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Wage Shocks and Consumption Variability in Mexico during the 1990s

By

Orazio P. Attanasio*
Miguel Székely**

***University College, London**
****Inter-American Development Bank**

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Abstract[♦]

This paper presents evidence on the relationship between economic shocks to relative male wages and changes in household consumption in Mexico during the 1990s, which is a period characterized by high volatility. In addition to performing this type of analysis for Mexico for the first time, the paper makes two main contributions. The first is the use of alternative data sources to construct instrumental variables for wages. The second is to examine differences across four consumption categories: non-durable goods, durable goods, education and health. Our results for non-durable goods consumption reject the hypothesis that Mexican households are able to insure idiosyncratic risk. For the comparisons across consumption categories, the conclusion is that households in Mexico tend to react to temporary shocks by contracting the consumption of goods that represent longer-run investment in human capital, which makes them more vulnerable in the future.

JEL Classification:
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1. Introduction

During the 1990s Mexico underwent a number of important shocks to its economy. Some of them reflect changes in the return to education that have also affected developed economies, albeit in slightly different manners. Others, such as the crisis that followed the peso devaluation of 1995, have been specific to Mexico and have anticipated large shocks that have affected developing countries, in particular in South East Asia, slightly later. There is now an extensive literature on the macroeconomics of these shocks.[1] The evidence on the microeconomic and distributional consequences of these shocks, however, is somewhat limited, and has mainly focused on income inequality. To the best of our knowledge, there is no analysis of the evolution of inequality in recent years in Mexico that considers what is probably the most important determinant of welfare: consumption.[2]

In this paper we use time series of cross-sectional data to study the evolution of consumption across and within education groups and relate it to changes in the distribution of wages. In doing so, we make an extensive use of synthetic cohort techniques. Our use of cohort techniques is justified not only on technical grounds (because of the absence of true panel data) but also on theoretical grounds: as Blundell and Preston (1998) point out, meaningful welfare comparisons and measures of welfare inequality based on consumption should compare individuals at the same point in their life cycle.

The main aims of this study are fourfold. First, we document briefly the evolution of relative wages across education groups and cohorts. We show that, in contrast to the US, where college graduates uniformly "win" during the 1980s and early 1990s, in Mexico the picture is more mixed. In particular, we see that for some years and some cohorts, better-educated people fare worse than people with less education. Second, we relate the changes in relative wages to changes in relative consumption. This piece of evidence can be interpreted from a purely descriptive point of view or could be interpreted, as in Attanasio and Davis (1996)—subsequently AD—as a test of the hypothesis of complete risk sharing across cohort and education groups. While a non-rejection of the hypothesis of full risk sharing would indeed be surprising, it constitutes a useful theoretical and practical benchmark. Third, we consider the

[1] See for instance, IDB (1995), and Hausmann and Fernández-Arias (2000a and 2000b.).

[2] Some recent examples are Lustig and Székely (1998), Wodon *et al*. (2000), Morley (2000), and ECLAC (2000). To our knowledge, the only recent works examining the distributional changes of shocks in Mexico are the papers by Cunningham and Maloney (2000, 2001), but these papers do not include an analysis of effects on consumption.

evolution of the variance of consumption and wages. This part of the paper follows and develops work by Deaton and Paxson (1994), Attanasio a nd Jappelli (2001) and Attanasio, Berloffa, Blundell *et al*. (2001) and Attanasio, Blundell, Preston *et al*. (2001). In particular, we test the restrictions implied by full risk sharing on the evolution of the cross-sectional variance of consumption. As we discuss below, these tests are complementary to those that look at the means of consumption. Fourth, in addition to non-durable consumption, we also consider the effects of changes in relative wages on other variables such as durable expenditure, health expenditure and education expenditures. The aim of this exercise is more descriptive but equally important. It has been argued that poor households might react to shocks by reducing investment in human capital (health and education expenditure). As a consequence, even relatively short-lived shocks might have persistent or even permanent consequences. Moreover, it might be the case that household durable expenditures are more reactive to changes in relative wages than non-durable consumption. Our exercise provides some evidence on these issues.

The rest of the paper is organized as follows. In Section 2, we summarize the theory of full risk sharing and its empirical implications. In the second part of the section, we also discuss our empirical specifications and the relevant econometric problems. In Section 3 we briefly describe the data sources, as well as the evolution over time of the variables of interest. In Section 4 we present our tests of risk sharing based on means and variances changes. In Section 5 we consider changes in expenditure items other than non-durable consumption. Section 6 concludes the paper with our interpretation of the results and some thoughts on future research.

2. Risk Sharing and its Implications.

The assumption of full risk sharing and perfect insurance markets has received a considerable amount of attention since Townsend's (1994) seminal contribution. Such a hypothesis has two important attractions. First, it constitutes an important conceptual benchmark against which it is interesting to measure the amount of risk sharing and consumption smoothing achieved in real economies. Second, it is possible to characterize in a relatively simple way the implications of such a hypothesis and test them. Moreover, as we discuss below, one can construct tests that are informative about the nature of the possible violation of the null.

2.1 Full Insurance: A Test Based on Means

As is well known, first best equilibrium allocations can be characterized by looking at the first order conditions of a social planner problem. The social planner is assumed to maximize the weighted utility of all agents with a set of arbitrarily fixed non-zero Pareto weights subject to a resource constraint. This dynamic optimization gives rise to the following first order condition:

$$(1) \qquad U_c(c_t^i(s_t), z_t^i(s_t))\, l^i\, b^i = m(s_t)$$

where U_c is the marginal utility of consumption for individual *i*, which is assumed to depend on its non-durable consumption and, possibly, on a vector of other variables *z*, some of which might be unobservable. l^i is the Pareto-weight given by the social planner to individual *i* in the maximization problem. Different sets of weights will correspond to different competitive equilibria with full risk sharing. The theory is silent regarding what determines these weights, except in saying that they are constant over time. b^i is the discount factor for individual *i*, and $m(s_t)$ is the Lagrange multiplier associated with the resource constraint at time *t*, if the state of the world is s_t. Since the work of Townsend (1994), Mace (1991) and Cochrane (1991), equation (1) has been used extensively to test the empirical implications of a model with full risk sharing. If one takes the log of equation (1) and considers it at two different time periods, one can eliminate both the discount factor and the unobserved Pareto weight. In particular, one gets:

$$(2) \qquad \log(U_c(c_t^i(s_t), z_t^i(s_t))) - \log(U_c(c_t^i(s_t'), z_t^i(s_t'))) = m(s_t) - m(s_t')$$

where s_t' is the state of the world at time t. Notice that t and t need not be adjacent periods.[3] If they are *k* periods apart, one can write equation (2) as:

$$(3) \qquad \Delta^k \log(U_c(c_t^i(s_t), z_t^i(s_t))) = n(s_t, s_{t-k}')$$

The main implication of equation (3) is that changes over time in the marginal utility of different individuals should be the same. The Pareto weights have been eliminated by differencing. The changes in marginal utility should be unaffected by the idiosyncratic shocks

[3] Moreover, if one considers many pairs of observations, the distance between the two time periods need not be the same.

received by individuals. Full insurance means that changes in the amount of resources available to an individual over and above the aggregate change should not be reflected in changes in marginal utility. The resource constraint can be taken into account by considering either time dummies, or, as in Mace (1991), aggregate consumption. Any other variable, such as individual income, should therefore not enter equation (3).

In the absence of panel data that follow the same individuals over time, one can still test the implications of perfect risk sharing by aggregating equation (3) over individuals belonging to a given group whose membership is assumed to be fixed over time. Insurance across groups implies that the average marginal utility of consumption for different groups should change in the same way and should be unaffected by group level shocks.

AD used synthetic panels to test equation (3) by forming year of birth and education groups and following the averages for these groups over time. The synthetic panel approach has two big advantages and one disadvantage. The advantages are the possibility of testing equation (3) even in the absence of longitudinal data and the gains in power that might be obtained averaging measurement error in wages, income or whatever measures of individual resources are used for the members of a group. The disadvantage is the fact that one focuses only on the insurance across groups. By taking averages over the members of a group, one cannot say anything about the extent of risk sharing within a group.

Assuming that the utility function is given by the following expression:

$$U(c_t^i(s_t), z_t^i(s_t)) = \frac{(c_t^i)^{1-g}}{1-g}\exp(q' z_t^i) \tag{4}$$

equation (3), aggregated over the individuals belonging to a group *g*, will take the form:

$$\Delta^k \frac{1}{\# g_t}\sum_{i\in g}\log(c_t^i) = q'\Delta^k \frac{1}{\# g_t}\sum_{i\in g} x_t^i + d_t + e_t^g \tag{5}$$

where the vector *x* includes the observable components of *z*, *#g* is the sample size of group *g*, and e_t^g reflects both the unobserved components of *z* and possible measurement error. Variables in *x* typically include demographic factors such as family size and composition. Given the nature of the averaging, it is likely that the residual term e_t^g has an MA(1) structure. To test the hypothesis of perfect insurance one adds to equation (5) variables that reflect changes in the amount of resources available to group *g*, such as the change in average (log) wages, and test the hypothesis that that coefficient is zero:

$$(5') \qquad \Delta^k \frac{1}{\# g_t} \sum_{i \in g} \log(c_t^i) = q' \Delta^k \frac{1}{\# g_t} \sum_{i \in g} x_t^i + d_t + f \Delta^k \frac{1}{\# g_t} \sum_{i \in g} \log(w_t^i) + e_t^g$$

Notice that, given the nature of the residuals under the null of perfect insurance, unlike with the estimation of Euler equations, one does not need to rely on large T asymptotics. Provided that enough groups can be identified, one can estimate consistently equation (5') using just two time periods. Notice also that if one does not want to give to (5') the structural interpretation we have been discussing, ignoring for a moment the role played by the demographic variables *x*, the coefficient f can be interpreted as the extent to which changes in *relative* wages are translated into changes in *relative* consumption.

If one wants to avoid taking differences of equation (1) one can still test the implications of the model, if enough time periods are available. To see this, take logs of equation (1) and aggregate across individuals belonging to group *g*.

$$(6) \qquad \frac{1}{\# g_t} \sum_{i \in g} \log(c_t^i) = q' \frac{1}{\# g_t} \sum_{i \in g} x_t^i + d_t + \frac{1}{\# g_t} \sum_{i \in g} \log(l^i \; b^i) + x_t^g$$

The third element of the right-hand-side of equation (6) can be taken care of by group dummies. If both the number of groups and the number of time periods is large enough, we can estimate consistently equation (6) and test whether variables such as average wages appear significantly in it.

Both in equation (5') and in equation (6), the presence of possible measurement error in wages (and the use of synthetic panels techniques on samples of limited size) induces attenuation biases. For this reason, it is necessary to use instrumental variable techniques. AD used complicated lagging schemes to construct appropriate instruments for log wages and their changes in equations (6) and (5'). This paper uses an alternative strategy that we discuss below.

Before moving to our next test, we should notice one last difference with respect to the test in AD on US data. As the Mexican data are not available for every year, we cannot perform all the tests that AD perform at different frequencies. Moreover, the surveys available are not equally spaced, so that strictly speaking the test we present does not refer to any specific frequency.[4] However, as mentioned in footnote 3 above, this does not matter for the validity of the test.

2.2 Full Insurance: A Test Based on Variances

As mentioned above, applying the tests discussed in the previous subsection to synthetic panel data, one considers the hypothesis of perfect risk sharing across groups, while ignoring what might be happening within groups. Therefore, to complement this evidence we perform an additional test, recently proposed by Albarran and Attanasio (2001) and Attanasio, Blundell, Preston *et al.* (2001), developing an idea in Deaton and Paxson (1994).[5] Consider once again the log of equation (1) above, and re-write it as follows:

$$(7) \qquad \log(U_c(c_t^i(s_t), z_t^i(s_t))) = m(s_t) - \log(l^i \; b^i)$$

Having defined groups with fixed membership, as in the analysis before, one can compute the cross sectional variance of both sides of equation (7). As the resource constraint multiplier is common across individuals it does not contribute to the cross sectional variance. The Pareto weights and the discount factors are constant over time: therefore an implication of the theory is that the cross sectional variance of the marginal utility of consumption is constant over time. While this implication had been noted in passing by Deaton and Paxson (1994), Albarran and Attanasio (2001) and Attanasio, Blundell, Preston *et al.* (2001) propose and implement explicit tests of such a hypothesis.

If we assume that the utility function in equation (4), equation (7) can be written as:

$$(8) \qquad Var_g(\log(c_t^i)) = d_g + Var(q' z_t^i)$$

where the subscript *g* indicates the fact that the variance is computed within a group, the *d* are group dummy variables and reflect the variance of Pareto weights and discount factors (as well as the covariance between these and the *z* variables that, in equation (7), take into account factors that might affect the marginal utility of consumption). Taking first differences of equation (8) one gets:

[4] Even though most surveys are two years from each other.

$$\Delta Var_g(\log(c_t^i)) = \Delta Var(q' z_t^i) \quad (9)$$

Equations (8) and (9) can be tested by adding other variables to check whether (changes in) the cross sectional variance of the marginal utility of consumption vary systematically. In particular, in what follows, we introduce (changes in) the cross sectional variance of log wages.

Notice that this test focuses on what happens within a group, so that it is complementary to the test discussed in the previous subsection. Notice also that there might be situations in which the hypothesis of perfect insurance is violated while the test just proposed does not reject the null. This is because, as is apparent from (7), the hypothesis of full insurance implies the constancy of the whole distribution of marginal utility. One can readily think of situations where the cross-sectional variance of the distribution does not change, while all the other moments or the order of the observations change, therefore violating the null.[6] This argument, however, is mainly an argument about the power of the proposed test.

2.3 Econometric Issues

As mentioned in Section 2.1, in the presence of measurement error or limited sample size, it is necessary to instrument any additional variable one adds to equations (5'), (7), (8) or (9) to eliminate or reduce attenuation bias. Unlike AD, we do not use lags and leads of these variables to construct instruments; instead, we use an alternative data source. In particular, we have two time series of cross sections that are drawn from the same population. While only one contains consumption information, both contain information on male wages, as well as information on group membership. Therefore we can use the average log wage (or its change) in one survey as an instrument for the same measure in the other survey. Before doing so, however, we check that, even after removing year (and in the case of levels, group) effects, the instrument still contains explanatory power for the wage measure in the original data set. A similar argument applies to the variances.

The fact that different cells have different sample sizes induces heteroskedasticity in our equations: larger cells will have lower variances. For this reason we weight all our equations by

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[5] See also Attanasio and Jappelli (2001) and Jappelli and Pistaferri (2001).

[6] In a recent paper, Jappelli and Pistaferri (2001) notice that the full insurance hypothesis implies non-mobility and, therefore, they compute Shorrocks mobility indexes to test it. This test, however, can only be used if a panel is available.

(the square root) of the sample size, therefore down-weighting the observations based on few observations.

The computation of the standard errors for the equation in first difference has to take into account the fact that, as the sample means are computed in levels, differencing induces MA(1) residuals. All results reported below take this feature of the residuals into account.

2.4 Extensions: Additional Expenditure Categories

In Section 4, we report the results obtained applying the tests we discussed so far to data on non-durable consumption. However, utility is also likely to be affected by the consumption of other commodities, such as durable goods, health or education. Therefore, we are implicitly assuming separability of the utility function between non-durable consumption and other commodities. As we have information on the expenditures on these other commodities, in principle, one could derive equations similar to (5') for the marginal utility of these items. The difficulty in following this approach lies in the fact that for these three items we do not measure consumption but expenditure. Expenditure on durables will have an effect not only on current utility but also on future utility. Health and education can be considered investment in human capital. For these reasons it is not possible to interpret the relationship between changes in relative wages and relative expenditures as a test of perfect insurance (as we did for non-durable consumption). However, we think that evidence on these items is interesting for at least two reasons.

First, it is possible that income (wage) shocks may have little short-term impact on non-durable consumption, and still have long lasting effects on welfare through inhibiting human and physical capital accumulation. In other words, (partial) smoothing of shocks might be achieved by changing investment strategies and therefore changing long-term prospects. Second, comparing whether wage shocks have larger impact on durable goods consumption, health, or education is in itself of interest. Differences among these three items are likely if their elasticity of substitution (and that of non-durable consumption) is different—that is, if households find it less painful (in the short run) to deal with changes in education expenditure than with changes in non-durable consumption. More generally, individuals might find it easier to reduce expenditure on some items rather than others. Browning and Crossley (1999) present a model in which liquidity-constrained individuals will react to temporary shocks by reducing the replacement of small durables that do not have a second-hand market and therefore implicit adjustment costs. Browning and Crossley (1999) present some evidence in this respect.

3. Data

In this paper we use two main data sources: the National Household Survey (which contains, among many other variables, data on both consumption and wages) and the National Employment Survey (which reports information on individual wages). This section presents the data and briefly describes the evolution of the variables used in the rest of the analysis.

3.1 Data Sources Description

Consumption and income data at the micro economic level are available in Mexico through the National Income and Expenditure Household surveys (ENIGH). This survey is representative at the national level and is also representative of rural and urban areas separately. ENIGH surveys are available for 1989, 1992, 1994, 1996 and 1998.[7] The five surveys are strictly comparable in terms of sampling frame, sampling methodology, timing (the survey is held during the last quarter of the year), and recall periods. The surveys are also strictly comparable in terms of the questionnaires that capture income and expenditures, which are the key variables of interest.

The data on consumption refer to the *household* as unit of observation. We impose two restrictions on the sample. The first is that we exclude all information from rural areas in order to make the household survey data compatible with the employment surveys, which are described later. The second is that we include only households whose head is between 25 and 65 years of age. This restriction is to minimize the effect of changes in sample composition due to household formation at early ages and household dissolution and differential mortality rates across socioeconomic groups at older ages. After these restrictions, we end up with urban samples of 7,401, 5,535, 6,614, 7,604, and 5,947 households for 1989, 1992, 1994, 1996 and 1998.

Three transformations to the consumption data are performed. The first is to compute a measure of non-durable consumption by subtracting from total consumption mortgage payments, expenditure on furniture and household appliances, orthopedic and therapeutic items, vehicles and leisure goods. The second transformation is to convert non-durable consumption into adult

[7] An additional survey for 1984 is also available, but we do not use it here since the data to construct instrumental variables for wages is not available.

equivalent units by using the scale proposed by Contreras (1996).[8] The third is to deflate each observation by using the CPI. The same CPI is used for the wage data discussed below.

Individual data on wages in the ENIGH surveys are available for each income earner in the household. We restrict the sample of wage earners to urban males ages 25-65, which results in sample sizes of 6,402, 4,676, 5,574, 6,508, and 5,071 individuals for each year, respectively. The gender restriction is imposed to guarantee greater stability in the sample throughout the 1989-1998 period. Unlike females, males of ages 25-65 are typically employed and have very low unemployment rates. The average labor force participation rate for this group is 94.2% across surveys, and the average unemployment rate is only 2.1%.[9]

Each survey contains a detailed breakdown of all income sources, including income from labor, entrepreneurial rents, interest income, property rents, transfers, and non-monetary income. To construct a comparable measure of wage incomes we use the survey questionnaire to identify the income obtained specifically as remuneration to labor earnings during the previous month, and then we divide that income by the number of self-reported worked hours. This is deflated by the CPI to compute real hourly wages. When an individual has more than one job, we compute real hourly wages from all jobs. The procedure is applied to all labor-income earners regardless of whether they are employees or self-employed. For the self-employed, having a breakdown of all other income sources in the survey questionnaires reassures us of the low measurement error in wages.

As already explained, in the presence of measurement error or limited sample size, we have to construct instrumental variables for wages to eliminate or reduce attenuation bias in our empirical estimation. Fortunately, for Mexico there is an alternative source of micro data on individual wages, drawn from the same population as the ENIGH, which we can use to construct instruments. This source is the Urban Employment Survey (ENEU), which has been conducted on a quarterly basis since 1987. The ENEU is a rotating panel with replacement every 5 quarters. Recent rounds of the ENEU have national coverage, but since the earlier ones are restricted to urban areas we restrict the sample to urban areas in all years. The survey questionnaire is much shorter than the ENIGH questionnaire and it focuses mainly on labor market characteristics. This

[8] This scale was originally developed for Chile, and it is used here because an adult equivalence scale for Mexico is not available. The scale gives a weight of 1.2 to the first adult, 0.8 to individuals 11 years of age or older, 0.4 to children ages 5-10 and 0.3 to children under age 5.

[9] This includes an increase in unemployment rates to about 5% for 1996, which is the year with the highest rate.

allows for a much larger sample size. The fourth wave of the survey, which we use here, is ideal for our purposes because it is held at practically the same dates as the ENIGH, uses the same survey questionnaire to obtain information on wages, and uses the same sampling frame and survey methodology as the ENIGH. Recall periods for wages are also the same as in the household survey.

To construct hourly wages we follow the same procedure as the ENIGH: we identify the income item that corresponds to wages (the income obtained specifically as remuneration to labor earnings), divide it by the number of self-reported hours, and then use the CPI to deflate the hourly wage. In this case we also include all wage earners—regardless of whether they are self-employed, informal workers or formal workers—and, as with the ENIGH, we restrict the sample to males 25-65 years of age. We end up with sample sizes of 28,549, 54,309, 56,884, 61,634, and 69,015.

As is common with employment surveys, the larger sample sizes increase the precision of wage estimates, but this comes at the cost of having a questionnaire with lower detail in incomes. Incomes are typically not broken down into categories, and this might induce some measurement error into the wage data. In fact, hourly wages in the ENEU data are typically 30% greater than hourly wages in the ENIGH for the same sample and period of time. However, one should remember that we are using the ENEU survey to instrument for measurement error in the relative wages in the ENIGH: for our purposes, therefore, it is sufficient that the measurement (and sampling variation) in the two surveys are independent and that the movements in relative changes across groups are correlated between the two surveys. In the following subsection we report evidence in this sense.

In our analysis we divide the sample into year of birth cohorts and education groups. The division into year of birth allows following the evolution of the variables of interest over the life cycle of different groups of individuals. Dividing the sample by education groups allows us to focus on shocks to the relative price of skills.

3.2 Descriptive Analysis

We consider three education groups, defined on the basis of the education attainment of the household head: (i) primary complete or less, (ii) some secondary education, and (iii) some higher education.[10] The cohorts are formed on the basis of 5-year bands: the first cohort includes household headed by individuals born between 1925 and 1929. These individuals are 60-64 years of age in the 1989 survey. The second cohort includes household heads born between 1930 and 1934, who are 55-59 years of age in 1989, and so on. Table 1 reports the average (over the different surveys) sample size of each group we consider in the two data sets.

Table 1.

Average Cell Size by Cohort

Cohort's Year of Birth	Age in 1989	Average Cell Size								
		ENE Wage Earners by Education group			ENIGH Wage Earners by Education group			ENIGH Households by Education group		
		Low	Medium	High	Low	Medium	High	Low	Medium	High
1925-1929	60-64	1806	314	181	70	14	11	108	16	11
1930-1934	55-59	2412	529	296	110	28	19	155	33	20
1935-1939	50-54	2765	718	476	162	55	30	221	64	33
1940-1944	45-49	3209	1104	860	236	81	57	290	94	64
1945-1949	40-44	3465	1650	1386	257	143	96	316	162	101
1950-1954	35-39	3750	2386	2097	301	221	156	353	241	161
1955-1959	30-34	3888	3214	2783	310	288	233	323	286	229
1960-1964	25-29	3467	4636	3212	304	431	255	274	365	210

Source: Authors' calculations from ENIGH and ENE surveys.

Table 2.

Wages as a Fraction of Wages in 1994 for the 1940-1944 Cohort with No Schooling

Survey Year	**Year of Birth of Cohort** 1925-29	1930-34	1935-39	1940-44	1945-49	1950-54	1955-59	1960-64
				Low Schooling				
1992	0.71	1.24	1.05	1.01	1.25	0.94	0.84	0.82
1994	1.11	0.84	0.89	1.00	0.92	0.88	0.88	0.84
1996	0.58	0.71	0.72	0.73	0.63	0.66	0.59	0.53
1998	0.46	0.64	0.81	0.90	0.79	0.76	0.75	0.81
				Medium Schooling				
1992	2.51	1.94	2.18	2.40	1.61	1.43	1.20	0.95
1994	2.24	2.78	2.09	1.97	1.77	1.56	1.33	1.02
1996	1.38	1.23	1.17	1.05	1.23	1.03	0.87	0.73
1998	1.44	2.01	1.69	1.29	1.32	1.12	1.06	1.01
				High Schooling				
1992	6.98	2.90	6.19	4.52	5.01	3.96	2.69	2.50
1994	3.20	7.41	4.08	4.74	6.02	3.82	3.69	3.16
1996	2.45	2.29	3.28	3.71	3.58	2.15	2.69	1.77
1998	5.62	2.76	1.96	2.49	2.85	4.53	1.87	1.77

Source: Authors' calculations from ENIGH surveys.

[10] The Mexican schooling system is divided into three main cycles. The first, comprising the first 6 years of formal education, is labeled Primary schooling. The second cycle comprises 6 years (separated into two sub-cycles), is labeled Secondary. The third includes technical education or college. We have classified individuals attending any year of post-secondary schooling in the group of higher education.

We start our descriptive analysis by discussing how relative wages have changed across different cohorts and education groups. We focus on four of the years for which data are available: 1992, 1994, 1996 and 1998. 1996 is a particularly interesting year as it comes immediately after the 1995 peso devaluation and the subsequent deep recession that affected the Mexican economy.

Table 2 reports wage rates for different groups relative to the real wage in 1994 of individuals with low educational levels in the cohort born between 1940 and 1944. Three features are apparent from the table. The first, not surprising, is that for all years and all cohorts, wages and schooling levels are closely related. Individuals in the group with higher education have greater wages than those with secondary or primary education, and individuals with secondary education have much higher wages than those with primary education. The higher-primary wage differentials vary considerably across cohorts and over time, while the higher-secondary differential is quite stable. For the higher-primary differences the greatest wage differentials are found among the youngest and oldest cohorts, and there are also variations over time. The average wage differential increased steadily between 1994 and 1998, which is consistent with previous evidence of increasing returns to higher education in Mexico.[11]

Table 3.

Non-Durable Consumption as a Fraction of Wages in 1994 for the 1940-1944 Cohort with No Schooling

Survey	**Year of Birth of Cohort**							
Year	1925-29	1930-34	1935-39	1940-44	1945-49	1950-54	1955-59	1960-64
				Low Schooling				
1992	0.90	1.02	0.93	0.93	0.84	0.70	0.81	0.74
1994	1.20	0.99	1.08	1.00	0.78	0.80	0.74	0.76
1996	0.76	0.74	0.77	0.70	0.62	0.60	0.65	0.52
1998	0.80	0.80	0.78	0.76	0.64	0.65	0.63	0.64
				Medium Schooling				
1992	2.22	1.83	1.99	1.78	1.74	1.25	1.36	1.30
1994	2.53	3.00	2.76	2.18	1.54	1.30	1.41	1.34
1996	1.62	1.36	1.09	1.24	1.03	1.02	0.89	1.00
1998	1.31	1.44	1.76	1.13	0.91	1.15	0.94	0.87
				High Schooling				
1992	3.60	2.49	3.38	3.09	3.03	2.36	2.57	2.78
1994	4.10	4.50	3.34	3.28	3.77	2.92	2.54	2.92
1996	2.69	3.93	2.69	2.10	2.17	1.67	2.00	2.01
1998	3.87	2.68	2.27	2.22	1.91	1.64	1.53	1.53

Source: Authors' calculations from ENIGH surveys.

[11] The evolution of returns to education in Mexico is documented in Attanasio and Székely (2001).

The second feature is that for all cohorts and education groups there is a sharp decline in real wages between 1994 and 1996. This reflects the effects of the Peso crisis by the end of 1994, which continued well into 1996. Between 1994 and 1995 GDP per capita contracted by around 7 percent in real terms, and even though the Mexican economy started its recovery in 1996, GDP per capita was nonetheless 4.2 percent lower in real terms in 1996 than in 1994. There are, however, important differences in the way different cohorts and especially different education groups were affected by this shock. The most affected were individuals with medium education levels, while the least affected in percentage terms was the group with the lowest level of education.

The third feature is that there are interesting differences in wage patterns across education groups that are not explained by the 1995 Peso crisis. While for 1992-1994 no clear pattern emerges, there are clear differences for the period 1996-1998. For individuals with secondary education there is an increase in real wages between 1996 and 1998 for all cohorts, but this is not the case for the low schooling and the highly educated group. For those with low schooling there are wage contractions among the oldest two cohorts, and an expansion that grows the younger the cohort. For the most educated there is a decline in real wages for cohorts born between 1935 and 1949 (with the only exception of the 1950-54 cohort), and increases for the oldest two cohorts.

Table 3 reports analogous figures for non-durable consumption. The first two features of Table 2 are also apparent in Table 3. The main difference is found in consumption patterns between 1992 and 1994 and between 1996 and 1998. During 1992-1994 GDP per capita increased by about 2 percent in real terms, and this is reflected in consumption increases for practically all cohorts among households whose head had either secondary or higher education. For households whose head had only primary schooling consumption declined in three cohorts and increased much less than in the other education groups in the rest.

During the 1996-1998 period, where GDP per capita increased by around 4 percent in real terms, consumption of non-durable goods increased only modestly. Interestingly, the group with the lowest education is the only one to register an increase across cohorts (the only exception is the cohort born between 1955 and 1959). In the group of households whose head has secondary schooling, in half of the cohorts there is a rise in consumption, and a decline in the other half. For households with the highest education there are declines in consumption in all but two cohorts. While the results for the groups of higher and primary education are in line with the wage contractions and expansions, respectively, as reported in Table 2, the decline in consumption for the middle group are more surprising, since the wage expansion registered by all cohorts is not accompanied by consumption increases.

We summarize the evidence in Tables 2 and 3 with two graphs reported in Figure 1 and 2 (the numbers inside the figures correspond to the education group of each cohort). To construct Figure 1 we regressed average log consumption and wages on time and group dummies (where a group is defined by cohort and education level) and plot the residuals of the two regressions against each other. We therefore focus on relative consumption and wage levels after removing time and group effects. To construct Figure 2, we regress *changes* in log consumption and log wages on year dummies and plot the residuals of the two regressions against each other. In each of the two figures we also plot the regression line. We therefore identify the changes in relative wages and consumption after removing year effects. Both figures present a marked positive relationship between the consumption and wage residuals. In both cases, the slope is positive and statistically different from zero. In the absence of measurement error and other factors these simple figures would represent the result of the first set of tests discussed in Section 2.[12] Interpreted in this way, therefore, the figures represent a first piece of evidence against the null of full insurance. More generally, one can interpret the two pictures as reflecting the extent to which changes in relative male wages (across cohort and education groups) are reflected in changes in consumption.

While the general message conveyed by the picture—that there is a positive relationship between relative wage and consumption changes—similar to that given by the evidence in AD, there are three important differences. First, in the US, over the period studied by AD, highly educated individuals report relative year-on-year gains in both wages and consumption (that is, they would be in the upper right part of Figure 2). Here, instead, households with more education can be found also in the southwest part of the graph. This is consistent with the observation we made in Tables 2 and 3 that in 1996 some highly educated cohorts fared worse than average. Second, at relatively high frequencies AD find no relationship between consumption and wage changes. While we cannot compute one-year changes because of the lack of data, even at two-

[12] Notice that the presence of measurement error attenuates the slope of the estimated lines.

year intervals (which would correspond to our changes, as we have roughly bi-annual data), AD do not find much of a relationship. Third, AD find that the size of the coefficient increases monotonically with the level of the differencing and is highest for the level specification. We find that the slope in Figures 1 and 2 is roughly the same and is actually lower for the levels than for the changes.

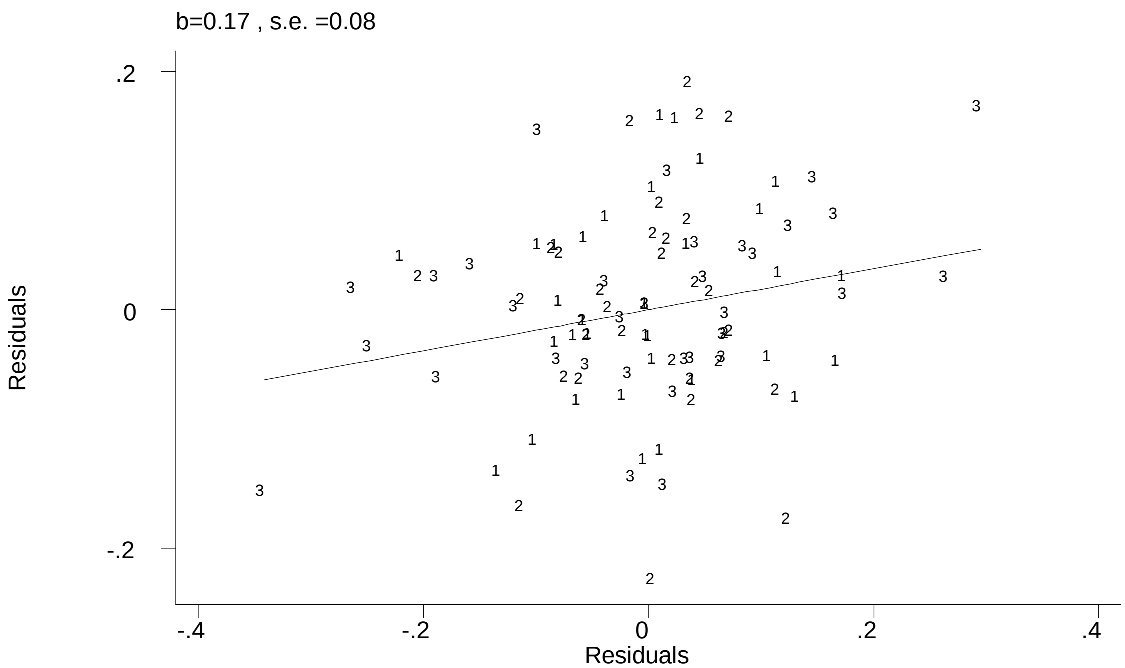

Figure 1. Relative Consumption and Wages

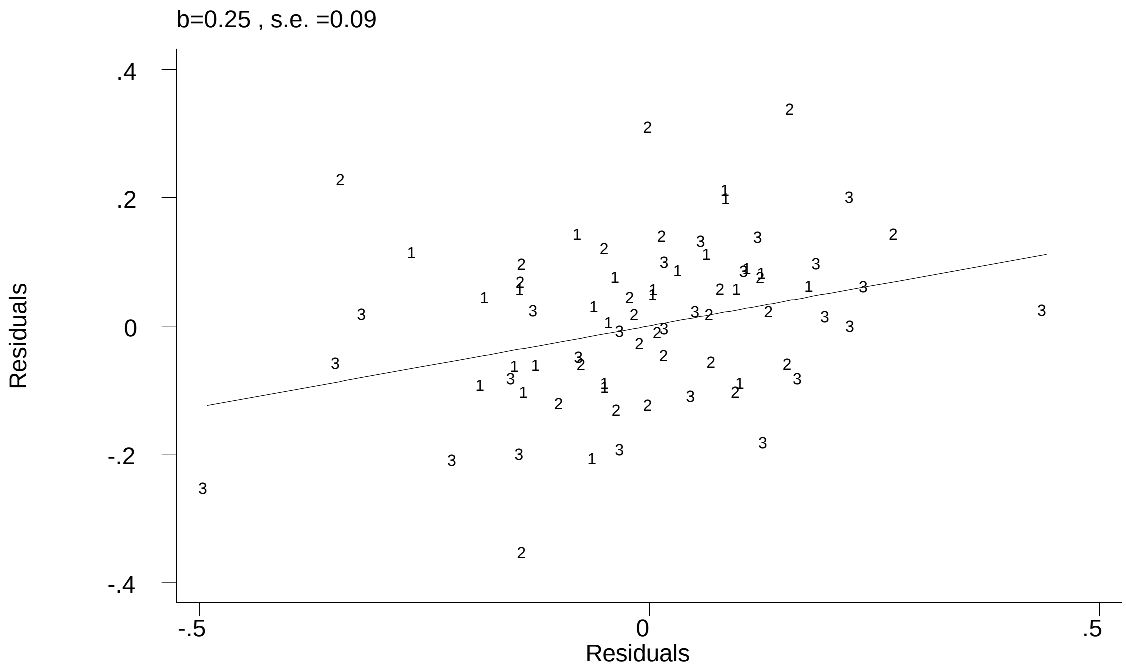

Figure 2. Relative Changes in Consumption and Wages

As mentioned above, the presence of measurement error would bias the coefficient on wages downwards. Moreover, it is possible that family size (and possibly other variables) affect the marginal utility of consumption in ways more complex than our simple adult equivalent scale would allow. For these reasons, in what follows we perform a more formal analysis. Before doing so, however, we discuss whether we can use our alternative measure of wages as an instrument for the wages observed in our primary data source.

3.3 How Good are ENEU Wages as Instrument for ENIGH Wages?

In this section we study to what extent the measure of wages we get from the ENEU survey can be used as instrument for the wages in ENIGH. As the two samples are independent, the measurement error and the sample error are independent and therefore our variables satisfy the first criterion we need for a good instrument. The movement in relative wages measured in the two surveys, however, should also be similar.

We start by regressing, for the cells with a median age of the household head less than 60, the average log wage in the ENIGH on the average log wage in the ENEU. We obtain a coefficient of 1.1 with a t-value of 48 and an R-squared of 0.96. This indicates that the two measures of wages are strongly correlated across groups and over time. To be able to use one as an instrument for the other in our tests, however, we have to check whether *relative* movements in wages are correlated. For this purpose, we proceed to add to the simple level regression time and group dummies. The significance of the ENEU wage in this regression is the appropriate requirement for this to be a good instrument in our level equation where time and group dummies enter (see equation (7)). Once again the results are satisfactory: the coefficient on the ENEU log wage is now 0.93 with a standard error of 0.24. We therefore conclude that, even after controlling for year and group effects, the ENEU and ENIGH wage measures are strongly correlated. As the two samples are independent, we can reliably use one measure of the wage as an instrument for the other in our level equation.

We then move to the consideration of the changes of average log wages. A simple regression on the same sample gives us a coefficient of 1.14 with a standard error of 0.08 and an R-squared of 0.78. When we add time dummies to this simple regression, the coefficient on the ENEU wage is 1.07 with a standard error of 0.3. Therefore we conclude that both for the level and the difference specification the ENEU wage is an appropriate instrument for the ENIGH wage (and vice versa).

As discussed above, in addition to the tests based on group means, we also perform tests based on group variances. Following a similar line of reasoning, we want to check whether the within-group variances (and their changes) are correlated across groups and over time in the two databases we use. We start with the level of the variance in wages: when we regress the variance of male log wages in the ENIGH on that measured in the ENEU we obtain a coefficient of 1.24 with a standard error of 0.14 and an R-squared of 0.47. If we add group dummies to this equation, the coefficient on the variance of ENEU wages is 1.12 with a standard error of 0.30. Therefore we conclude that also for the within group variance, the ENEU seems to mimic the behavior of the ENIGH data set. Finally, when we look at changes in the variance we obtain a coefficient of 0.64 with a standard error of 0.4 and an R-squared of only 0.038. Therefore, for changes in the standard deviation of log male wages, the ENEU does not seem to be a very informative instrument for the corresponding measure in the ENIGH. We will have to keep this in mind when we interpret the results below.

4. Results for Non-Durable Consumption

In this section we report the results we obtain using our time series of cross sections from Mexico. In the first subsection, we report the results on the test based on means, while in the second, we report the results obtained on the test based on cross-sectional variances.

4.1 Results on Means

We report two sets of results: the first is based on running the regressions in levels. These correspond to estimating equation (6). The second set of results is based on results in differences and corresponds to the estimation of equation (5'). Notice that, while equation (5') is written in terms of first differences, this is not necessary. Indeed it is not even necessary that the distance between subsequent periods be the same: considering the log of the equation at two arbitrary dates allows us to eliminate by differencing the unobserved Pareto weights. As the residuals are originated by measurement error, there is no reason why our test should be invalid when we consider years that are not equidistant. This is very convenient, as our surveys are not equally spaced.

As a measure of consumption we use non-durable consumption per adult equivalent. The right-hand side variables, in addition to group and year dummies, include the log of family size, to allow for the possibility that our adult equivalent scheme does not capture completely changes in the needs of the households during their life cycle. We have also tried to include a polynomial in age, but the results are basically unaffected.

In the first column of Table 4, we report the estimates obtained by OLS. In the second we use the ENIGH average log wage as a regressor and the ENEU average log wage as an instrument. In the third column, instead, we use the same instrumenting technique as AD, which is appropriate in the absence of an alternative data source. Finally, in column 4 we use the ENEU wage as a regressor and the ENIGH wage as an instrument. In all regressions, we take into account heteroskedasticity by using cell sizes as analytic weights.

Table 4.

Regressions in Levels
Dependent Variable: Log non-durable consumption per adult equivalent

Independent Variable	Regression Results (1)	(2)	(3)	(4)
Log Wage	0.372	0.196	0.139	0.181
	(0.074)	(0.145)	(0.271)	(0.120)
Log Family Size	-0.835	-0.756	-0.731	-0.750
	(0.085)	(0.104)	(0.147)	(0.98)
R-squared	0.99	0.99	0.98	0.98
Number of Observations	99	99	91	91
Wage Variable	*ENIGH Wage*	*ENIGH Wage*	*ENIGH Wage*	*ENEU Wage*
Instrument for Wage Variable	*n.a.*	*ENEU Wage*	*Moving average of lags and leads of ENIGH*	*ENIGH Wage*

Source: Authors' calculations.
Note: All specifications include year and group dummies. Cell size used as analytic weight.

The results we obtain are a bit surprising. First, instrumenting does not increase the size of the OLS estimates. Second, even though the OLS estimates in the first column indicate a rejection of the null, the standard errors (reported under the coefficient estimates) we obtain when using any of the instrumenting strategies in columns 2, 3 and 4, increase substantially, so that the null is not rejected. Moreover, the coefficient on the wages goes down substantially in column 2, relative to the OLS estimates in column 1.[13] These results are quite different from those obtained by AD for the US in their level specification. In particular, they obtain, after instrumenting, a coefficient close to 1. In all specifications, the log of family size plays an important role, indicating the relevance of controlling for demographic effects, over and above those incorporated in the adult equivalence scales. However, exclusion of such a variable from the equation does not change substantially the results obtained on the wage coefficient.

[13] If we use the ENEU measure of average log wage and estimate the equation by OLS, we get a coefficient of 0.166 with a standard error of 0.137.

Table 5.

Regressions in First Difference
Dependent Variable: Change in Log non-durable consumption per adult equivalent

Independent Variable	Regression Results (1)	(2)	(3)	(4)
Change in Log Wage	0.372	0.464	0.362	1.674
	(0.087)	(0.147)	(0.255)	(0.537)
Change in Log Family Size	-0.8	-0.838	-0.856	-1.158
	(0.094)	(0.109)	(0.144)	(0.192)
R-squared	0.85	0.99	0.83	0.71
Number of Observations	75	75	75	75
Wage Variable	*ENIGH Wage*	*ENIGH Wage*	*ENEU*	*ENEU Wage*
Instrument for Wage Variable	*n.a.*	*ENEU Wage*	*n.a.*	*ENIGH Wage*

Source: Authors' calculations.
Note: All specifications include year and group dummies. Cell size used as analytic weight.

In Table 5, we report our results from estimating equation (5'). The Table is made of 4 columns. The first reports OLS estimates obtained using (changes in) the ENIGH measure of log wages as a regressor. The second column reports IV estimates where the ENEU measure is used as an instrument for ENIGH. In column 3 and 4 we use ENEU log wages as a regressor. Column 3 estimates the relation by OLS, while in column 4 the ENIGH log wage is used as instrument. The standard errors of this table are corrected for the presence of MA(1) residuals induced by firs-differencing measurement error.

Unlike the results in Table 4, the coefficients reported in Table 5 indicate strong rejections of the null hypothesis and, overall, results that are much more similar to those obtained by AD for the United States. Notice, in particular, that the point estimates increase when instrumenting and that the wage coefficient is statistically different from zero in all specifications. For the case in which we use the ENIGH wage as a regressor, and the ENEU wage as an instrument, we obtain a coefficient of 0.46 while if we use the ENEU wage as a regressor, the point estimate of the coefficient increases to 1.6. Overall, these results indicate some rejections of the null hypothesis that changes in relative male wages are not reflected in changes in relative consumption levels.

So, the two main differences between our results and those for the United States in AD are, first, that there are no substantial rejections of the null in the level specification, where AD get their strongest rejections. Second, the point estimates we obtain in the difference specification are a bit larger than those obtained by AD. Notice that here the comparison is a bit difficult because we do not have yearly surveys. The closest comparison that can be made is with the bi-annual differences in AD.

The differences in results can be explained by many factors. We mention three. First, it might be that the level of risk sharing and the mechanisms through which it operates are very different in Mexico and the United States (which is the country analyzed in AD). Second, it is possible that the nature of the shocks experienced by the two countries over the relevant period is very different: after all, the null tests the smoothing of a particular set of shocks over a particular time period. Third, it is possible that the model for Mexico is somewhat mis-specified. However, the limited number of cross sections available (relative to the United States) makes it difficult to check different specifications.

4.2 Results on Variances

As discussed above, the tests based on the level and changes of the cross sectional variance of log consumption are complementary to those obtained using the means. Unfortunately, the results we obtain are not extremely informative. They are reported in Tables 6 and 7 and can be summarized as follows.

In Table 6, the OLS results indicate marginal rejections of the null. Surprisingly enough, however, the sign on the variance of male log wages differs when one uses the ENIGH or the ENEU wage. This is particularly surprising given the positive correlation (even after controlling for group effects) between the two measures. When one uses instrumental variables, the signs flip again, being positive and marginally significant for the specification using ENIGH data to instrument the ENEU and negative and significant when ENIGH data are used as a regressor.

Table 6.

Regressions in Levels
Dependent Variable: Cross-Sectional Variance of Consumption

Independent Variable	Regression Results (1) OLS	(2) IV	(3) OLS	(4) IV
Variance of Log Wages	0.237	-0.516	-0.444	2.037
	(0.115)	(0.348)	(0.188)	(1.094)
Variance Log Family Size	-0.115	0.33	0.98	-9.470
	(0.156)	(0.263)	(0.181)	(0.550)
R-squared	0.73	0.24	0.7	n.a.
Number of Observations	99	99	99	99
Wage Variable	*ENIGH Wage*	*ENIGH Wage*	*ENEU Wage*	*ENEU Wage*
Instrument for Wage Variable	*n.a.*	*ENEU Wage*	*n.a.*	*ENIGH Wage*

Source: Authors' calculations.
Note: All specifications include year and group dummies. Cell size used as analytic weight.

Table 7.

Regressions in Changes
Dependent Variable: Change in Cross-Sectional Variance of Consumption

Independent Variable	Regression Results (1) OLS	(2) IV	(3) OLS	(4) IV
Chg. Variance of Log Wages	0.315	-0.471	-0.252	6.853
	(0.064)	(0.829)	(0.251)	(5.672)
Chg. Variance Log Family Size	0.369	0.239	0.33	-0.049
	(0.144)	(0.308)	(0.165)	(0.763)
R-squared	0.29	n.a.	0.062	n.a.
Number of Observations	75	75	75	75
Wage Variable	*ENIGH Wage*	*ENIGH Wage*	*ENEU Wage*	*ENEU Wage*
Instrument for Wage Variable	*n.a.*	*ENEU Wage*	*n.a.*	*ENIGH Wage*

Source: Authors' calculations.
Note: All specifications include year and group dummies. Cell size used as analytic weight.

As for the specification in differences, we have a similar pattern for the sign of the coefficients, but only the results in column (1) are significantly different from zero. These results might indicate, more than a non-rejection of the null, the poor performance of the instruments.

5. Results for Other Expenditure Items

In this section we consider the relationship between relative changes in wages and relative changes in the expenditure on durable goods, health and education. As mentioned in Section 2, these cannot be given the structural interpretation given to analogous results for non-durable consumption. However, they are informative about which components of expenditure (if any) react to changes in wages.

Tables 8 and 9 present tests analogous to those in Tables 4 and 5, but instead of using the group mean of non-durable good consumption as a dependent variable we use the group mean of: (1) log of expenditure on durable goods, (2) log of health expenditures, and (3) log of education expenditures, respectively.[14] Expenditures on durable goods and health are per adult equivalent, while expenditures on education are per school age child (children 6 to 21 years of age). For each variable we present only IV regressions.

Table 8 refers to the results in levels. In the first set of regression (regressions 1a, 2a and 3a) the ENIGH log wages are instrumented with the ENEU data; the second (1b, 2b and 3b) use ENEU log wages instrumented by ENIGH log wages; the third (1c, 2c, and 3c) use the ENIGH log wage with the AD instrument. Similarly to our results in Section 4, all regressions take into account heteroskedasticity by using cell sizes as analytical weights.

The first feature of the table is that very different results are obtained depending on the definition of the instrument. In particular, it appears that the results obtained with the AD instrument are extremely imprecise and variable. It seems that such an instrument is not very informative. We will therefore focus our discussion on the results obtained with the other two instrumenting strategies, that is, using the ENEU wage measure as an instrument for the ENIGH and vice-versa.

[14] The specific items in education expenditures are education fees, books, materials, uniforms and equipment. Health expenditures include fees for all medical consults and services, medicine, hospital fees, medical tests, medical equipment, and health insurance. Durable goods consumption is constructed by deducting non-durables from total household consumption.

While the point estimates we obtain are not extremely precise, for the three categories we consider, the coefficient on log wages is estimated to be considerably higher than those obtained with non-durable consumption, indicating a greater sensitivity of these items to wage shocks. As far as comparing among these three items, the estimates are too imprecise to allow us any conclusion.

The results in first differences, reported in Table 9, are remarkably similar to those in Table 8. Table 9 presents IV regressions analogous to those in column (2) and (4) in Table 5 (regressions 1a and 2a use ENEU log wages to instrument ENIGH data, while (1b and 2b use the ENIGH to instrument ENEU log wage). As in Table 8, the relative size of the estimated coefficients changes with the instrument used and the point estimates are not very precise. However, for all expenditure categories considered in this table the coefficients on wages seem to be larger than when the measure of non-durable consumption is used as dependent variable

Table 8.

Regressions in Levels
All Regressions Estimated with Instrumental Variables

Independent Variable	Consumption Item Used As Dependent Variable								
	Durable Goods			Health Expenditures			Education Expenditures		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
Log Wages	0.757	1.579	-0.758	0.705	1.038	0.604	0.402	1.525	-0.397
	0.582	*0.962*	*1.174*	*0.436*	*0.692*	*0.783*	*0.424*	*0.749*	*0.851*
Log Family Size	-1.382	-1.680	-0.704	-1.660	-1.762	-1.615	-0.914	-1.348	-0.557
	0.416	*0.511*	*0.638*	*0.312*	*0.368*	*0.425*	*0.303*	*0.399*	*0.463*
R-squared	0.89	0.88	0.86	0.92	0.92	0.92	0.96	0.95	0.95
Number of Observations	99	99	99	99	99	99	99	99	99
Wage Variable	*ENIGH*	*ENEU*	*ENIGH*	*ENIGH*	*ENEU*	*ENIGH*	*ENIGH*	*ENEU*	*ENEU*
Instrument for Wage Variable	*ENEU*	*ENIGH*	*AD*	*ENEU*	*ENIGH*	*AD*	*ENEU*	*ENIGH*	*AD*

Source: Authors' calculations.
Note: All specifications include year and group dummies. Cell size used as analytic weight.

One interpretation of this evidence is that expenditure on durables and human capital investment by the household reacts more to wage shocks than non-durable consumption. So, even temporary shocks may have long-run implications through inhibiting the accumulation of income-earning assets. This strategy may make households even more vulnerable to shocks in the long run. A word of caution is necessary, however, given the low precision of our point estimates.

Table 9.

Regressions in Difference
All Regressions Estimated with Instrumental Variables

Independent Variable	Change in Consumption Item Used As Dependent Variable					
	Durable Goods		Health Expenditures		Education Expenditures	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Log Wages	0.921	1.137	0.847	1.785	0.495	1.658
	0.700	*1.751*	*0.480*	*1.209*	*0.473*	*1.143*
Log Family Size	-1.038	-1.130	-1.326	-1.594	-0.937	-1.247
	0.790	*0.625*	*0.353*	*0.456*	*0.398*	*0.515*
R-squared	0.31	0.36	0.46	0.49	0.56	0.53
Number of Observations	75	75	75	75	75	75
Wage Variable	*ENIGH*	*ENEU*	*ENIGH*	*ENEU*	*ENIGH*	*ENEU*
Instrument for Wage Variable	*ENEU*	*ENIGH*	*ENEU*	*ENIGH*	*ENEU*	*ENIGH*

Source: Authors' calculations.
Note: All specifications include year and group dummies. Cell size used as analytic weight.

6. Conclusions

This paper uses synthetic panel techniques to explore the relationship between shocks to relative male wages and household consumption in Mexico during the 1990s. This is a particularly interesting case, since during the decade the country experienced a large negative shock (the 1995 peso crisis) along with periods of strong growth.

Our analysis is the first, to the best of our knowledge, of the effects of relative shocks to wages on consumption in Mexico. We document the extent of the changes in relative wages over the sample period and present evidence on several components of consumption expenditures, namely expenditure on non-durables, durables, health and education.

The evidence on non-durable goods consumption can be interpreted as a test of the hypothesis that Mexican households are able to insure idiosyncratic risk. We consider both tests based on means and variances, which, as we discuss, are complementary, as the former could be interpreted as testing the hypothesis of risk sharing across groups, and the latter as testing risk sharing within groups. Our results on means reject the hypothesis that Mexican households are able to insure idiosyncratic risk, while those for the variances are more mixed. The tests of the variances, instead, do not yield significant rejections of the null, partly due to the low precision of the estimates.

Our results on the other components of expenditure indicate that the effect on these components to changes in relative wages is much stronger than the effect on non-durable consumption. We have speculated that this result might indicate that high frequency shocks might be smoothed by reducing the investment in human capital investment (and durables) and therefore, might have long-run consequences.

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