A Simple Test of Consumption Insurance

John H. Cochrane


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A Simple Test of Consumption Insurance

John H. Cochrane

University of Chicago

Are consumers effectively insured against idiosyncratic shocks to income or wealth, either by formal institutions such as charities, private insurance, and government programs or by informal mechanisms such as gifts and "loans" from relatives, friends, and neighbors? Under full insurance, consumption growth should be cross-sectionally independent of idiosyncratic variables that are exogenous to consumers. This proposition is tested by cross-sectional regressions of consumption growth on a variety of exogenous variables. Full insurance is rejected for long illness and involuntary job loss, but not for spells of unemployment, loss of work due to strike, and an involuntary move.

I. Introduction and Motivation

If markets are complete or if there is some other mechanism or set of institutions that implement a full-information Pareto-optimal allocation, then an individual's consumption should not respond to idiosyncratic income or wealth shocks. This proposition can be viewed as a cross-sectional counterpart to the permanent income hypothesis: full insurance implies that consumption should not vary across individuals in response to idiosyncratic shocks, just as constant borrowing and lending opportunities imply that consumption should not vary.

This paper stemmed from a conversation I had with Barbara Mace in December 1986 regarding her related paper (Mace 1988, this issue), which I acknowledge gratefully. I thank Andrew Atkeson, Robert Townsend, and anonymous referees for many helpful suggestions and Paul Raca for outstanding research assistance. This research was partially supported by National Science Foundation grant SES-88-09912. I completed a revision while a visiting scholar at the Hoover Institution. An early draft of this paper appeared as National Bureau of Economic Research Working Paper no. 2642.

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over time in response to forecastable shocks. This paper presents a simple test of consumption insurance motivated by this analogy: as Hall (1978) regressed consumption growth over time on a variety of potential forecasting variables, this paper presents regressions of consumption growth across individuals on a variety of idiosyncratic variables.

The notion that individuals might be well insured and hence able to smooth consumption over states of nature meets a great deal of initial resistance, just as the notion that individuals might smooth consumption over time did when Modigliani and Brumberg (1954) and Friedman (1957) advanced the permanent income hypothesis and life cycle model. Much of this resistance comes from the same two sources. First, everyday experience seems to deny the existence of formal markets in which consumers can insure against idiosyncratic income shocks, as it seems to deny the existence of formal markets in which consumers can borrow for life cycle smoothing. Second, everyday experience seems to affirm that individual consumption does respond to individual shocks, as it seems to affirm that large fractions of temporary income shocks are typically consumed.

But competitive markets in explicit contingent claims are not necessary to implement consumption-insured allocations since a wide variety of formal and informal institutions can also provide consumption insurance. Examples include unemployment, disability, and medical insurance; welfare and other government social programs; the array of state-contingent government transfers, including farm support, drought relief, payments to workers displaced by foreign competition, “disaster area” designation following a flood or earthquake, and so forth; local or national charities; implicit insurance on the part of employers such as labor hoarding or a willingness to continue employment of workers who become unproductive; and informal insurance mechanisms including gifts and “loans” from relatives, friends, and neighbors. Some specifications of intergenerational altruism can implement consumption-insured allocations within a family (Abel and Kotlikoff 1988; Altonji, Hayashi, and Kotlikoff 1989), and cross-sectional altruism may similarly help implement a consumption-insured allocation across families: many people are willing to help unrelated individuals or groups that are affected by an observable unfortunate outcome such as an earthquake, flood, disease, and so forth, help that is not forthcoming on the basis of the observation of differences in the levels of income alone.

As to the second criticism, consumption may well change among lottery winners or other extreme individuals who come to mind when one is evaluating the lessons of everyday experience. But this observation does not show that the kinds of shocks that are important to
economists and to individuals in the economy are not insured. If an individual loses his job, gets sick or disabled, works in an industry that suffers a loss in demand or a farm that suffers bad weather, and so forth, does he suffer a loss of wealth, revealed in his consumption choice? Or are shocks like these, shocks that are important and recurring real risks of everyday life, partially or completely insured?

Beyond direct interest in this question, the presence or strength of consumption insurance has methodological implications for macroeconomics and finance. Full insurance implies the existence of a representative consumer, that is, a social welfare function defined over aggregates that is independent of changes in the distribution of income or wealth over time,\(^1\) and most of contemporary macroeconomics and finance uses the representative consumer formulation. A rejection of consumption insurance suggests that the representative consumer formulation may be inappropriate, that is, that there may be important relations between distributions of income or consumption and the behavior of aggregates,\(^2\) and it motivates models with private information or other explicit impediments that generate imperfect insurance.

II. Specification

The consumption insurance tests are based on the proposition that with full consumption insurance, consumption growth should be cross-sectionally independent of idiosyncratic variables. Hence, \(\beta\) should equal zero in cross-sectional regressions of the form

\[
\log \left( \frac{c_{t+1}}{c_t} \right) = \alpha + \beta X_{t+1} + \epsilon_{t+1}, \quad j = 1, 2, 3, \ldots, N, \tag{1}
\]

where \(c_{t+1}/c_t\) is household \(j\)'s nondurable consumption growth, and \(X_{t+1}\) is an idiosyncratic shock variable. I also present nonparametric \(\chi^2\) tests for independence of \(X_{t+1}\) and consumption growth.

The regression is a cross-sectional analogy to Hall's (1978) regression of consumption growth on ex ante variables to test the permanent income hypothesis. However, it is only an analogy because the

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\(^1\) The form of the social welfare function may depend on the initial distribution of income or the initial assignment of Pareto weights. The Pareto weights do not change through time, so the social welfare function does not vary in response to changes in income or wealth.

\(^2\) The "may" is important. A representative consumer formulation can still exist without full insurance, given certain restrictive functional forms for utility and technology, e.g., quadratic utility and linear technology as in Hansen (1987). Thus a rejection of full insurance is suggestive but is not proof that a representative consumer formulation is inappropriate.
permanent income hypothesis and consumption insurance are distinct propositions, and each may hold independently of the other. Consumption insurance studies households’ ability to smooth consumption over states of nature; the permanent income hypothesis studies their ability to smooth consumption over time. They may be able to do each and not the other. Alternatively, consumption insurance states that Euler equation errors should be equal across households, whereas the permanent income hypothesis predicts that the expected Euler error is zero for each household. The Euler equation errors can be equal and not have mean zero (consumption insurance without permanent income), and vice versa.

In particular, “self-insurance” through accumulation of assets is an alternative to consumption insurance, not a mechanism for implementing it. A self-insured household must adjust consumption to idiosyncratic shocks, even if only to their permanent components.

The theory of risk sharing states quite generally that discounted marginal utility growth should be the same for all households. However, measured consumption growth can vary across households if their utility functions have different shapes (less risk-averse households will have more volatile consumption paths) or different rates of time preference, if consumption growth is measured with error, or if households experience preference shifts. (Preference shifts are likely to occur in household data. For example, household composition changes—a baby is born or a child leaves the household—and changes in preferences [appetites] with age of existing household members are obvious and important sources of preference shifts.)

These sources of heterogeneity show up in the error term of a regression such as (1). Therefore, right-hand variables should be uncorrelated with the measurement error in consumption growth, well measured themselves, and uncorrelated with variation in preferences across households. In particular, right-hand variables should be exogenous to the household and not decision variables. The same preference shifts that affect the household’s desired allocation of consumption over time and states of nature may well affect its desired allocation of leisure or other decision variables, so changes in decision variables are likely to be correlated with preference shifts.

Unfortunately, income does not satisfy these requirements, and I therefore do not emphasize it as a right-hand variable. First, despite the fact that regarding income as exogenous to the household has a long tradition in the permanent income literature, much of income is labor income and, thus, is a decision variable likely to be correlated with preference shifts. Second, reported income includes some of the payments that implement a consumption-insured allocation, such as transfer payments and wages (which contain insurance features), as
well as the random flow ("endowment") that the allocation insures. In this case, the theory predicts that reported income growth should be correlated with preference shifts. Third, measured income is likely to be correlated with the (substantial) measurement error in consumption. Households that over- (under-) estimate their income are likely to over- (under-) estimate their consumption as well, as part of a general error in perception of their standard of living.

A number of recent papers study consumption insurance with specifications related to the regression (1). Scheinkman (1984) first suggested empirical tests of consumption insurance. Mace (1988, this issue), who ran the first explicit test of consumption insurance, and Townsend (1989) run pooled time-series, cross-sectional versions of (1) with aggregate consumption growth and individual income on the right-hand side. Abel and Kotlikoff (1988) and Altonji et al. (1989) run similar regressions but interpret them as tests of intergenerational altruism. (In related empirical work, Altug and Miller [1990] test a parametric model of household behavior that includes consumption insurance, and Brennan and Solnik [1989] show how international capital flows help produce a consumption-insured allocation.)

In addition to the use of income as a right-hand variable, these papers all exploit functional forms for utility and restrictions on preference shocks in order to test the relation between individual and aggregate consumption and to pool cross-sectional regressions at different dates. Of course, some statistical or economic restrictions are necessary since the pure theory of risk sharing has no testable content. One can always construct preferences for which marginal utility growth is equal across households, given any set of observations on consumption growth (Scheinkman 1984). But the simple test turns out to be more robust as well since it can handle a wider range of preferences and shocks to preferences. Naturally, this robustness comes at a cost: a pooled time-series, cross-sectional regression has more power against its more restrictive null hypothesis.

III. Testable Implications of Consumption Insurance

Consider an endowment economy with \( N \) households, which have time-separable expected utility functions over a single consumption good. (These assumptions are generalized below.) Uncertainty is summarized by a state variable \( s^t \) that takes on countably many values at any date \( t \).

The Pareto-optimal consumption allocations are derived from the planning problem, maximize a weighted sum of individual house-
holds' utilities:

$$\max \sum_{j=1}^{N} \lambda^j \sum_{t=1}^{\infty} \sum_{s^t} (p^t)\pi(s^t)u(c^j(s^t), \delta^j(s^t)),$$  (2)

where $j$ indexes households, $t$ indexes time, $\lambda^j$ is household $j$'s Pareto weight, $p^t$ is its rate of time preference, $\pi(s^t)$ is the probability that state $s^t$ occurs, and $c^j(s^t)$ is household $j$'s consumption at date $t$ in state $s^t$. The preference shifts $\delta^j(s^t)$ can index arbitrary cross-sectional and intertemporal variation in preferences. The notation $\Sigma_{t'}$ indicates a sum over all possible states at each date.

The feasibility constraint is that aggregate consumption must be less than the aggregate endowment, at each date and in each state:

$$c^A(s^t) \equiv \sum_j c^j(s^t) \leq \sum_j e^j(s^t) \equiv e^A(s^t) \quad \text{for all } s^t,$$  (3)

where $e^j(s^t)$ is household $j$'s endowment at time $t$, and $e^A(s^t)$ is the total amount of the consumption good available at time $t$. The analysis is unchanged if there is a production function linking the total amount of consumption available at different dates or states: the planner can solve a plan with production by first determining the optimal distribution of aggregates across dates and states, and then determining how to distribute those aggregates, acting as though they were endowments.

If we take a derivative with respect to $c^j(s^t)$, the first-order conditions for the problem maximize (2) subject to (3) are

$$(p^t)\lambda^j u_c(c^j(s^t), \delta^j(s^t)) = \mu(s^t) \quad \text{for all } s^t,$$

where $\mu(s^t)$ is the Lagrange multiplier associated with the feasibility constraint (3), divided by the probability of state $s^t$, $\pi(s^t)$. From here on, I shall use the conventional notation for a random variable $c^j = c^j(s^t)$, $\delta^j = \delta^j(s^t)$, $\mu = \mu(s^t)$, and so forth. With this notation, the first-order conditions are

$$(p^t)\lambda u_c(c^j, \delta^j) = \mu.$$  (4)

The multiplier $\mu$ depends on aggregate consumption. It is constant across households $j$, so individual households' endowments do not enter into the determination of individual households' consumption allocations, given aggregate consumption and the Pareto weights.

However, household consumption and endowments may be correlated because endowments may be correlated with the Pareto weights $\lambda^j$. For example, in a competitive equilibrium, a household's Pareto weight $\lambda^j$ rises with the time 0 contingent claim value of its endowments, so households with typically high endowments will have high consumption allocations. But the Pareto weight $\lambda^j$ does not depend
on time since data at different dates represent the evolution over
time of a single plan. Therefore, observations at two or more dates
can be used to remove this fixed effect. In particular, we can get rid
of the \( \lambda^j \) by dividing the first-order conditions (4) for an individual
household at two points in time:

\[
\rho^j \left( \frac{u(t_{i+1}, \delta_{i+1})}{u(t_i, \delta_i)} \right) = \frac{\mu_{i+1}}{\mu_i}. \tag{5}
\]

The discounted growth of marginal utility is constant across house-
holds, and given aggregate consumption, individual households’ end-
dowments do not enter into the determination of discounted mar-
ginal utility growth.\(^3\)

To test consumption insurance via the independence of some idio-
syncratic variable \( X_{i+1}^j \) and measured consumption growth, we must
add a statistical assumption that \( X_{i+1}^j \) is not coincidentally associated
with the variables that drive a wedge between measured consumption
growth and marginal utility growth. Alternatively, to test that “\( X_{i+1}^j \)
does not enter into the determination of individual households’ con-
sumption allocations” by “\( X_{i+1}^j \) is statistically independent of individ-
ual households’ consumption allocations,” we must assume that
\( X_{i+1}^j \) is not coincidentally correlated with the variables that do deter-
mine individual households’ consumption allocations.

This point and the list of variables of which \( X_{i+1}^j \) must be indepen-
dent are most easily seen in an example. Suppose that each household
\( j \)’s utility function displays constant relative risk aversion with risk
aversion coefficient \( \gamma^j \) and a multiplicative shock \( b_t^j \) (to conform to
the notation above, take \( \delta_i^j = [b_t^j \gamma^j] \)):

\[
u(c_t^j, [b_t^j \gamma^j]) = b_t^j \frac{(c_t^j)^{1+\gamma^j} - 1}{1 + \gamma^j}. \tag{6}\]

The condition (5) that marginal utility growth is equated across house-
holds becomes

\[
\rho^j \frac{b_{i+1}^j(c_{i+1}^j)^\gamma^j}{b_t^j(c_t^j)^\gamma^j} = \frac{\mu_{i+1}}{\mu_i} = \text{constant across households.} \tag{7}\]

Taking logs and adding a measurement error \( \xi_{i+1}^j \) to log consump-
tion growth, we get

\[
\log \left( \frac{c_{i+1}^j}{c_t^j} \right) = \frac{1}{\gamma^j} \left[ \log \left( \frac{\mu_{i+1}}{\mu_i} \right) - \log \left( \frac{b_{i+1}^j}{b_t^j} \right) - \log(\rho^j) \right] + \xi_{i+1}^j. \tag{8}\]

\(^3\) When considering data at many different dates, one can remove the fixed effect
in different ways. For example, one can take the ratio of marginal utility at \( t \) to the
first period’s marginal utility rather than the previous period’s. These specifications
may yield different results by emphasizing features of the data at different frequencies.
Now, consider an endowment or other idiosyncratic variable $X_{t+1}^j$. Equation (8) shows that if $X_{t+1}^j$ is cross-sectionally independent of preference shifts $\log(b_{t+1}^j/b_t^j)$, $\gamma^j$, $\rho^j$, and measurement error $\xi_{t+1}^j$, then $X_{t+1}^j$ is cross-sectionally independent of consumption growth $\log(c_{t+1}^j/c_t^j)$.

Regressions of the form
\[
\log\left(\frac{c_{t+1}^j}{c_t^j}\right) = \alpha + \beta X_{t+1}^j + \epsilon_{t+1}^j, \quad j = 1, 2, \ldots, M \leq N, \tag{9}
\]
can test whether consumption growth and the right-hand variable $X_{t+1}^j$ are independent. If the measurement error and preference shifts are homoskedastic and uncorrelated across households, ordinary least squares (OLS) estimates and $t$-tests can be used to test $\beta = 0$. I shall also use a nonparametric $\chi^2$ test for independence of the events that consumption growth and $X_{t+1}^j$ fall into discrete categories.

It is tempting to cast a nonzero coefficient $\beta$ in (9) as a measurement of the rule for allocating consumption growth conditional on outcomes $X_{t+1}^j$ and thus as a measurement of the strength of consumption insurance as well as a statistical rejection of full insurance. However, if there are several correlated shocks that are not perfectly insured, then a single regression coefficient as in (9) will not be the same as the true allocation rule, which would be revealed by the multiple regression coefficient of consumption growth on all the shocks.

IV. Generalizations

The separability, power form, and restrictions on individual heterogeneity (preference shocks) of the example above are not essential to the proposition that an idiosyncratic variable independent of preference shocks and measurement error should be cross-sectionally independent of individual consumption growth. That result is robust to nonseparable utility, and it can be generalized to other forms of utility.

A. Separability

First, suppose that utility is not separable between goods. Thus include a second good, $l$ for leisure, with an associated endowment $z$. The planning problem (2)–(3) then becomes
\[
\max \sum_{j=1}^{N} \lambda^j E \sum_{t=0}^{\infty} (\rho^j)^t u(c_t^j, l_t^j, \delta_t^j)
\]
subject to $c_t^j = \sum_j c_t^j \leq \sum_j e_t^j = e_t^A$, $l_t^A = \sum_j l_t^j \leq \sum_j z_t^j = z_t^A$. 
There are now two sets of first-order conditions, one with respect to $c$ and the other with respect to $l$. But individual endowments and other idiosyncratic variables still do not enter into the determination of individual allocations, given the aggregates $c^i_t$ and $z^i_t$. For example, with a power specification of utility

$$u(c^i_t, l^i_t, \delta^i_t) = b^i_t(c^i_t)^{1+\gamma^i}(l^i_t)^{1+\phi^i},$$

consumption allocations are a generalization of (8):

$$\log\left(\frac{c^i_{t+1}}{c^i_t}\right) = \frac{1}{1 + \gamma^i + \phi^i} \left[ -\phi^i \log\left(\frac{\mu^c_{t+1}}{\mu^c_t}\right) - (1 + \phi^i)\log\left(\frac{\mu^l_{t+1}}{\mu^l_t}\right) 
- \log\left(\frac{b^l_{t+1}}{b^l_t}\right) - \log(\rho^i) \right] + \xi^i_{t+1}, \tag{10}$$

where $\mu^c_t$ and $\mu^l_t$ are Lagrange multipliers on aggregate consumption and aggregate leisure, respectively. Therefore, $X^i_{t+1}$ cross-sectionally independent of preference shifts $b^l_{t+1}/b^l_t, \gamma^i, \phi^i,$ and measurement error $\xi^i_{t+1}$ again implies $X^i_{t+1}$ cross-sectionally independent of consumption growth.

As (10) shows, the only effect of nonseparability is that the Lagrange multiplier on aggregate leisure (and other consumption goods that enter utility nonseparably) will, in general, enter along with the aggregate consumption multiplier in the determination of individual consumption allocations. This has no effect on a single-period cross-sectional regression since all aggregate quantities are swept into the constant.

It is initially surprising that the cross-sectional regression is robust to nonseparability. For example, one might reason that losing one’s job forces up one’s leisure allocation. If consumption and leisure are not separable, the increase in leisure changes the marginal utility of consumption, and the plan should adjust consumption to reflect that change in marginal utility. Therefore, losing a job should be correlated with consumption growth. The flaw in this logic is that leisure is also part of the plan and is thus allocated across households without regard to endowments, just like consumption. The planner might respond to a preference shift by increasing one household’s consumption and leisure at the same time, but a right-hand variable that is uncorrelated with preference shifts is uncorrelated with such changes in consumption.

The crucial assumption in this example is not separability, but the planner’s ability to freely transfer leisure across households like any other good, for example by having one person do another’s work. If this assumption fails and if leisure enters utility nonseparably, then shocks to the endowment of leisure act as preference shocks for the
marginal utility of consumption. Right-hand variables must then be independent of leisure, as job loss is certainly not. The same is true if there are time-varying, person-specific prices (wage rates).

B. More General Utility Functions

For other utility functions or in the presence of further individual heterogeneity, including multiple nonmultiplicative preference shocks, further statistical assumptions on the right-hand variable can be used to derive its independence from measured consumption growth. These complications just add variables to the list of variables that drive a wedge between marginal utility growth and consumption growth, and of which the right-hand variable must be independent.

If utility does not have the power form with multiplicative shocks studied above, then marginal utility growth is not just a function of consumption growth. But we may still write marginal utility growth as a function of consumption growth and the initial consumption level. Then if the right-hand variable is additionally independent of the initial consumption level, it should still be independent of consumption growth. Precisely, write marginal utility growth as

\[
\rho^j u_c(c_{t+1}, \delta_{t+1}) = \rho^j f(c_t, c_{t+1}, \delta_t, \delta_{t+1})
\]

\[
= \rho^j g\left( c_t, \frac{c_{t+1}}{c_t}, \delta_t, \delta_{t+1} \right) = \frac{\mu_{t+1}}{\mu_t} = \text{constant across } j.
\]

This equation determines an individual household’s consumption growth as a function of a constant (whose value depends on aggregate consumption at the two dates), the initial levels of the household’s preference shocks and consumption, and measurement error. Thus if a right-hand variable \(X_{t+1}\) is cross-sectionally independent of the initial distribution of consumption levels \(c_t\), as well as preference shifts \(\rho^j, \delta_t, \delta_{t+1}\), and measurement error \(\xi_{t+1}\), then it should still be cross-sectionally independent of consumption growth. (If the initial right-hand variable was correlated with initial consumption levels, we could use instead the residuals from a projection of \(X_{t+1}\) on \(c_t\).)

More generally, utility over the stream of contingent allocations \(\{c'(s'), \ell'(s'), \ldots\}\) may have an arbitrary (monotonic and concave) form, it need not be time-separable or have the expected utility property, and arbitrary shocks may be included. Since individual endowments or other idiosyncratic variables do not enter into the determination of individual allocations, these complications serve only to extend the list of variables that they must not be coincidentally associated with if they are to be used as right-hand variables. Of course, the limit of completely arbitrary preferences amounts to a new shock
for each household, date, and state, and with one data point there is no way to check that a right-hand variable is independent of such shocks. Thus the statistical power of such tests declines to zero as fewer restrictions on preferences are adopted.

C. Pooled versus Cross-sectional Regressions

Pooled time-series, cross-sectional regressions are affected by nonseparability, functional form assumptions, and generalization of preference shocks. From (8) and (10), right-hand variables in such regressions must be uncorrelated over time with variations in the growth of Lagrange multipliers, as well as preference shocks and measurement error. They typically are not. For example, individual income (leisure) is likely to be correlated over time with aggregate income (leisure) and hence with the Lagrange multipliers.

To control for this problem, Townsend (1989) and Mace (this issue) include aggregate consumption growth on the right-hand side. However, including it on the right-hand side may fail to control for correlation of the right-hand variable with Lagrange multiplier growth if utility does not have a power form, utility is nonseparable across goods, or there are preference shocks. First, a right-hand variable such as income may be correlated with aggregate leisure and hence the leisure multiplier. If utility is not separable, the leisure multiplier also determines consumption growth (see [10]). Second, changes in the distribution of preference shocks over the population affect the Lagrange multipliers, unless these changes are ruled out. Third, if utility does not have the power form, then the consumption multiplier growth is not proportional to aggregate consumption growth. Fourth, the aggregate is typically taken over the subsample being studied, not the whole economy in question. In all these cases, removing the correlation of the right-hand variable with aggregate consumption growth by including the latter in a regression may not remove its correlation with growth in the Lagrange multipliers, so the right-hand variable may spuriously enter the regression.

This discussion is not meant as a criticism of Townsend’s and Mace’s papers, but just to point out the different assumptions required for pooled versus strictly cross-sectional regressions. These potential sources of bias may be unimportant. Yet again, they may not.

V. Results

The data are taken from the Panel Study of Income Dynamics (PSID). The Appendix presents a description of the variables. The consumption measure is growth in food consumption from 1980 to 1983, in
percentage units. The right-hand variables are cumulated from 1981 + 1982 + 1983. Thus 1980 serves as a base year, and the regressions study the effect of the cumulated shocks from 1981 to 1983 on 1983 consumption. I chose a period longer than a year so that more households would receive shocks and so that the timing problems that result from using a discrete-time model to study time aggregates would be reduced. One can also study longer periods, but rejections would then suggest slow changes in living standards over many years rather than the effects of sudden shocks that might more plausibly be insured.

The right-hand variables include days of work lost because of illness, a dummy variable for involuntary job loss, weeks spent by the household head looking for employment if he or she suffered an involuntary job loss, days of work lost to strikes, and a dummy variable equal to one if the household reported that it was forced to move as a result of external events. I also include the number of individuals moving in or out of the household, as a test whether the technique can pick up a coefficient that should be positive, and growth in total family income, to compare the results with other tests of consumption insurance.

Of course, these are not perfect right-hand variables. One can devise stories under which each might be correlated with preference shifts or measurement errors in consumption. In particular, they rest on the idea that an "involuntary" job loss or move and illness or strikes are best modeled as changes in circumstance or "endowment" that the plan should insure, where a "voluntary" job loss, move, or other decision variable may be induced (as part of the plan) by a preference shift and, hence, should not be used as a right-hand variable. This identification could be mistaken. But these variables are the best the PSID has to offer, and they are at least plausibly better right-hand variables than reported income.

I present results for the subsample of households with no composition changes as well as for the full sample. Composition changes imply a shift in the household's utility function. If the right-hand variables are correlated with these shifts, excluding households with composition changes reduces bias. If the right-hand variables are not correlated with these shifts and if the shifts do not increase the variance of the error term more than they increase the sample size, including households with composition changes can improve the precision of estimates. Also, if the right-hand variable is not correlated with preference shifts because of composition change, then the results should be roughly the same for both groups.

Table 1 presents summary statistics for consumption growth. Note the wide cross-sectional dispersion in consumption growth rates,
TABLE 1

Statistics on Consumption Growth: Percentage Change from 1981 to 1983

<table>
<thead>
<tr>
<th>Households That Change Composition</th>
<th>Not in Sample</th>
<th>In Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean consumption growth</td>
<td>13.99</td>
<td>12.53</td>
</tr>
<tr>
<td>Standard deviation</td>
<td>47.27</td>
<td>59.55</td>
</tr>
<tr>
<td>Number of households</td>
<td>1,741</td>
<td>4,629</td>
</tr>
</tbody>
</table>

which suggests that there is a good deal of measurement error in the consumption data.

Table 2 presents the tests of consumption insurance. For each right-hand variable, table 2 presents the OLS regression coefficient of consumption growth on a constant and that variable, an OLS regression using a dummy version of the right-hand variable (one if \( X_{t+1}^i > 0 \), zero if \( X_{t+1}^i \leq 0 \)), and a \( \chi^2 \) test for independence of the events consumption growth \( > 0 \) and right-hand variable \( > 0 \). The OLS regression using dummy variables is equivalent to an estimate and test of the difference in sample means between the group with \( X_{t+1}^i > 0 \) and \( X_{t+1}^i \leq 0 \).

The days of illness variable is statistically significant in the regressions. The relation is primarily due to households with lots of illness: the \( \chi^2 \) test and the regression of consumption growth on the days \( > 0 \) dummy show almost no effect of illness on consumption growth, but the regression of consumption growth on the dummy days of illness \( \geq 100 \) gives a large (11–14 percentage point decline in consumption growth) and significant coefficient. Furthermore, that coefficient is larger than the slope coefficient on days of illness alone would suggest. Overall, the results suggest that short spells of illness are well insured, which is an obvious feature of most employment contracts, but that very long spells are not fully insured.

The involuntary job loss variable has the largest and most significant coefficients. Households that experienced an involuntary job loss had consumption growth 24–27 percentage points lower than households that did not, which is about half of the standard deviation of reported consumption growth rates. The \( t \)-statistics and probability value for the \( \chi^2 \) test clearly reject independence. Figure 1 presents a histogram of the conditional distributions of consumption growth given an involuntary job loss and given no involuntary job loss, and the difference in the conditional distributions is clearly visible. (Dynarski and Sheffrin [1987] find a similar effect of job loss on consumption.)
TABLE 2
CROSS-SECTIONAL REGRESSIONS OF CONSUMPTION GROWTH ON IDIOSYNCRATIC VARIABLES AND TESTS FOR INDEPENDENCE

<table>
<thead>
<tr>
<th>Composition Changes in Sample?</th>
<th>Regression Coefficient</th>
<th>( p )-Value of ( \chi^2 ) Test (%)*</th>
<th>Number of Households in Category or Other Statistic</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Days</td>
<td>Days &gt; 0</td>
<td>Days ≥ 100</td>
</tr>
<tr>
<td>No</td>
<td>-.05</td>
<td>-.18</td>
<td>-14.22</td>
</tr>
<tr>
<td></td>
<td>(-2.36)</td>
<td>(-.78)</td>
<td>(-2.89)</td>
</tr>
<tr>
<td>Yes</td>
<td>-.06</td>
<td>.70</td>
<td>-11.27</td>
</tr>
<tr>
<td></td>
<td>(-3.74)</td>
<td>(.40)</td>
<td>(3.29)</td>
</tr>
</tbody>
</table>

2. Involuntary Job Loss

|                                | Regression Coefficient | \( p \)-Value of \( \chi^2 \) Test (%)* | Number of Households in Category or Other Statistic |
|                                |                      |                                   | Total | Lost Jobs |
| No                             | -24.03 (-4.95)       | -.065                               | 1,173 | 76       |
| Yes                            | -26.74 (-7.81)       | 1.1E-10                             | 3,373 | 291      |

3. Weeks Job Search Given Involuntary Job Loss

<table>
<thead>
<tr>
<th></th>
<th>Weeks</th>
<th>Weeks &gt; 0</th>
<th>Mean Weeks</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Weeks &gt; 0</td>
<td></td>
<td>If Weeks &gt; 0</td>
</tr>
<tr>
<td>No</td>
<td>-.34</td>
<td>-5.65</td>
<td>99.9</td>
</tr>
<tr>
<td></td>
<td>(-1.10)</td>
<td>(-.58)</td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>-.73</td>
<td>-15.92</td>
<td>10.4</td>
</tr>
<tr>
<td></td>
<td>(-3.83)</td>
<td>(-1.90)</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Weeks &gt; 0</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>No</td>
<td>18</td>
<td>22.56</td>
</tr>
<tr>
<td>Yes</td>
<td>46</td>
<td>29.52</td>
</tr>
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</table>
4. Strike Days

<table>
<thead>
<tr>
<th></th>
<th>Days</th>
<th>Strike Days &gt; 0</th>
<th>Total</th>
<th>Days &gt; 0</th>
</tr>
</thead>
<tbody>
<tr>
<td>No</td>
<td>-.21</td>
<td>-9.52</td>
<td>83.8</td>
<td>1,741</td>
</tr>
<tr>
<td></td>
<td>(-1.24)</td>
<td>(-1.20)</td>
<td></td>
<td>36</td>
</tr>
<tr>
<td>Yes</td>
<td>-.05</td>
<td>-7.15</td>
<td>94.1</td>
<td>4,629</td>
</tr>
<tr>
<td></td>
<td>(-.37)</td>
<td>(-1.32)</td>
<td></td>
<td>123</td>
</tr>
</tbody>
</table>

5. Involuntary Move

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>Move</th>
</tr>
</thead>
<tbody>
<tr>
<td>No</td>
<td>15.59</td>
<td>(2.28)</td>
</tr>
<tr>
<td></td>
<td>69.8</td>
<td>.41</td>
</tr>
<tr>
<td>Yes</td>
<td>-5.64</td>
<td>(-1.67)</td>
</tr>
<tr>
<td></td>
<td>1,741</td>
<td>49</td>
</tr>
<tr>
<td></td>
<td>4,629</td>
<td>335</td>
</tr>
</tbody>
</table>

6. Income Growth

<table>
<thead>
<tr>
<th></th>
<th>Income Growth</th>
<th>Income Growth &gt; 0</th>
<th>Total</th>
<th>Mean Growth</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>No</td>
<td>.05</td>
<td>8.74</td>
<td>.71</td>
<td>1,065</td>
<td>3.59%</td>
</tr>
<tr>
<td></td>
<td>(2.43)</td>
<td>(2.75)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>.10</td>
<td>15.94</td>
<td>6.8E-07</td>
<td>3,156</td>
<td>4.41%</td>
</tr>
<tr>
<td></td>
<td>(7.09)</td>
<td>(6.93)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

7. Number Move in Minus Move out

<table>
<thead>
<tr>
<th></th>
<th>Number</th>
<th>Number &gt; 0</th>
<th>Total</th>
<th>Number ≠ 0</th>
<th>Movers (Mean)</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yes</td>
<td>16.10</td>
<td>28.32</td>
<td>1.1E-21</td>
<td>4,629</td>
<td>.004</td>
<td>.98</td>
</tr>
<tr>
<td></td>
<td>(18.7)</td>
<td>(13.5)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note.—All regressions are log consumption growth = \( \alpha + \beta \) right-hand variable + error and are estimated by OLS. When right-hand variables are continuous, results are also reported for regressions using dummy versions of the right-hand variable. For example, the coefficient under days \( \geq 100 \) in panel 1 is a regression of consumption growth on a dummy variable that takes the value one if that household has more than 100 days of illness. *t*-statistics are in parentheses.

* This value gives the percentage probability value of a \( \chi^2 \) test of the hypothesis that consumption growth is greater than zero and either the right-hand variable, if already a dummy, or right-hand variable greater than zero is independent.
The number of weeks spent looking for employment by the head conditional on an involuntary job loss is an attempt to find a continuous scale over which the effects of a job loss can be seen. One might expect that a job loss that was followed by little unemployment would have smaller effects on consumption than a job loss followed by a long search for a new job. Here, the evidence is less clear. Among households with no composition change, the coefficients on weeks of search and the $\chi^2$ are far from significant. Among households with a composition change, the coefficients are larger and more significant. But one hesitates to trust results that hold only in families that change composition since this suggests that the right-hand variable enters only because it is correlated with preference shifts accompanying the composition change.

These results are also puzzling in another way. The coefficients on the dummy for weeks $> 0$ are much smaller than the coefficients for the involuntary job loss dummy: those who look for work following a job loss suffer less loss in consumption growth than those who lose a job and do not search at all! Furthermore, most households that report an involuntary job loss do not report any weeks spent looking for employment. Only 18 of the 76 families with no composition change and 46 of the 291 total families that experienced an involun-
tary job loss reported any unemployment. This fact calls into question just how involuntary the “involuntary job loss” really is, that is, whether it really is uncorrelated with preference shifts, and it calls into question the stylized picture that involuntary job loss causes economic distress due to lost wages during the long search for a new job.

The strike variable is another external shock to labor income. The coefficients are economically interesting: households that reported any work loss due to a strike reported seven to nine percentage point consumption drops, and each day of strike produces one to four times as much loss in consumption as each day of illness. However, the coefficients are all statistically insignificant.

The involuntary move dummy is an indicator of an external shock to the household. In the sample with no composition changes, the coefficient is large (15 percentage points) and significant ($t$-statistic of 2.28) but has the “wrong” sign: a move increases consumption. For the whole sample, the coefficient is negative but relatively small. Its $t$-statistic is insignificant, though the $\chi^2$ test rejects independence.

The regression of consumption growth on income growth yields a positive and significant coefficient. The coefficients imply that a 1 percent increase in income results in a $0.10 - 0.20$ percent increase in consumption. (Income is a 2-year growth rate, while consumption growth is measured over 4 years.) The permanent income hypothesis with no risk sharing predicts that a purely transitory 1 percent income shock increases consumption by 1 percent times the interest rate, so these are “large” coefficients from the point of view of consumption insurance. They are also somewhat larger than the coefficients Mace (this issue) finds for data from the Consumer Expenditure Survey. However, the fact that the coefficients on income are roughly double for households with a composition change is consistent with the view that income changes are correlated with household preference shifts, in this case induced by household composition changes.

The regression of consumption growth on net number of new people added to the household is a test of the power of the technique to uncover a coefficient that ought to be positive and large in the substantial measurement error of the consumption variable. Consumption growth increases by 16.10 percentage points for every added person, with a $t$-statistic of 18.7; the $\chi^2$ test has an impressive $p$-value of $1.06 \times 10^{-23}$.

VI. Concluding Remarks

The central contribution of this paper is a simple and robust technique for testing consumption insurance and for measuring which
shocks are and are not insured. The tests are based on the proposition that measured consumption growth rates should be independent of variables that are exogenous to households.

Many of the variables examined here do not yield a robust rejection of the theory: the coefficients or the t-statistics are small in samples of several thousand, or the rejection holds only for the suspect sub-sample. Thus the evidence does not contradict full insurance for illness of less than 100 days, spells of unemployment following an involuntary job loss, loss of work due to strike, and an involuntary move.

On the other hand, the loss of more than 100 days of work due to illness and the involuntary loss of a job are important right-hand variables, whose associations with consumption growth are both economically and statistically significant. Of course, one can argue that these results indicate violations of assumptions rather than of theory. Sick people might lose their appetites: a correlation of the right-hand variable with a preference shift in the error term. If leisure and consumption are substitutes and leisure cannot be transferred across individuals, a job loss will be correlated with decreased consumption, even under perfect risk sharing.

Income growth also yields a statistically and economically significant rejection, confirming other studies. However, income is likely to be correlated with cross-sectional variation in preferences and consumption measurement error, so this is not good evidence against consumption insurance.

Consumption insurance regressions can be extended in a variety of ways: most obviously to other right-hand variables, to see what other kinds of shocks are and are not insured, and to other data sets. Also, consumption insurance may hold more closely among groups that are geographically close or work together or among relatives than it does in society at large, since by regular contact such groups are better able to work out informal implementation mechanisms. (That it does may explain why this paper rejects consumption insurance for a sample distributed throughout the United States, but Townsend [1989] does not reject consumption insurance in farming villages in India.) Consumption insurance regressions among sub-groups can be used to test this idea.

Eventually, research on consumption insurance should try to isolate the alternatives rather than simply reject the null. Ideally, lack of insurance should be tied to problems of observability of effort or output, to the imperfect enforceability of contracts, and so forth. Also, it is desirable eventually to go beyond testing whether the final allocation satisfies a Pareto optimum and examine what the implementation mechanisms are: following an insured shock, who does pay to maintain a household’s consumption?
Appendix

Variable Definitions

Consumption growth: Percentage consumption growth, $100 \times \log(1983$ consumption + $1980$ consumption). Consumption is total food consumption (food at home plus food stamps plus meals away from home). Data are rejected if there is split-off (family starts in sample), refusal of an interview, bad quality of the match, or bad food accuracy codes; the household members were farmers; or consumption equaled zero in either 1980 or 1983. One set of regressions includes a further selection if there is any household composition change from 1980 to 1983.

Illness: Days of work missed by head in 1981, 1982, and 1983 because worker or someone else was ill. Data were rejected for bad accuracy codes.

Involuntary job loss: Dummy variable equaled one if head was employed in 1980, lost job in 1981, 1982, or 1983, was unemployed, and gave reason (1) “company folded/changed hands/moved out of town; employer died/ went out of business,” (2) “strike; lockout,” or (3) “laid off; fired.” Lost job variable equaled zero if head was employed in 1980 and stayed employed or lost job for other reasons, including quit. Data were rejected if head was not employed in 1980.


Strike days: Days of head’s work lost because of strikes in 1981, 1982, and 1983. Data were rejected for bad accuracy codes.

Involuntary move: Dummy equaled one if head moved in 1981, 1982, or 1983 because of “response to outside events (involuntary reasons): [household unit] coming down, being evicted, armed services, etc., health reasons, divorce, retiring because of health.”


Number move in minus out: Total number of movers in minus movers out of household in 1981, 1982, and 1983.

References


