potential source of insurance, but the state-level variations in the law are so extensive that quantifying them would have taken the paper too far afield. The insurance effects of workers compensation would occupy an entire separate study.

In our study, we use the Survey of Income and Program Participation (SIPP) to examine the savings effects of Aid to Families with Dependent Children (AFDC), Food Stamps, and Unemployment insurance. Our study extends the literature in several ways. First, the SIPP database is a monthly income panel, whereas most papers in the literature focus on annual data. Second, we focus on a comprehensive package of social programs as opposed to one program, an approach still in a relative minority in the literature. Third, we focus our attention on the insurance effects of programs as opposed to the asset test effects. Practically speaking, this means we use expected benefits, rather than actual benefits, as regressors in our wealth regressions.[2]

SIPP collects monthly data on individuals in panels that cover periods of about 2.5 years. SIPPs panel nature allows us to estimate the income process, especially the degree of income risk. Moreover, an extensive wealth survey was conducted at the end of the 1984 panel, which allows the following research design: use the 24-month panel before the wealth survey to estimate permanent income and income risk, then do a cross-section regression of end-of-panel individual wealth on (a) the panel-based permanent income and income risk, and (b) end-of-panel measures of expected social program benefits. Our regressions on these data generally support the findings of the preceding literature: the savings of respondents in the SIPP data react negatively to the generosity of social programs.

The paper is organized as follows. To fix ideas and motivate our empirical model, Section II presents a simple model of precautionary saving. In Section III we discuss the data and our empirical specification. Section IV presents our results. Section V summarizes the main findings.

II. Theoretical Framework

In this section we discuss wealth choices in the face of risk using simulations of a simple two-period model.[3] An agent has first-period income y_1, which is certain. Second-period income will be zero with probability $p, w > 0$ with probability $1 - p$. The agent can save an amount s in the first period; for simplicity interest and discount rates are zero. First-period consumption is $y_1 - s$; second-period consumption is either s or $w + s$. Consumption is evaluated by the utility function $u(c_t) = (1 - \varepsilon)^{-1} c_t^{1-\varepsilon}$, $t = 1, 2$, which exhibits constant

[2]For expected benefits, we estimate the probability that a family will be poor and multiply this by the maximum possible benefits available in that family's state of residence. This approach fully captures the insurance effect of social programs. It identifies families by their economic risk, and then measures the insuring effect of social policy as the overall generosity of the social programs for which the family might become eligible. The alternative is to regress wealth on the **actual** amount of benefits currently received. Such an approach does not directly address the insurance effect of social programs; it does not allow for the fact that some families who receive no benefits at all may still have a savings disincentive because they know the state will protect them **if** their income falls enough so that they **do** receive benefits.

[3]Kimball (1990) shows that the basic theory of precautionary saving is isomorphic to the standard theory of Arrow–Pratt risk aversion.

relative risk aversion with coefficient ε.[4] With E being the expectation operator conditional on information available in period 1, total utility is $u(c_1)+E(u(c_2))$.

First consider an unemployment insurance program that replaces a fraction r of the "normal" second-period income w in the event that income falls to zero. In that case total utility is $u(y_1-s)+pu(rw+s)+(1-p)u(w+s)$. Optimizing over choice of s, the first-order condition is $-(y_1-s)^{-\varepsilon}+p(rw+s)^{-\varepsilon}+(1-p)\times(w+s)^{-\varepsilon}=0$. Suppose we have the following parameter set: $y_1=\$20{,}000$, $w=\$20{,}000$, $p=0.05$, $r=0.4$, $\varepsilon=3.0$. In that case, a simple optimization solution algorithm reveals that the optimal savings level is $s^*=\$1{,}430$. The uncertainty in second-period income causes the agent to save 7.15 percent of first-period income for a "rainy day."

Next consider a means-tested assistance program that provides, rather than income replacement, a minimum income guarantee g. In this case total utility becomes $u(y_1-s)+pu(s+g)+(1-p)u(w+s)$. Here the first-order condition is $-(y_1-s)^{-\varepsilon}+p(g+s)^{-\varepsilon}+(1-p)(w+s)^{-\varepsilon}=0$. Using the above parameter values and an income guarantee $g=\$6{,}000$, optimal savings is $s^*=\$2{,}200$, 11 percent of first-period income.

The following table provides optimal savings amounts for different values of the parameters:

SIMULATED OPTIMAL SAVINGS

Cell Entries are the Optimal Values of s^*		First-Period Income (y_1) and "Normal" Second-Period Income (w)	
Social policy parameters	Values	$y_1=w=\$10{,}000$	$y_1=w=\$20{,}000$
Replacement rate (r)	$r=0.2$	\$1,600	\$3,190
	$r=0.4$	\$710	\$1,430
Income Guarantee (g)	$g=4{,}000$	\$710	\$3,190
	$g=6{,}000$	\$260	\$2,200

The table shows that savings rise as the level of normal incomes rises and fall as the generosity of social programs increases. Further investigation reveals that savings respond positively to increases in the probability of income loss (p). Also, at a consumption floor of \$4,000, the elasticity of savings with respect to changes in the consumption floor is -0.66 when normal income is \$20,000, -1.2 when normal income is \$10,000. At a replacement rate of 0.4, the elasticity of savings with respect to changes in the replacement rate is -1.68 when normal income is \$20,000, -0.3 when normal income is \$10,000. In other words, the consumption floor approach has a greater impact on savings among those with lower incomes; the replacement-of-income approach has a greater income among those with higher incomes.

[4]The constant relative risk aversion (CRRA) function has two useful features. First it implies that richer individuals are more willing to gamble fixed amounts of income, which fits well with intuition. Second, it implies that a fixed loss will lower utility by more than a gain in the same amount will raise utility; aversion to loss exceeds the desire for gain. This also fits with intuition. Most simulations in the literature rely on CRRA utility (e.g. Carroll, 1992; Engen and Gruber, 1995).

The model thus shows that wealth levels should be correlated both with incomes and income risk, as the precautionary savings literature suggests, but also with measures of the generosity of social policies. A completely accurate model would have to include additional parameters for time preferences as well as life-cycle motives, but such a model would quickly become too complex to be solved simply. Our empirical specification will include controls for all of these factors.

Finally, it is important to notice that the savings disincentive effects of social programs in this model occur (1) in the absence of any explicit asset test for eligibility, and (2) independent of program participation. In other words, this model focuses attention on the effect of social program generosity when there is only some *potential* that it will be needed. These are the effects we will look for in the data.[5]

III. Data and Empirical Strategy

A. *Data*

The data are drawn from the 1984 panel of the Survey of Income and Program Participation (SIPP). We select a sample of working-age (18–60) couples who were continually married during the entire 24-month panel period, and who had no imputed asset data. Furthermore, since unemployment insurance does not replace lost self-employment income we exclude households with self-employment income. This results in a sample of 3,262 couples, of whom 3,198 (98 percent) have positive net wealth. Couples who meet the sample requirements account for about 45 percent of all U.S. households.

The SIPP is a short-period panel in a comparatively small window (two years), and as such it offers a unique perspective on the questions raised by the precautionary savings literature. Do the effects found in the studies based on annual data appear in monthly data as well? At the same time, using monthly data raises new problems which need to be addressed.

First, consider the benefits of using monthly data. Monthly data capture more of the volatility often observed in the economy. Studies of poverty and welfare caseloads show that long continuous spells of low income are quite rare. The far more common experience, especially among the married couples in our sample, is to see income drop for a month or two and then rebound (Ruggles and Williams, 1989).

SIPP also has an accuracy advantage, since the time of recall is short. Individuals are interviewed every four months, and validation studies suggest that SIPP's wealth data are accurate (McNeil and Lamas, 1989; Curtin, Juster, and Morgan, 1989). SIPP offers more wealth detail than the Michigan Panel Study of

[5]Actually, expected transfers will have two distinct effects. As a component of expected income, they will have an income effect, but as insurance, they will have a variance-reduction effect. There is probably no way to identify these effects separately.

Income Dynamics (PSID), the only other wealth panel.[6] SIPPs individual design also reduces errors in the construction of household aggregates.

Now consider the costs of using monthly data. Foremost is the fact that we are forced to estimate permanent income and income risk using only a two-year window of income data. In the end every measure of permanent income is only "permanent" in the context of the data window in which it is observed. Studies based on annual data can estimate a more "permanent" version of permanent income, and one more accurate as an estimate of the expected income in any period of life. Seeking to capture short-term income variation is purchased at the cost of losing accuracy in the estimates of the lifetime income process. Although the survey design is beyond our control, we can make adjustments to reduce the effect of errors in measuring the income variables, by instrumenting them. Therefore, following the practice of Carroll and Samwick (1997) we instrument for the income and risk variables with measures of education, 1-digit industry dummies, and 1-digit occupation dummies; we also instrument with a dummy for living in a metropolitan statistical area.[7]

B. *Empirical Specification*

Our principal interest is in the relationship between the generosity of social insurance benefits and the wealth holdings of couples. Using the theoretical model developed above as a guide, we will regress wealth on measures of each couple's permanent incomes, their income risk, the generosity of various social programs in the area they live, and a set of controls. Specifically, we run OLS regressions of the general form

$$\ln W = b_0 + b_1 \ln P + b_2 v + b_3 E(\text{BENEFITS}) + b_4 X + \varepsilon \tag{1}$$

where W is wealth, P is permanent income, v is a measure of income risk, E(BENEFITS) is a vector of measures of social program generosity, and X is a vector of controls.

Since Ziliak (1998) and Engen and Gruber (1995) find wealth effects vary by degree of wealth liquidity, we examine three measures of net worth: non-housing net worth, total net worth, and liquid wealth. Total net worth includes financial and business assets, debts, cash savings, housing equity, and the value of automobiles. Non-housing wealth removes the housing equity or debt, while liquid wealth includes only financial assets. We expect that precautionary motives will be strongest for the most liquid forms of wealth, since any "rainy day" savings would have to be available in emergencies.

We obtain measures of permanent income and income risk in several ways to make sure our results are robust to different definitions of risk. Our base

[6]The Survey of Consumer Finances can be treated as a panel, since the 1989 sample re-interviews some members of the 1983 sample. (There is also a phone re-interview in 1986, but it is not extensive and the data are not considered as reliable as the 1983 and 1989 data.) With only two observations on income, however, any estimates of permanent income and income risk would be too inaccurate.

[7]Furthermore, Skinner (1988, p. 250) points out that individuals facing high risks may not be inclined to save more, simply because they happen to be less risk-averse than others. If the level of income uncertainty an individual faces is indeed endogenous, that would be another reason to instrument the variable.

specification uses panel data on log monthly income to estimate a random-effects income regression, using the fitted value as permanent income and the individual-specific error variance as income risk (see Table A.2 in the appendix). This follows closely the method used recently by Kazarosian (1997) and introduced by King and Dicks-Mireaux (1982). We also used simpler methods, such as using the (detrended) average of log income as permanent income and the variance of income around this as income risk. This is similar to the methods employed by Carroll and Samwick (1998) and Engen and Gruber (1995). A third variation measures income risk by the Equivalent Precautionary Premium, defined by Kimball (1990) as the intensity of the precautionary saving motive at the point of zero precautionary savings.[8] In order to account for errors in the measurement of our income variables, we instrument all these measures using education, industry, occupation, and SMSA variables.[9]

The variables of most interest have to do with the generosity of social programs. At the simplest level one could measure this by including actual receipts from different programs; all else equal, a couple that receives more from a given program must be living in a jurisdiction in which social programs are more generous. However, this approach ignores the fact that the savings effects we are looking for derive not from actual transfers but from potential transfers: a couple will save less if it knows that its transfers will be higher *if* it becomes poor; whether or not it actually becomes poor and receives the transfers does not affect the power of the *ex ante* saving incentive. To measure the strength of this incentive we need two pieces of information: the couple's *ex ante* probability of future poverty at any given point in time, and the amount of benefits that a couple living in the same jurisdiction would receive if it became poor. Multiplied by each other, these measures produce the level of expected benefits from social programs in the jurisdiction. We take expected benefits as the basic measure of social program savings disincentives.[10]

To operationalize the expected welfare benefits measure, we use probits to estimate a probability of poverty in a given month for each couple, as a function of exogenous (in the short run) characteristics such as age, race, education, family size, region, and income variance (these results may be found in Table A.3 in the

[8]Actually we use the Carroll and Samwick version of the premium, which they call the Relative Equivalent Precautionary Premium. The formula is

$$\mathrm{REPP} = 1 - \left(\frac{1}{T}\sum_{t=1}^{T}\left(\frac{y_{it}}{p_i}\right)^{-\varepsilon}\right)^{-1/\varepsilon}.$$

We evaluate this expression using $\varepsilon = 3.0$.

[9]In varying our methods, we have found that our results are not affected by the specification of permanent income and income risk, but that they are strengthened by instrumenting income.

[10]Engen and Gruber (1995) use the replacement rate of unemployment insurance. Although this variable is endogenous with respect to the income component of their dependent variable, they are able to adapt some controls to the regression that reduce the problem. In any case our results for UI are similar to theirs, indicating that the precise method of measuring UI generosity is not central. We chose our method because the concept of "replacement rate" formulation does not really apply to consumption-floor type programs like AFDC and Food Stamps, whereas the "expected benefit" formulation can be applied to all program types and thus allows complete comparability of the coefficients across programs.

appendix). We multiply this probability by the maximum available monthly benefit from means-tested programs (AFDC and Food Stamps) in the couple's state of residence; we call this variable EXPECTED TRANSFERS. For the expected UI benefit we estimate the probability of unemployment for the husband and wife separately. We then multiply the predicted probability of unemployment times the maximum weekly state unemployment insurance benefit to obtain EXPECTED UI.[11] Together these variables give us rough but exogenous variation in the generosity of the two program types, as perceived by married couples living in different states.[12]

In measuring EXPECTED TRANSFERS we focus on the two most important sources of means-tested transfer income for working-age families: Food Stamps (FS) and Aid to Families with Dependent Children (AFDC). AFDC can be thought of as divorce insurance for married mothers; in areas maintaining the unemployed-parent program, AFDC-UP provides insurance for couples against the joint event {children, low income, unemployment}.[13] FS provides insurance against low food consumption for any reason. Both of these transfer measures vary by geographic location. Each state independently sets its AFDC benefit levels. FS benefits are administered through a uniform national formula that allows variation by household characteristics (such as utility expenses), which will also vary by state.[14] Previous research shows that this variation is substantial (Hagstrom, 1991, 1996; Fraker and Moffitt, 1988; Ohls and Beebout, 1993).

We obtain information about maximum state benefit levels in the sample period (1984–86) from the appropriate editions of the Green Book (Committee on Ways and Means, various years). In constructing the variables we assign to

[11]The ideal measure here would be some estimate of the exogenous probability of eligibility at various levels for each couple, multiplied by the benefits available at each level. Such an ideal measure is impossible to construct, however. In the U.S., eligibility rules vary significantly across states, and the income available under different eligibility conditions also varies considerably. To construct a practical measure, we conceptually divide the precautionary savings effects of a transfer program into two parts. First, precautionary savings effects are perceived more keenly by couples who face high probabilities of becoming eligible for programs; therefore a practical measure should include some estimate of a couple's probability of becoming poor. Second, precautionary savings effects are stronger for couples in areas where transfer benefits are more generous. Therefore a practical measure should include an estimate of the overall generosity of the transfer system in the locality where a couple lives. As a rough estimate of the latter we take the maximum available in the various programs.

[12]For future research it might be worth pursuing the data that would allow us to study other means-tested programs, such as Supplemental Security Income, general assistance, and local relief. Unfortunately the data are either unavailable in SIPP, or in the case of SSI would involve a different sample.

[13]Peters (1993) finds that expected short-term financial possibilities are more strongly related to the probability of divorce that are longer-term economic consequences of divorce. While a complete treatment of the risk of future marital dissolution is beyond the scope of this paper, we do limit our sample to couples who stay married thorough the sample period to minimize the effect of recent changes in marital status.

[14]Specifically, the variation has three sources: (1) the earnings disregard, which treats non-labor income differently from labor income, (2) the size of the shelter deduction, and (3) the waiver of the shelter deduction cap for households with elderly or disabled members. If, for example, a middle-class household in Alabama expects to face lower housing costs if poor than a similar household in Wisconsin, the Food Stamp formula is less generous *ex ante* for the Alabama household. It deducts fewer expenses and offers a lower grant amount for potentially poor Alabamans, and deducts more expenses and offers a higher grant amount for potentially poor Wisconsinites. Therefore Food Stamps provide less food insurance in Alabama than in Wisconsin.

each household the maximum benefit available in the state to households of the same size.

The remaining variables in the regression control for heterogeneity in preferences and discount rates. The wealth-age profile (AGE and AGE SQUARED) accounts for life-cycle motives. Family structure variables reveal aspects of the household's potential labor resources as well as investments in children. Other variables include ethnicity, region of residence, and state-level controls.[15] Table A1 in the appendix reports descriptive statistics for all the variables.

IV. Results

Table 1 presents a base-case regression of log non-housing net wealth against the variables of interest and the set of controls. Most of the variables exhibit statistical significance at the 5 percent level. Overall the results support precautionary savings theory, confirming that wealth rises with both permanent income and income risk. The coefficient on income risk indicates an elasticity of 0.493, slightly smaller than those implied by Carroll and Samwick's (1998) results for annual data.

The coefficient on EXPECTED TRANSFERS is negative and statistically significant, indicating that means-tested transfers do have a measurable effect on net worth. The coefficient on EXPECTED UI is negative and about the same substantive magnitude as EXPECTED TRANSFERS, also statistically significant. The two coefficients indicate that a ten percent increase in expected income support benefits, from either type of program, will reduce net worth holdings by about 1.8 percent. These effects seem substantively important and in the middle of the range of comparable previous estimates.[16]

The other variables show sensible patterns for the most part. Wealth accumulation is somewhat lower among non-whites, and families that have more adults working have less wealth (holding income constant). One can see the latter as a precautionary savings effect: a household with two workers at $2,000 each has much less probability of a zero-income month than a household with one worker at $4,000. Therefore households with more working members have less motive to save. Next, wealth rises with the number of children; with more mouths to feed, precautionary savings much be larger. Holding the number of kids constant, however, increasing family size lowers the need for precautionary savings, since many of the extra individuals—siblings, aunts and uncles, cousins, grandparents—may be potential sources of income in time of need. In other words, the family itself is a form of precautionary income insurance. Finally, the life-cycle variables (age and its square) do not have the signs predicted by the life-cycle model. This may only indicate that most life-cycle saving by working-age couples is done through

[15]We explored a number of different controls for state-level effects, finding that these generally had no impact on the results. Therefore we report results only with state per capita income.

[16]From the figures given on page 24 of Engen and Gruber (1995), one can infer that the wealth elasticity of UI using the replacement rate method is -0.075, roughly one-half the size of our elasticity using the expected benefit method. Ziliak (1998) reports log–log figures with a wide range of elasticities. The most negative elasticity (which is not strictly comparable to ours due to differences in variable definition) is -0.386 (Table 4).

TABLE 1

BASE WEALTH REGRESSION: OLS, DEPENDENT VARIABLE IS LOG NET WEALTH, EXCLUDING HOUSING WEALTH

Independent Variables:	Coefficient	Standard Error
Log (Permanent income)	1.207	0.050*
Log (Income risk)	0.493	0.059*
Log (Expected transfers)	−0.172	0.044*
Log (Expected UI)	−0.185	0.071*
Age	−0.034	0.016*
Age squared (× 1,000)	0.760	0.200*
Family size	−0.182	0.027*
Working adults, number in HH	−0.216	0.041*
Children aged less than 18	0.197	0.034*
Head is non-white	−0.111	0.074
Couple lives in northeast	−0.023	0.069
Couple lives in midwest	0.116	0.058*
Couple lives in west	0.103	0.071
State per capita income, $000	0.049	0.023*
Constant	0.882	0.514

Source: SIPP.

Notes: $R^2 = 0.406$. Observations = 3,143. Asterisks indicate statistical significance at the 5 percent level, two-tailed test. For regions, the omitted category is South. The sample includes all working-age continuously-married SIPP couples during a 24-month period in the mid-1980s, except respondents who have imputed asset data or are self-employed.

TABLE 2

NET WEALTH REGRESSIONS: TYPES OF WEALTH BY LIQUIDITY (Standard errors listed under the coefficients)

Dependent Variable	(1) Log Non-Housing Wealth (base case)	(2) Log Net Wealth	(3) Log Liquid Wealth
Log (Permanent income)	1.207	1.260	1.712
	0.050*	0.054*	0.088*
Log (Income risk)	0.493	0.358	0.648
	0.059*	0.065*	0.099*
Log (Expected transfers)	−0.172	0.012	−0.251
	0.044*	0.048	0.073*
Log(Expected UI)	−0.185	−0.339	−0.242
	0.071*	0.078*	0.120*
R^2	0.406	0.407	0.319
Observations	3,143	3,152	2,788

Source: SIPP.

Notes: OLS. Asterisks indicate statistical significance at the 5 percent level, two-tailed test. Controls include all the variables listed in Table 1: age, age squared, family size, children, race, and region.

housing equity. The coefficients on state income and the regional variables are not generally significant.

Table 2 presents variations on the base-case regression that test for sensitivity of the results to wealth liquidity. As expected, wealth liquidity has some impact on the nature of responses to social policies. Regression 1 repeats the key coefficients from the base-case regression. Regression 2 uses total net wealth, including housing equity, as the dependent variable. The effect of EXPECTED TRANSFERS reverses sign but becomes statistically insignificant at the five percent level. The effect of EXPECTED UI remains negative, but becomes larger in absolute value. This finding suggests that UI is more likely to affect the savings decisions of households with substantial housing wealth. Regression 3 uses liquid wealth (i.e. only financial assets) as the dependent variable. Here, both the transfers and UI coefficients are negative and substantively larger than in the base case. The regression results appear to be reasonably robust; depending on the form of wealth, the coefficients seem largely stable and generally negative. The hypothesis that income support programs depress savings receives fairly broad support.

Table 3 considers variations in the definitions of permanent income and income risk, and in the sample definition. Regression 1 shows coefficients from

TABLE 3

NET WEALTH REGRESSIONS: PERMANENT INCOME, INCOME RISK, AND SAMPE

(Standard errors listed under the coefficients)

Sample	(1) All Couples	(2) All Couples	(3) All Couples	(4) Non-Recipient Couples Only
Dependent Variable	Log Non-Housing Wealth	Log Non-Housing Wealth	Log Non-Housing Wealth	Log Non-Housing Wealth
Log (Permanent income) as random-effects fitted value	1.207 0.050*	—	1.156 0.050*	1.241 0.058*
Log (Permanent income) as simple average	—1.176 0.049*	—	—	
Log (Income risk) as random-effects error variance	0.493 0.059*	—	—	0.561 0.063*
Log (Income risk) as simple variance	—	0.509 0.059*	—	—
Log (Income risk) as relative precautionary premium	—	—	2.606 0.504*	—
Log (Expected transfers)	−0.172 0.044*	−0.185 0.044*	−0.144 0.044*	−0.158 0.045*
Log (Expected UI)	−0.185 0.071*	−0.183 0.071*	−0.208 0.071*	−0.202 0.074
R^2	0.406	0.409	0.398	0.369
Observations	3,143	3,143	3,143	2,895

Source: SIPP.
Notes: See Table 2.

the base case. Regression 2 uses simple (detrended) average income as permanent income and the simple variance of income as income risk, and there is virtually no effect. Regression 3 uses the Equivalent Precautionary Premium as the measure of income risk, which lowers the effect of welfare but raises that of UI (the coefficient on risk changes because the variable is of different scale, see Table A1). Regression 4 removes from the sample all couples who received benefits from any of the programs during the sample period; this seems to have no effect. Overall, the basic result that savings fall when expected social program benefits are large seems to be robust to most definitional and sample changes.

V. Conclusions

We find evidence that income support programs act as a replacement for precautionary savings, and that they have a greater effect on more liquid forms of wealth. For non-housing wealth, both means-tested and unemployment insurance benefits have negative effects on savings, with an elasticity of about -0.18. Reviewing all of our regressions, the elasticities of these programs' effects tend to range between 0.01 and -0.34. All the coefficients were negative except one, and it was not statistically significant. Overall, these findings contribute to a growing body of evidence that the income-insuring effects of social policies reduce private savings.

Appendix

TABLE A1
Variable Descriptions

Variable Names and Descriptions	Mean	Standard Deviation
Household net worth	49,327	61,653
Net worth minus housing equity or liability	19,575	39,816
Liquid wealth	8,112	21,991
Log Permanent income—random effects method	7.754	0.572
Log Permanent income—simple average method	7.731	0.585
Log Income risk—random effects method	0.123	0.359
Log Income risk—simple variance method	0.122	0.362
Log Income risk—Relative precautionary premium (based on CRRA utility with parameter 3.0)	0.009	0.052
Expected transfers—log of (maximum combined benefit multiplied by the couple's estimated probability of being poor in any given month)	3.129	1.340
Expected UI—log of (maximum unemployment insurance benefit time likelihood of unemployment)	3.015	0.690
AGE—head's age at start of panel	39.790	10.832
AGESQ—AGE squared	1700.546	890.973
WORKING—Members of the couple who work full time (0, 1, or 2)	1.601	0.555
KIDSLT18—number of people younger than 18	1.232	1.229
FSIZE7—total family size	3.651	1.348

TABLE A1—continued

Variable Names and Descriptions	Mean	Standard Deviation
NOHISCHL—head has no high school degree	0.238	—
HISCHL—head has only high school degree	0.401	—
SOMECOLG—head has some college, no degree	0.171	—
COLLEGE—head has college degree	0.190	—
NONWHITE—head non-white	0.105	—
SMSA—head lives in an SMSA	0.491	—
NEAST—couple lives in the northeast	0.228	—
MIDWEST—couple lives in the midwest	0.268	—
SOUTH—couple lives in the south	0.325	—
WEST—couple lives in the west	0.179	—
PCINC80—per capita income in state of residence (000)	9.827	1.170
TAXRAT85—per capita tax revenues in state of residence	176.412	23.032

Source: Survey of Income and Program Participation. Base number of observations: 3,262.

TABLE A2

Random Effects Model on the Natural Log of Family Income

	Coefficient	Standard Error
Husband Variables		
NOHISCHL	0.8112284*	0.3601972
HISCHL	0.577022**	0.3063509
SOMECOLG	0.184837	0.3333337
AGE * NOHISCHL	−0.0005728	0.0010971
AGE * HISCHL	0.0012114	0.0008863
AGE * SOMECOLG	0.0028945*	0.0011091
AGE * COLLEGE	0.0039578*	0.0012035
AGESQ * NOHISCHL	1.13e−06	1.05e−06
AGESQ * HISCHL	−4.68e−07	8.71e−07
AGESQ * SOMECOLG	−1.81e−06**	1.10e−06
AGESQ * COLLEGE	−2.41e−06*	1.17e−06
Wife Variables		
NOHISCHL—wife	−0.6456802**	−0.3742769
HISCHL—wife	0.0028011	0.3254175
SOMECOLG—wife	−0.4780611	0.3396434
AGE * NOHISCHL—wife	0.0068058*	0.0010409
AGE * HISCHL—wife	0.0054733*	0.0008143
AGE * SOMECOLG—wife	0.0080015*	0.0010708
AGE * COLLEGE—wife	0.0058296	0.0013804
AGESQ * NOHISCHL	−6.05e−06*	1.05e−06
AGESQ * HISCHL	−4.98e−06*	8.31e−07
AGESQ * SOMECOLG	−7.66e−06*	1.11e−06
AGESQ * COLLEGE	−4.91e−06*	1.41e−06
Intercept	5.313163*	0.2834407

sd (*u*_husbid	= 0.5512443	Number of obs = 138065
sd (*e*_husbid_*t*)	= 0.4433995	*n* = 5,596
sd(*e*_husbid_*t* + *u*_husbid)	= 0.7074415	*T*-bar = 24.4935
corr(*u*_husbid, X)	= 0 (assumed)	*R*-sq within = 0.0076
		between = 0.2398
		overall = 0.1659

.... theta ....

min	5%	median	95%	max	
0.6269	0.8310	0.8412	0.8412	0.9198	$\chi^2(22) = 2498.29$; Prob $> \chi^2 = 0.0000$

Source: SIPP.

TABLE A3

AUXILIARY PROBIT REGRESSIONS: FAMILY INCOME BELOW POVERTY LINE AND UI ELIGIBILITY

Independent Variables	Dependent Variable					
	Below Poverty Line		Unemployment Insurance Eligibility			
	All Households		Husband		Wife	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
v_eit_ln	0.4912*	(0.0359)	0.1517	(0.0421)	0.2206	(0.0602)
age—husband	−0.0576*	(0.0224)	−0.0873	(0.0259)		
agesq—husband	0.0006*	(0.0003)	0.0001	(0.0003)		
age_wif					−0.0122	(0.0278)
agesqw					−0.00004	(0.0004)
non-white	0.5663*	(0.0838)	0.4656	(0.1059)	0.3309	(0.1150)
educ_hus (years)	−0.0477*	(0.0109)	−0.0849	(0.0114)		
disabled—husband	0.5619*	(0.0754)	0.4438	(0.1152)		
educ_wif	−0.1056*	(0.0128)			−0.1061	(0.0152)
dis_wif	0.2473*	(0.0765)			0.4853	(0.1548)
self-employed—husband	0.3010*	(0.0771)				
self-employed—wife	−0.1767	(0.1168)				
family size	−0.0104	(0.0360)				
Number of Kids less than 18	0.02234*	(0.0418)				
SMSA	−0.1822*	(0.0622)	0.0406	(0.0756)	0.0332	(0.0812)
Northeast	−0.1125	(0.0902)	0.1112	(0.1075)	−0.0106	(0.1171)
Midwest	0.0945	(0.0727)	0.1727	(0.0950)	0.1397	(0.0988)
West	0.0880	(0.0841)	0.2259	(0.1047)	0.1353	(0.1146)
Intercept	1.1076*	(0.4293)	0.9849	(0.5073)	0.1539	(0.5240)
Observations	5531		3878		2676	
Chi-squared	763.96		134.11		97.89	
(degrees of freedom)	(16)		(10)		(10)	
Log Likelihood	−1148.93		−708.96		−626.37	
Pseudo R-squared	0.250		0.086		0.073	

Source: Authors' calculations from SIPP.

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